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THE
BRITISH JOURNAL
OF
PSYCHOLOGY

GENERAL SECTION

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THE BRITISH
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GENERAL SECTION

MIND AND MEDIUM IN ART[1]. (I.)

BY CHARLES MARRIOTT.

I.

THE following notes are prompted by the belief that in all aesthetic discussion, and psychological experiment in connexion with the subject of aesthetics, too little attention is paid to the question of the direct action on the mind of the medium itself. By 'medium' is understood the tools and materials employed in the particular art concerned.

A few simple illustrations may make this clearer. Let us take a symbol with which everybody is familiar, particularly since its mournful application in the late war: the cross. As carried out in wood, that is to say in the form of two crossed sticks or planks, apart from any religious meaning that it conveys most people find the symbol aesthetically satisfactory if the proportions are reasonably good. But, carried out in stone, in the same form and proportions, the symbol becomes unsatisfactory. It seems to need modification. In practice the modification is generally made. The most popular forms of the cross in stone are either the Celtic, or 'wheel,' cross, or else the tall form, common in village crosses, in which the shaft is greatly elongated and generally tapering from the base, and the arms are reduced to comparative insignificance.

The reason seems to me quite simple. In the wooden form, whether the two sticks or planks are mortised or merely nailed together, the grain of the wood, though it may not be perceptible to the eye, is felt to run horizontally across the arms. The wooden cross explains itself as a structure. Repeated in stone the same form does not explain itself. Since it is inconceivable that the grain of the stone can run vertically in the shaft and horizontally in the arms, the impression given is that the cross has been punched out of the stone in an arbitrary manner in disregard of its nature and substance. Not only that, but the impression conveyed is that of actual weakness at the crossing of the shaft and arms. In addition there is the sense of difficulty in workmanship; that it must have cost an amount of labour disproportionate to the result achieved to cut the stone to the lateral depth necessary for the projection of the arms to that extent. As I said, in practice the feeling of discrepancy with regard to the grain, the effect of weakness at the crossing, and the sense of disproportionate labour are all generally avoided by certain modifications. In the Celtic cross an amount of stone is left, in the shape of the 'wheel,' which not only strengthens the structure but, so to speak, contradicts the method of construction employed in wood, and actually lessens the labour; and in the tall cross the same results are obtained by evasion; by making much of the shaft and little of the arms. When and why these modifications were adopted in translating the wooden cross into stone it is impossible to say; but I am persuaded that they were prompted by aesthetic instinct having regard to the nature of stone. If it be objected that the reason was practical, my answer is that practical and aesthetic reasons are at bottom the same thing; or, to put it another way, that aesthetic appreciation is dependent upon the sense, which may not be conscious, of practical problems effectively solved. However, for the moment, the important point is that here we have a simple form and proportion which is satisfactory in one material and unsatisfactory in another.

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[1] A contribution to the Symposium presented at the Congress of Philosophy in Oxford, 24–27 September, 1920.

Now let us take an illustration from architecture. It would be quite possible, I suppose, to build a Parthenon of the same scale and proportion in brick; but I do not think that the result would be satisfactory. Whether or not Doric architecture originated in the imitation of wooden structures, it is obviously a style adapted to construction with stones which share with timber the character of length. Whatever it is or is not, classical architecture is essentially a style of building with long stones. For satisfactory translation into brick it involves the modifications implied in Renaissance, represented in England by Tudor and Queen Anne. Here again we have a form which, for aesthetic reasons based upon practical convenience, is satisfactory in one material and unsatisfactory in another.

Or, again, we might take an illustration from the printed page, and one which carries the argument a little further because it transfers the attention from the materials used to the act of using them. Nobody can look at an illuminated manuscript without feeling that the character employed is well adapted to the act of writing. You can see the formation of the letters with the pen or brush. With the introduction of wood-engraving, which is the link between writing and printing, this fitness of the character becomes less evident. You can still follow in sympathy the movements of the engraver's hand in cutting the type, but the form of the character seems too elaborate for the method. With the introduction of printing from movable type, or typography proper, the unfitness of the character for casting positively jumps at you; and, in fact, with the advance of printing the Gothic character was generally abandoned in favour of Roman, which was probably an engraved or incised character before it was a written one.

Thus, in the transition from writing to printing, the form of the character was modified and even changed out of regard for the materials and the convenience of the hand in using them. The same symbols were employed, and with exactly the same meaning; but they were adapted to the changed methods in book production. Not only that, but our aesthetic pleasure in the written or printed page depends a good deal on this fact. Minor variations in form and irregularities in arrangement which add beauty and interest to the written page, because they suggest the movement and convenience of the hand, have no meaning in the printed page. Instead of an aesthetic merit they become an aesthetic defect; though the purpose of the text, to convey meaning, remains exactly the same. Apart from the strain it puts upon the eyes, the Gothic type of a German newspaper offends the aesthetic sense because it looks ill-adapted to the methods of printing as it was not to formation with the pen or brush.

The above illustrations have the advantage of not being complicated with any question of likeness to nature. They show, or seem to show, that even in the more abstract conditions of art we cannot consider form apart from the means by which it is produced. When we turn to the arts of representation we find exactly the same thing. For our full aesthetic satisfaction, any form of nature must be adapted to the material in which it is represented and to the convenience of the hand in using that material. Thus, we expect a broader treatment of landscape in water-colour than in oils because the, so to speak, 'natural' play of water-colour is in broad washes. Or, to take extreme illustrations, stained-glass, mosaic, tapestry and embroidery obviously do not lend themselves with ease to such close imitation of nature as either oil or water-colours. The same natural forms may be represented in all these materials; but in each case there are modifications determined by the convenient use of the material concerned. In a note to his Lectures on Architecture and Painting delivered at Edinburgh, November, 1853, Ruskin put this commonplace of art, as it might be called, into the following words: "In every branch of art, only so much imitation of nature is to be admitted as is consistent with the ease of the workman and the capacities of the material."

II.

The question, from a psychological point of view, is how far our enjoyment of a work of art is dependent upon this adaptation of means to end; whether, in fact, Ruskin's axiom is an arbitrary aesthetic rule or a commonplace of human psychology. Personally, as I have tried to show, I believe that the dependence is much greater and much closer than is commonly supposed, and that in itself it affords the most promising guide to both artistic education and psychological experiment.

The objection that we can enjoy works of art without knowing how they are done, and that in moments of highest aesthetic enjoyment we become unconscious of the means, is only apparent. Directly the means are used out of character we *do* become conscious of them, whether we understand their characteristic use or not; as is implied in the old saying *ars est celare artem*.

Nor does unconsciousness of the means seem to depend on likeness to nature in the sense of imitation—as the old saying is commonly interpreted. A very wide departure from nature is accepted by the simplest intelligence so long as the means are used in character. Leaving out such crude examples as the child's wooden horse or Dutch doll, the outline drawing is a case in point. It does not really resemble what it represents, but it presents no difficulty to the mind because it explains itself by the means adopted: the pointed pencil. The same is true of every form of art in which the means are obvious; such as stained-glass or mosaic. Paradoxical as it may sound, the means are ignored in proportion as they are obvious. It is when the means are of a kind to allow the artist to "play the game of art closely up to the real aspects of nature," as in painting, that difficulties begin and the true meaning of *ars est celare artem* is confused with the question of likeness to nature in the sense of imitation. The same person who will swallow an Egyptian papyrus will jib at an Impressionist painting. He says it looks 'painty.' The reason seems to me quite simple. In the papyrus the means though perfectly obvious are ignored because they are used in character; in the Impressionist painting they are obtruded because, to a certain extent, they are used out of character. To a certain extent they are used to create optical illusion; they do not seem entirely "consistent with the ease of the workman and the capacities of the material."

This habit of the human mind of straining at gnats and swallowing camels in the use of materials seems to me to throw light upon the very nature of art and of artistic appreciation. Broadly speaking there are two views of art as commonly held: that it is primarily the representation of nature, and that it is primarily the direct action of abstract form, colour and proportion or, in general terms, of design. Both views assume the transference of emotion as the aim of art, and each allows for the other as an aid to that purpose; but neither takes much account of the direct action of the medium in the process.

The suggestion that I would put forward is that art is primarily the characteristic use of materials, and that the representation of nature and the direct action of design are both incidental to that. They may or may not be present in the particular art concerned. So far from denying either, this view provides the connecting link or the reconciling factor between them. In this view the transference of emotion is looked upon as a consequence rather than an aim, and aesthetic appreciation as mainly a sympathetic response, not necessarily conscious, to the characteristic use of tools and materials.

That this view ignores any distinction between art and craft may be cheerfully admitted. The distinction may exist, but most of the troubles of art, including the separation of art from life, to the disadvantage of both, are due to the various attempts to fix it arbitrarily. For our purpose it is enough to say that craft is a necessary condition of art, and that the nature of art and of aesthetic appreciation can be studied profitably only in that condition. I have the tantalising conviction that the whole aesthetic problem is stated in the Athanasian Creed—if we could only work it out! At any rate we can say that art is neither made nor begotten but proceeding from craftsmanship in relation to the subject or idea. Or, to put it another way, the relation between art and craft may be not unlike that suggested by William James, in *Human Immortality*, as existing between thought and the brain. That is to say, that the function of craft is not creative but transmissive or, if you like to put it that way, limiting. This would account for the fact that there are plenty of people with a high degree of aesthetic sensibility, and even 'creative ideas,' but no sign of artistic talent. They lack the limiting capacity of craftsmanship which is the necessary condition of art. It would also account for the fact that though art is universal its limitation through craftsmanship results in the expression of what we call personality.

III.

Let us put it this way: all art is the record of human gestures in the presence of the subject or idea and as conditioned by the nature of the medium in which they are made. This would include even literature, which may be regarded as a gesture of the mind in the medium of words. It is notorious that we can enjoy hearing poetry in a language that we do not understand; indeed, I am not sure that *pure* aesthetic appreciation of poetry does not exclude the dictionary meaning of words—as pure aesthetic enjoyment of painting excludes the literal meaning of the subject. Aesthetic appreciation is the recognition, which may be instinctive and may be informed, of the propriety or 'naturalness' of such gestures having regard to the nature of the subject or idea and of the tools and materials with and in which they are made. With a responsive material, such as oil paint, these gestures may be as closely imitative of the subject as is consistent with freedom of brush-work—Ruskin's "ease of the workman"; but with a refractory material, such as granite, they must be far from it in order to seem natural.

In his book on *Art* (Chatto and Windus, 1914) Mr Clive Bell has aptly defined the quality that moves us common to all works of visual art as "significant form." But significant of what? Of human gestures in the medium concerned. As I tried to show in the illustrations of the cross, the Doric building, and the characters used in writing and printing, a form which may be significant in one material ceases to be so in another when the human gestures implied are obviously inconvenient. Oddly enough, the pictures in Mr Bell's book are all striking illustrations of the way "significant form" is conditioned by "the ease of the workman and the capacities of the material." Each represents a different medium, and in each of them the gestures implied are expressive not only of the subject but of the tools and materials employed.

Incidentally, this conception of "significant form" would explain the difference between the emotions we receive from nature directly and from works of art and, also, the occasional difficulty in distinguishing between the two. If we consider the scenes or aspects of nature which excite emotions which may be called aesthetic, we shall find, I think, that they are generally such as recall human gestures or, rather, the ampler gestures of a Creator working in the materials at His command. This might suggest the ultimate meaning of art. At any rate, there is some very sound aesthetics in 'Omnia Opera.'

The validity of the view put forward in this paper could only be tested by extensive and careful experiment with a number of subjects. I merely suggest the direction in which they should be made; that is to say, with abstract forms, may be, but always in relation to human and not mechanical gesture in some definite material. In a paper intended for discussion assertion is perhaps better than argument; and I shall content myself, now, with suggesting some of the consequences and, as I think, advantages of the view put forward.

1. It abolishes all the distinctions between one art and another on the grounds of the representation or the non-representation of nature.

2. It abolishes all the artificial and uncertain distinctions between various forms of the same art; such as decorative or pictorial painting, realistic or romantic poetry or drama.

3. At the same time it makes the sharpest possible distinction between one art and another. Whatever painting is or is not it involves the use of paint; whatever sculpture is or is not it involves the use of some substance that can be carved or moulded; whatever architecture is or is not it involves the use of building materials; whatever music is or is not it involves the use of musical sounds and of instruments to produce them; and whatever literature is or is not it involves the use of words. Only on the basis of the tools and materials used in them can confusion between the arts be avoided. On the other hand, it is when the arts are divided on a craft basis that they are most harmoniously combined—as we see in the Greek temple or the Gothic cathedral.

4. Without attempting to define the purpose of art it will include every purpose for which any art could possibly be practised; from the mere exercise of the muscles to the illustration of a theme or the interpretation of an idea.

5. While allowing for all the infinite degrees of art it brings all practitioners of the same art into the same category; the house-painter with Michelangelo, the letter-writer with Shakespeare. The differences between elementary and advanced, good, bad and indifferent art remain as before, though with a much firmer guide to their establishment, but at no point in the practice of any particular art can you say: "This is art" or "This is not art." Thus, if a man paints a door, I do not see how flatness can be regarded as other than an aesthetic consideration; though, of course, an extremely simple one.

6. It brings art into close relations with all other forms of human activity in work and play.

From the human point of view the last two advantages are by far the most important. So long as art is regarded as primarily the representation of nature, or as primarily the direct action of design, it is bound to be regarded as a special subject. The difficulty is to get the ordinary person to carry into aesthetic regions the perception and judgment that he displays in the other departments of life. He will persist in thinking of art as a mystery complicated by arbitrary rules of taste; and the consequence is that he chooses his clothes and pursues his hobbies on sound aesthetic principles, so far as they go, and takes his art and literature on trust. It is a fact that the pictures in an average home are as a rule less good artistically than the carpets and curtains, and the domestic reading is less good literature than the letters and conversation. I am not proposing anything new—indeed it is the oldest view of art—but only that the subject of art and literature should be approached from a fresh angle. Once accustom a man to the idea that art is primarily the characteristic use of tools and materials in view of the particular purpose, and you have given him a clue to the highest aesthetic appreciation. Whatever art may be, essentially, in the last analysis all aesthetic virtues are virtues of necessity. The laws of painting are as surely derived from the nature of paint and brushes as the laws of cricket are derived from the nature of bat and ball. Any man who has handled a tool in a hobby or an implement in a game is on the way to understanding art in its highest developments. The purpose may be different, but the principles are the same. The advantages of this approach to the subject do not, however, end with aesthetic appreciation. They reflect backwards upon work and play. The first-hand knowledge and judgment derived from work, of whatever kind, are carried into the aesthetic region, and the refinements of art are instinctively applied to work; so that sweeping a floor becomes an artistic exercise.

All art is, finally, nature, but nature in the larger sense defined by Reynolds in his *Seventh Discourse.* "My notion of nature comprehends not only the forms which nature produces, but also the nature and internal fabric and organisation, as I may call it, of the human mind and imagination." If to these words we add "and the nature of the tools and materials used by man," it seems to me that we have a conception of art that will cover all its practices and, at any rate potentially, explain all its phenomena.

MIND AND MEDIUM IN ART[1]. (II.)

By A. B. WALKLEY.

Mr Marriott opens his enquiry by approaching art from without, and the close of it finds him, I think, left still outside. He takes the 'practical' view of aesthetics, as distinguished from the 'theoretic,' which, in its innumerable varieties, has been the view normally taken throughout the history of the science, from its Greek beginnings to the present day. Indeed, he takes the ultra-practical or 'craftsman's' view. "Craft," he says, "is a necessary condition of art, and the nature of art and of aesthetic appreciation can be studied profitably only in that condition." This is an extreme position. Mr Marriott will not take it as a reproach, but, from his own statement, as a compliment, if I call it, as Greek criticism would have called it, 'banausic.'

He starts with the familiar fact that construction is modified by materials. A stone cross naturally takes a different shape from a wooden cross. The forms of classical architecture were conditioned by the use of long stones. In the transition from writing to printing script changed as the practical conditions changed. Conclusion: "practical and aesthetic reasons are at bottom the same thing"—or "aesthetic appreciation is dependent upon the sense, which may not be conscious, of practical problems effectively solved." I fail to see how this conclusion logically follows from these premises. Because practical reasons have not been ignored in the aesthetic field, does it necessarily follow that there are no others? That they are "the same thing" as aesthetic reasons? Is conformity to the nature of the material the sole criterion of beauty? "Aesthetic appreciation is dependent upon the sense, etc." Is this true; or should we prefer to say "*includes* the sense"?

But Mr Marriott is firm about it. "Art is primarily the characteristic use of materials." Imagination (I use the word to cover both his "representation" and "design") is merely incidental—and may not be there at all. Tools and materials are 'the thing.' "The limitation of art through craftsmanship results in the expression of what we call personality." Some people would call this putting the cart before the horse. They would say that the expression of personality, in the process of externalisation, has to use craftsmanship. I will return to that later. But we are already a long way from the notion of art as a spiritual activity, as an operation of the mind.

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[1] A contribution to the Symposium presented at the Congress of Philosophy in Oxford, 24–27 September, 1920.

And Mr Marriott's 'practical' theory covers "all art." "All art is the record of human gestures in the presence of the subject or idea and as conditioned by the nature of the medium in which they are made." It seems to me this definition is at once (1) too complete and (2) incomplete. It is too complete, for it covers life as well as art. When a fly settles on your nose you wrinkle your nose and lift your hand to brush it off—both human gestures made in presence of the subject and conditioned by the medium, your own body. The record of these gestures would be, not art, but history. Similarly, with all the gestures, say, in Darwin's *Expression of the Emotions in Man and Animals.* Again, the definition is incomplete, because the word 'gesture' presents an incomplete image. Gestures of what? we say. Gesture is movement with meaning. It is the expression of something. It is the outward and visible sign of something inward and spiritual. And what can that inward and spiritual thing be but the very object of our search, the aesthetic fact?

Here we come to the kernel of the subject. It seems to me that you may go on considering gestures for ever and yet be as far as ever from possessing the aesthetic fact, the 'true inwardness' of art. The outward and visible sign will never conduct you to what is inward and spiritual. Broad washes will not explain to you the beauty of the water-colour, nor long stones, characteristically used, the beauty of the Parthenon. "Once accustom a man to the idea that art is primarily the characteristic use of tools and materials in view of the particular purpose, and you have given him a clue to the highest aesthetic appreciation." You will at best, I venture to think, have given him a clue to appreciation of the characteristic use of tools and materials. Hard by the Parthenon stands the little temple of Nikè; down below them in the town is the temple of Theseus. In each building the architect has used long stones characteristically; yet while that practical condition has never been lost sight of, three different beautiful forms have been produced, each of them an individual thing. How can you account for this save by reference to the artist's mind, his artistic impulse, vision, inspiration—call it what you will? Long stones used characteristically did not *make* these buildings what they severally were; the artist *used* them to embody his imaginative vision, the art in him. Take another illustration from a very different form of art—drama. It is one of the commonplaces of Shakespearean criticism that Shakespeare's plays were conditioned by the practical circumstances under which they were produced. They were produced on a 'platform' stage, and therefore show some peculiarities which would be out of place on our modern 'picture' stage—*e.g.* an excess of narrative and lyrical passages. The dramatic action of *Hamlet* is interrupted while the Queen lyrically describes Ophelia's death. Shakespeare makes her do this for 'practical reasons' (the conditions of the platform stage). But do these 'practical reasons' account for the aesthetic fact, the pure poetic beauty of the passage? Mr Marriott would have to say yes, for "practical and aesthetic reasons are at bottom the same thing." There you have a case where the artist has used his medium characteristically, as they used the long stones in the Parthenon; but the characteristic use of the medium would never have given you a clue to the beauty of the passage, which was contributed by Shakespeare, the poet. Further, Shakespeare had, like other dramatists, to consult the tastes and expectations of his audience. This was one of his 'practical' conditions. How did he meet it? Professor Raleigh puts the answer pithily. "The audience asked for bloodshed and he gave them *Hamlet.* They asked for foolery, and he gave them *King Lear.*" Shakespeare, then, effectively complied with the practical conditions of his problem, as did the architect of the Parthenon with his long stones. But effective compliance with practical conditions will no more account for the beauty of the Parthenon than it will for the beauty of *Lear* and *Hamlet.*

Are we not forced then to the conclusion that this 'practical' investigation advocated by Mr Marriott, the consideration of 'gestures,' medium, material and tools, can never answer the fundamental question, what is Art? The enquiry from without having failed to bring us to the goal, may we not have better luck by trying the method of enquiry from within? After all, that is the normal method. The history of aesthetics through the ages runs mainly along that line. Aestheticians, from Plato onwards, have nearly all agreed in supposing that art is primarily a 'function,' as the mathematicians say, of the artist's mind rather than of his medium or of his tools. Instead, that is to say, of seeking to penetrate to the secret of art through broad washes and long stones, they began, so to speak, at the other end and considered art as a spiritual activity. One point of view, Mr Marriott's, says every landscape is the characteristic use of paint and brush in the presence of a corner of nature. For the other point of view you have Amiel's "every landscape is a state of the soul."

What is the dominant theory to-day among aestheticians who have pursued the traditional line, of considering art as a 'function' of the artist's mind? It is the expressionist theory of Benedetto Croce. "Art and Beauty are one and the same thing, and that thing is an experience of the human spirit. We might even say that it is the simplest of all such experiences, and the earliest. Whenever a human being concentrates his total feeling into an image which is at once its essence and its utterance, the spirit of art is awakened. Beauty, in other words, lives in the creative imagination, and there alone, and art is nothing more and nothing less than the experience which we call beauty. All else is irrelevant. True and false, real and unreal, good and bad, have no place in the aesthetic world. Here nothing counts but the perfection of the imaginative act in itself, and by its own standard. And this is in principle all that we need to know about the beautiful. It has no subdivisions within itself, and its manifestations array themselves in no kind of series. Aesthetic philosophy lies in explaining the scale of beauty among the experiences of the spirit, and in defending the simplicity of the principle against traditional but unjustified intrusions. This is the meaning of Croce's doctrine that beauty is expression. And that doctrine is sound[1]."

I have purposely quoted Dr Bosanquet's exposition of Croce because he is one of Croce's most formidable critics—and, as it happens, he is most formidably critical of Croce's handling of the very subject now under discussion, the relation between art and medium. For Croce this relation is purely external, a relation not within the aesthetic fact, but between the aesthetic fact, which is pure intuition-expression, and a particular moment in the historic situation of the artist. Croce holds—it is his cardinal position—that the aesthetic activity is purely internal, the expression within the mind of the artist's intuition. He may or may not, as he chooses, extrinsicate his expressions, communicate them to others in pictures, statues, and other so-called 'works of art.' But the real work of art is the internal expression, of which the picture or statue is only a practical record. Of course the artist does not work *in vacuo*. His aesthetic activity is exercised in a certain historic situation—at a given moment of time, in certain surroundings, with a certain mind and temperament. His intuitions will depend on the circumstances of his historic situation and, if he externalises them in 'works of art,' these circumstances will include his medium and tools and the joy (since every practical end successfully achieved involves joy) of "using" them, in Mr Marriott's phrase, "characteristically." But they remain always 'historic situation'; they are never 'art.' This abolishes the distinction between the arts according to the media or means of externalisation. "Since the individuality of the intuition involves the individuality of the expression, and a picture is distinct from another picture no less than from a poem, and pictures and poems are not of value for the sounds that strike the air and for the colours that are refracted by the light, but for what they have to say to the spirit in so far as they come home (*s' interiorizzano*) to the spirit, it is vain to turn to the abstract means of expression to construct the other series of kinds or classes; that is to say any theory whatever of the division of the arts is without foundation[1]."

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[1] Bernard Bosanquet, "Croce's Aesthetic," *Brit. Acad. Proc.* IX.

Of course the 'man in the street' will never swallow this. With him, perhaps, it would be idle to argue on a question of philosophy. But the man in the street has on this issue a potent philosophic ally, Dr Bosanquet, whose high authority as an aesthetician makes it impossible to ignore him. He is all for the old division of the arts. "After all it is necessary for philosophy to study the specialities and limits of the arts, for in considering them we are considering the qualities and aspects of the human spirit; nay more, of the spirit which is in the world. If you know or can feel how the beauty of bronze—that is, its expressive capacity—differs from the beauty of marble, you are on the way to understand the diverging beauties of the arts; and if you insist on neglecting these affinities of the spirit, your theory remains abstract, and has no illuminating power[2]. No illuminating power! For my part, I find a theory which (apart from its general applications) unifies all the so-called 'arts' and shows me their common basis one of the most "illuminating" in the whole history of aesthetics. Not that Croce denies—who could?—the *practical* value of the old classification of the arts. Indeed, he expressly insists on it[3]. What he denies is its *theoretic* value: its place in a philosophy of aesthetics. "To reject," says Dr Bosanquet, "the function of the body—our own and nature's—is not to honour but to bereave the spirit." But Croce does not reject it; he simply removes it out of a particular category.

Elsewhere[4], Dr Bosanquet, I submit, does injustice on this point to Croce by an imperfect quotation. "Croce says that the artist has every stroke of the brush in his mind as complete before he executes it as after. The suggestion is that using the brush adds nothing to his inward or mental work of art. I think that this is false idealism. The bodily thing adds immensely to the mere idea and fancy, in wealth of qualities and connections." Now what Croce says (*Estetica*, Ch. XIII, end) is: that the artist "never gives a stroke of the brush without having first seen it with the imagination; and if he has not yet seen it, he will give it, not to extrinsicate his expression (which in that moment does not exist), but as it were on trial, and to have a mere *point d'appui* for further meditation and internal concentration." The stroke of the brush, that is, gives him a new 'historic situation,' from which he will have a new intuition. The bodily thing, then, does not 'add' to the idea and fancy; it creates a new *milieu* for the idea and fancy to work in and so to produce new expressions. Croce is trying to isolate the aesthetic fact in the idea and fancy; Dr Bosanquet says no, you must include the bodily thing, the *milieu*, in it, too. According to Croce, "the old aesthetician Baumgarten advised poets, as a means of promoting inspiration, to ride, to drink wine moderately, and, if they were chaste, to look at pretty women." Schiller, the common anecdote goes, used to smell rotten apples for the same purpose. These are all 'historic situations.' Would Dr Bosanquet include them in the aesthetic fact, too?

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[1] *Breviario di Estetica*, 71.

[2] Bosanquet, *loc. cit.* 12.

[3] Croce, *op. cit.* 72, 73.

[4] Bosanquet, *Three Lectures on Aesthetic*, 73.

Finally, there can be no question of tabulating the consequences and advantages of the Crocean view of art, as Mr Marriott does with his view, because that would carry us far beyond the limits of the present discussion—the relation of art and medium. It will be seen, however, from what has been said, that Croce sweeps away not only the false distinctions that Mr Marriott abolishes but also those that he retains; not only the distinction between representative and non-representative art and the distinction between the various forms of the same art, but also the distinction between one art and another. Further, he not only, with Mr Marriott, brings all practitioners of the same art into the same category, but *all men*, in the act of expression, into the single category of the artist.

MIND AND MEDIUM IN ART[1]. (III.)

By HENRY J. WATT.

As in so many of the general problems of art it is important to coördinate discussion that springs chiefly from the arts of vision with their less substantial sister. In music, at first glance, we might be tempted to say there are no tools and materials. The composer records his inspirations upon paper with a view to legibility only. He elaborates in imagination the effect he desires, and restricts his record to the minimum required by the performer for adequate reproduction. The performer in turn must be able to imagine from this record, at least approximately, the effect intended, and he will then be able to produce that effect for others on his instrument, voice, violin, piano, etc. He may in some cases be able to enjoy it completely apart from any real instrumental performance. But this power clearly presupposes very perfect acquaintance with the sounds produced by a performer on the instrument for such a record. Hence clearly the musical instrument is a necessary medium in the progress from composer to enjoyer. So instruments are the materials of music. Perhaps the performer might be held to be the tool. But there is no value in carrying the analogy into such detail.

The instrument has long been recognised as an important condition of 'style'[2] in music. The human voice is a very special instrument with its own great virtues and limitations; it cannot be treated by a composer as if it had the freedom and range of a violin or organ, or as if it were devoid of that marvellous power of changing its timbre from instant to instant at the behest of the singer, a power lacking (? almost) entirely in the piano and only imperfectly attained in the organ at enormous expense. Orchestral works can be *studied* at the piano, but they must be heard or imagined in the orchestra. Berlioz—great genius of orchestration—relates with scorn how in his contest for the *Prix de Rome* he was required to produce a work for voice and orchestra which was judged by reproduction on the piano, while the winning work was properly heard only after judgment had been given ("*par des gens qui ne sont pas musiciens*")[1]. All this is probably commonplace amongst musicians.

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[1] A contribution to the Symposium presented at the Congress of Philosophy in Oxford, 24–27 September, 1920.

[2] Cf. Sir Hubert Parry's *Style in Musical Art*. London, 1911.

Would it were so amongst the great public and the performers who meet their taste! Every other *virtuoso* regales us with Wagner's Pilgrims' Chorus on the piano, an instrument in which that work produces only a farcical effect. Just because it is designed for the orchestra, it is obviously difficult to play on the piano and so, in those for whom essential beauty is a secondary or remote consideration, it excites wonder and applause. The translation is as appropriate as would be the embodiment of Laokoon in glass or straw. To write piano pieces round the melodies of Schubert's loveliest songs, as Liszt did, shows an equal lack of taste. Anyone who really knows and appreciates the song could only be bored and disgusted with the piano's feeble echoes of the rapture of "Hark, Hark, the lark," or the tempestuous terrors of the Erlking.

And it is quite easy to account for the characteristic style of each instrument. The organ has always been and must ever remain a somewhat lethargic instrument incapable of producing intensive stresses. But it gives sustained tones of great variety. So the composer has perforce to build his works with tones of certain intensive, temporal, and colour values. The bricks and mortar of piano works are very sharp and short, but intensively malleable with a rather limited and mysterious range of colour. The violin has almost complete freedom in action, sustenance, stress, and colour. These things are familiar and need only be briefly recalled.

Let us now turn to Mr Marriott's thesis, which seems kin to the thoughts of Semper and others[2]. Are practical and technical reasons the same thing as aesthetic reasons? The answer cannot be a simple yes or no. We must distinguish two aesthetic processes—aesthesis, or act of enjoyment, and judgment thereon. Judgment we may at once put in its place. It is always cognitive; it always implies a conceptual apprehension of the objects, predicates and relations involved in it, whether these objects be things, sensations, or mere subsistents, and whatever the predicates and relations may be. The aesthesis is not so easily placed. It is of course always an act involving a 'relation' or unifying process of experience. It cannot be mere sensation for example; for then it would be indistinguishable from the sensory data of cognition or of instinctive action. Nor can it be merely subjective, as if the objective counterpart of it were irrelevant, i.e. the tones, colours, forms, of the beautiful object. Beauty cannot be skimmed off all its actual forms in a pure essence in which they play no part. Beauty like knowledge always attaches to some object.

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[1] "N'est-il pas évident que le piano, anéantissant tous les effets d'instrumentation, nivelle, par cela seul, tous les compositeurs?" *Mémoires*, vol. I. chap. 22.

[2] Cf. E. Meumann, *Aesthetik der Gegenwart*, Leipzig, 1912, 125 ff.

The analogy of knowledge might tempt us to suppose that there is a 'logic' of beauty, shall we say "intuition expressing itself," as there is a science of the pure concept and its relations[1]. It is quite legitimate to make abstraction of the forms and laws of any range of cognitive processes from their (integrative) basis, as logic abstracts from the basis of any meaning in my mind or yours at one and another moment. But that need not lead us to suppose that logic is completely independent of knowledge and psychology. We must land ourselves, as the greatest have done, in a philosophical *cul de sac* if we do not realise that all knowledge is one great system in which the highest grows continuously out of the lowest. The crown is not arbitrarily placed upon the head of sense or assumed by it. The kingship is inherent.

In beauty we shall hardly find a parallel to this. There must always be an act of grace or marriage between the objective and subjective in beauty, a marriage of true minds, born for one another and finding each other's nature predisposed for harmony. We have to discover the secret being of this harmony that is the relation between subjective and objective mind. Namely, what is that rhythm of interaction between tones in all their variety, groupings, and sequences, between colours, forms, and motions, between smells, tastes and other limited resources of the simpler senses, on the one hand, and the pulses, acceptances, anticipations, appetites of the subjective mind, on the other, that appears or is for us pleasure? How shall we describe the collisions, refusals, divagations, and distresses between these two streams of actions that is displeasure? A pure science of these things is hardly yet in existence. Until it has been fully and intimately grounded in the detail of knowledge, I do not see what value such formulas as the "expression of intuition" can have. These terms are so vague and general and their whole atmosphere is so inimical to the detail without which general propositions (except in logic) dare not be advanced, that they can only be set down as anti-science and obscurantism.

Having thus divided the aesthesis into three parts, we may notice that our knowledge of the subjective side is exceedingly vague and schematic. Our knowledge of the 'relation' that is pleasure-displeasure is equally feeble. The objective side alone is open to inspection with moderate ease. But even here there is great controversy. What especially is included in this objective field? A great wave of opinion has drenched us with the dogma that only sensory data and complexes are admissible. Away with all 'subject' and association! No perceptual, reminiscent, associative, literary, anecdotal, thematic adhesions are to be tolerated in any art (*scilicet* visual art). Colours, forms, times, spaces (indicated),—these and the like alone are fit elements of a work of art.

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[1] This is the thesis towards which Mr Walkley inclines, though I am not at all sure that he really means it to be taken in so extreme a form.

But there is a good deal of perversity in this dogma in spite of the earnestness and desperation with which it has been preached. For music has within the same period been striving to attain the very opposite goal, namely to force upon its medium perceptual adhesions that can hardly be induced even to cast a shadow in the desired direction. This is another thing that makes it so important always to compare the visual and auditory arts. They are so differently constituted that they are nearly always trying to do opposite things. Vision gets tired of being representative and tries to be as directly divine as music, which at the very moment aspires after more and more contact with the things of earth. Vision longs to be rhythmical while music wooes the rhythmic freedom of adagio[1] and might even yet become a new unmeasured music. Vision makes frantic efforts after a pure harmony of colour, when music tries to shake out of itself the last traces of modulated sweetness in order to be more characteristic of things that are not so clearly inherent in its nature. Of course it is well to create an art of characteristic noise, and it may even thrill our being; but it no more affects the course of music that has been building itself up in a steady progress through the centuries than the beauties of prose annul and negate the much older and still potent charms of poetry.

So we may pass over the attempt to create a dogma of limitation. The arts of vision in the greatest creators have known no limitation to the purely sensuous range of data. They have always freely admitted whatever furthered their virtues, felt as these are in the immediate poignancy of the aesthesis. In the Winged Victory, the Laokoon, the Creation of Adam, the Last Judgment—there is much more that is of the greatest aesthetic force than ever the mere eye can see.

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[1] "Hier steht nun das Adagio dem Allegro gegenüber, wie der gehaltene Ton der figurierten Bewegung. Dem Tempo adagio giebt der gehaltene Ton das Gesetz; hier zerfliesst der Rhythmus in das sich selbst angehörende, sich allein genügende reine Tonleben....Was im Allegro der Wechsel der Figuration ausdrückte, sagt sich hier durch die unendliche Mannigfältigkeit des flektierten Tones." Wagner's *Gesammelte Schriften*, Leipzig, 1907, VI. 284.

But the difficulty is to show how these perceptual moments link up with the purely sensory data to form the objective stuff of art; we think of course that we understand all this perfectly, simply because we know which colours to put together, which tones to group and to serialise. As one of my critics tells me, and as many others think, "The rest may reason and welcome; 'tis we musicians know." But they do not know. Nor do the colourists know why one colour, one form, one space goes with another. Guesses can be made and some accord fairly with the little direct psychological knowledge there is. But we are far from the needful pure science of these things. So long as this is so, what right has anyone to say (in the face of the practice of the great masters) that perceptual processes do not belong properly to the objective stuff of art? Of course I should not dispute the fact that some subjects are tawdry and dangerous, and great regions of non-perceptual art in vision may still lie unexplored. For a medium sets limits and in doing so it opens up channels closed to other media. The question concerns the canons of art only.

The difficulties of the problem may be exposed in another way. Colours, forms, tones and their groupings, etc., may obviously be beautiful. But, tell us, can a subject possibly be beautiful? Frankly, is it conceivable that a thought, an idea, a perceptual substance-unity (the orange without the colour, shape, smell, weight, etc.—what is left over), or an association, can be beautiful, *schön* ('shining')? Never! Hence let subject be for ever banned from art (and dragged into music).

Let us answer this by a counter-question. Why are colours and forms beautiful? Because they 'shine' and grip the eye, sending a thrill into the soul through a pore of that sense, as it were? Beauty streaming to you straight from the canvas and labelled such from its very start? Surely not. Tones are not felt to be 'out there' at the piano, in the singer's throat: they are everywhere, nowhere; they do not 'shine'; they merely blend with one another in your being so that you think they and your soul are one (or some other such vagueness). In fact, again, we know next to nothing of the basis of these sensuous delights. And as we succeed in laying bare their nature, they seem strangely to evaporate into things that look like relations, clearness of sequence, repetitions, variations, and echoes, that all seem so like those things with which only the conceptual intellect can deal adequately. But this last conclusion would be very wrong indeed. They are clearnesses and distinctnesses and recurrences of sensory stuff that strike and thrill our subjective mind to its responses. They are not conceptual relations of the intellect. This is a mistake made by most prominent thinkers about art. It has led one of them recently to assert more or less definitely that a person who does not *know* all that is going on in a musical work, the tones, intervals, chords, keys, modulations, reminiscences, etc., can no more properly enjoy it than he could enjoy poetry in an unknown tongue[1]. That I am persuaded is quite wrong, untrue to the great and spontaneous joy that the greatest art gives even to the inexperienced provided they are 'pure in heart.' One could hardly dispute that the trained and swiftly judging mind must reap the greater and fuller joy from the art. But the germ of enjoyment is untutored. If it were not so, no intellect could bring it to birth.

If, then, we do not know in detail the roots of beauty in colours and forms, how may we deny that beauty can also spring from perceptual, conceptual, memorial and even emotional constructions? We simply cannot.

But it seems perfectly clear and certain that without a basis of sensory beauty there can be no work of art. Pure intuition or no, a work of art must be created, it must be embodied. And beauty must permeate its whole being, sensory, perceptual, imaginative, from whichever end of the series its soul may spring.

We may now return to the problem of discussion. That now takes form in the question—are there any limits in art to the range of perceptual and other adhesions to the sensory basis of the work? Do these adhesions include such as are concerned with the medium and its characteristic manipulation? Or does the range of the latter out-reach the former? For, of course, it is out of the question that actual matter and its manipulations could bodily enter the aesthetic object, which is always as apprehended and known, i.e. essentially mental or spiritual, as Mr Walkley properly recalls. The sole question is,—are these perceptual and cognitive attachments irrelevant to the scope of the aesthesis or are they not? And my answer is,—they are not and never can be irrelevant in good art, i.e. *quâ* in a good spectator. But on the other hand there is no means of fixing a norm for the extent of them that a good spectator shall have at his disposal. That is simply another form of the truth often so crudely distorted that powers and therefore tastes differ. The judgments by which they express themselves cannot be disputed any more than can any other facts; but their basis and general validity can be discussed.

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[1] C. Stumpf, "Die Attribute der Gesichtsempfindungen," *Abh. d. preuss. Ak. Wiss.* 1917, VIII. 56 f. "Wie dagegen ein wahrhaft künstlerischer, *verstehender* Genuss unserer harmonischen und polyphonen Musik ohne *jede* Zerlegung, jede Beachtung des selbständigen Ganges der Stimmen möglich sein soll, wäre schlechterdings nicht einzusehen. Dabei wollen wir gar nicht bestreiten dass auch ein Dahinströmen ganz unzerlegter Klangmassen durch das Auf- und Absteigen des Ganzen und durch den Wechsel der Klangfärbung und der Zeit- und Stärkeabstufungen gewisse 'musikalische' Stimmungen hervorrufen kann. Der Geniessende mag dabei der Musik gegenüberstehen wie etwa den Farben der Kalospinthechromokrene oder wie einer ihm zwar unverständlichen aber mit Effect gesprochenen Rede." My analysis of the primarily melodic functions of music shows that this account is far from the whole truth of the 'natural' apprehension of music. An exact parallel to this applied to the 'natural' enjoyment of a picture or of dancing would surely be ridiculous. Debussy on the other hand dreads in the auditor a knowledge of means. Does not this cast some doubt upon their dignity?

We are now merely repeating what was noted above that the spectator brings his mind and all its sensibilities into the aesthesis. Much of this mind, namely the perceptual, associative, conceptual 'forms' that apply themselves to the sensory stuff offered, become simply continuous therewith and form the objective mind of the aesthesis. The rest of the mind's dispositions are free to respond to this presentative complex and to play with it until its interest is exhausted. You cannot show marble, bronze or wood to a human eye without its perceiving the material and thinking about it in some way or another. Whatever thus accrues to the aesthetic moment must either favour or hinder the aesthesis. Being intimately bound up with its objective part, these adhesions cannot remain indifferent to it. They will either agree with the colours, forms, and manipulated textures of the bronze, or contrast with them. Some minds are pleased by contrast; it arouses their wonder and astonishment. Perhaps this motive makes an appeal in the choicest circles more frequently than is commonly supposed. Has not a notable composer of ours[1] declared that novelty is the essence of the beautiful? Does not the present age of strenuous inspiration try desperately to produce wondrous effects, using devices that many of the previous masters may well have rejected in principle? But of course this makes a much more artful wonder than the bottled schooner in full sail or than the veiled figure in an ivory piece. The main trend of the beautiful seems, however, to keep aloof from this fringe of the marvellous in mere technique. No doubt a great deal of pure joy can be got from a sense and knowledge of the nice operations in a work of art. But surely the first ground of the aesthesis in sense must be the natural sensuous coherences themselves of colours, tones, forms, and motions. And from this ground progress towards ideas and emotions should in the main be direct, not swerving far aside into special perceptual tracks, unless it intends thereby to return to the final reaches of mind with greater force and appeal. Nothing can of course prevent the normal range of perceptual and other associates appearing, while greater experience in art will inevitably refine and extend this range in special directions. Thus a work must appeal differently to the tyro and to the expert. And there must be a world of artistic objects for all grades of experience. Are we not nowadays learning to 'appreciate' the art of the negro, the primitive, and the peasant? Thus art must always allow for and largely first build upon the normal contents of mind and memory.

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[1] Cyril Scott, *Philosophy of Modernism in relation to Music*, p. 3: "In order to create something beautiful, one must create something new." Mr Scott chooses a mid-way ("Romanticism—newness within limits") between classicism and futurism and in a certain sense his statement is a truism. But it is surely strained to imply, as his examples suggest, that the newness that is beauty should always reside in some general feature or style of work.

And it is perhaps one of art's most telling devices to appeal to mind by some special deviation from the currents and expectations of the normal mind. In this sense it may be true that novelty is the essence of art. I would rather say that the purpose of the novelty is to direct the mind of the spectator into a special direction. But this device will hardly extend to the usage of the medium of art in ways that are unnatural.

In summary we may say that the objective aspect of art cannot be limited to strictly sensuous data. It must be extended to include any 'objective' that is adequately founded upon these data. Hence the largely conceptual arts of literature. The spectator's awareness of all that pertains to the material of art and its manipulations thus comes within its scope. But I fail to see how one could reasonably hold that of all the non-sensuous objectives of art only those appertaining to the material and its manipulations should be admitted. Zeal for an important and perhaps neglected part may blind one to the rest, just as one's contempt for a greatly exaggerated value (e.g. of subject) may lead one to depreciate it unreasonably. The field of art is the whole soul and its congruences,—and its conflicts, too, its dissonances as well as its consonances (provided we can use them 'paraphonically'). It is the extreme sweetness of many subjects that has made subject such taboo. Great skill is needed to make art of these pretty ('easy') beauties. The discords of life that also appeal so strongly to our emotions need similar skill of treatment. And just as music gains more perhaps from the progressive conquest of dissonances, so the greatest wealth of the arts of subject has probably always lain on the side of the dramatic and tragic and less than pretty. But in the sphere of the perceptual associates of the medium of art, there seems to be no analogue of this; we do not see great artists growing more artful in the false, perverted, and imitative use of materials and tools, nor does the art seem to flourish that scorns technique, perverts good form and flouts good taste for no artistic end but merely for the love of novelty.

Music is comparatively free from these remoter influences of tools and materials. In the visual arts perceptual associates can be involved that are not at all visible but merely known, as one knows that the arms of a marble cross bear great strain, which again one knows is a natural virtue of any sort of wood, even of a tree branch. In music probably every aspect of the instrument that is artistically relevant is actually heard, as the grain of wood and marble is seen. Do we ever genuinely find a work the lovelier, the better its style suits the mere instrument, as Chopin's music suits the piano? Is it not enough to say that his music suits the tones that are typically produced by piano action? An auditor does not seem greatly urged to go beyond the heard sound and its typical characters to the thing that produces them. He may have special interest in instruments and he may know them by their tone. The different trade types of piano differ greatly in their timbre, but even acoustic specialists could hardly say at all precisely why they differ and no aesthetic listener probably takes any thought of the point of hammer blow into his aesthetic moment. And yet this is clearly a matter of the characteristic use of an instrument. I think one may take it that the finest violin music may be completely enjoyed without any thought of the material, manufacture, technique, and environment of origin, of the performer's violin. So although there is a vast amount of the greatest interest to be known about instruments in relation to music, I doubt whether it has any necessary aesthetic value whatever.

But perhaps we do wrong to mention the instrument in this connexion. We might rather think of the performer's vital, beauty-expressing movements than of the mere mechanism that carries them to the sounding strings. Should we have thoughts of Pachmann's finger-action and pedalling in order fully to savour the beauty of his touch? Must we first know how to play the big drum and cymbals, the bassoon and viola, ere we can hope to exhaust the loveliness of a symphony, and must we think of the playing of every instrument while we listen? Surely not. Such considerations are probably of little influence in music; certainly far less so than in the visual arts. Tones in a sense are divorced from material and are not shaped even by the tools that physically determine them. They are entities of independent being, divine as folks say, again.

But in thus discarding the most of the perceptual and memorial associations of music and its instruments and performers from co-operation with the aesthesis, I would not be thought to set up any limits in this respect. Such limits cannot be set up in the visual arts where association is more direct and powerful. It is not necessary to have any fixed stock of technical thoughts, but everyone has many of these and they are naturally prominent and influential. In music such thoughts also exist, but they are not naturally responsive and influential during aesthesis.

Finally, I think a protest should be made against the attempt to turn art into a kind of utilitarian practice. Art may well have originated in the useful crafts and it has something in common with the gestures and expressions that are of social utility. But just as the study of mind from the biological side has in the primates had to recognise beyond hunger, love and play a delight in mere sensuous or intellectual effects, as when a monkey plucks at a vibrating pin or a child beats a drum indefinitely, so the science of art must rise above the biological cant that was natural in a recent generation and restore even to popular favour the idea that art is one of the three supreme ends of the spirit, a delight in congruous (true, good) being. After all biology now proclaims loudly that an organ is first formed and then, may be, finds a use (the world has not much time to waste on useless things). Well, then, art and knowledge and morality have first been formed and have been put out on loan. No amount of use or habit-craft and no amount of mere 'need' would ever have brought them into being. And so no matter out of what actions or organs they arise and no matter how they are used, they are values essentially independent of these attachments. Mr Marriott's maxim can only be true if he means by characteristic just *aesthetic*. If, however, he means to suggest that what is in some sense objectively characteristic is found by us to be also aesthetic, does he not go far to show how the values of art may be objectively or *really* grounded and permanent, as knowledge is?

It may be the recognition of this that has drawn Mr Walkley towards the nerveless abstractions that Croce puts in the place of the theory of art. Although knowledge and beauty are unique phases of the spirit, they must be used by the mind as a whole. The new essence they bring to birth is not degraded in the least by any interaction with the other level of mind. It must surely be evident that things cannot be taken out of the personality or imagination or intuition of an artist and embodied in sights, sounds, or motions, unless these vessels can *contain* them. Here we come upon the majestic truth that "I and my Father," the work of art and the artist's 'personality' to that extent are one and yet so completely two.

MIND AND MEDIUM IN ART[1]. (IV.)

By EDWARD BULLOUGH.

I agree with Mr Walkley's criticism of Mr Marriott's proposal to base an aesthetic theory upon Medium and its technical manipulation. The attempt has been made more than once—the example of Semper has been mentioned by Mr Watt—and has always broken down in the face of objections such as are urged by Mr Walkley—in the last resort because Art is a manifestation of mind and cannot be satisfactorily accounted for by the nature and treatment of the material it employs.

The second part of Mr Walkley's paper rests upon the theory of Benedetto Croce. I am in doubt how far I agree or disagree with him. Croce's aesthetic theory does not stand by itself, as is far too commonly supposed. It is part of a general philosophical, and in addition monistic, system and is exposed to all the difficulties created by such a dependence. I feel doubt about the fourfold division of the activities of the spirit; about the precise limits of the theoretical and practical activities; about the ultimate implications of his general point of view; as regards the aesthetic sphere in particular, about the nature of the 'intuition' and the essential 'lyricism' of Art, and their consequences. Indeed, I am inclined to agree with the objections recently urged by G. A. Borgese in the Preface to the new edition of his *Storia della critica romantica in Italia*[2]. I readily admit that Croce's conception of intuition as the

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[1] A contribution to the Symposium presented at the Congress of Philosophy in Oxford, 24–27 September, 1920.

[2] "Compiutosi in De Sanctis lo svolgimento della critica cosidetta romantica, due vie restavano aperte; o attuare ulteriormente e più risolutamente quella restaurazione del senso classico dell' arte che in Italia paradossalmente si chiamò romanticismo, o, traverso quello pseudoromanticismo, pervenire ad un romanticismo autentico.

Il Croce s' avviò, senza notevoli perplessità, per questa seconda strada, e sempre più si raffermò nel considerare l' arte come instinto, come 'Genie,' come irreducibile individualità, come 'hohe Intuition' anteriore alla logica e alla volontà, come liricità pura, come interiezione, come grido. Il suo metodo critico si andò sempre più precisando in tre canoni: (1°) negare qualunque continuità di sviluppo nella storia dell' arte, tutta frantumandola nelle monadi senza finestre delle singole individualità poetiche; (2°) cercare in ogni poeta l' opera fondamentale e genuina, generalmente di prima e spontanea gioventù, rifiutando come sovrapposizioni logiche e volitive i suoi pretesi progressi di maturità; (3°) leggere ogni poeta ed ogni opera come un' antologia in cui alcuni frammenti lirici

kernel of the artistic attitude, his antagonism to the current treatment of the history of Art and to the sterile discussions of the differences and relative position of the arts, and his insistence on the unity and uniqueness of Art, etc. have rendered immense services to aesthetic studies; yet I cannot but feel a certain sympathy with the complaint of Professor Bosanquet (with whose aesthetic views I otherwise disagree) that Croce's theory yields little enlightenment on the particular subject now under discussion. Croce's theory is a philosophical, not a psychological, statement, and there is much that seems psychologically obscure in it. Moreover, the subject of the present debate is in itself, as a matter of psychological fact, so obscure that its elucidation would appear to me as little furthered by a philosophical treatment which deals with it from an ideal point of view, as by Mr Marriott's hypothetical illustrations which substitute assumptions for experience.

I. What do we actually know about the relation of Mind and Medium in Art?

By *Medium* I assume we mean the material or physical datum which the artist handles: paint and canvas; chalk and paper; marble or bronze; brick or stone or concrete; the tones of an instrument or the human voice; the human body, its gestures and attitudes, whether in actual space as in dancing or acting, or in imaginary space as in the cinematograph film.

By *Technique* I mean the manipulation of these media.

The *Mind* refers, I take it, to the mind of the artist as well as of the spectator or listener (recipient), but chiefly to the former.

As regards the term *Art*, I must beg leave to make one or two more extensive observations. In the first place we would, I believe, all agree that Art represents but a segment of the whole range of aesthetic activity. The aesthetic attitude embraces the appreciation of natural objects as well as of artefacts; it covers certain borderlands of Art such as ceremonial and ritual which have admittedly artistic affinities; it permeates ordinary human intercourse in what we vaguely call 'manners.' As a matter of personal conviction, I hold that there is nothing in the whole range of personal experience from sheer sense-experience to the most abstract thought which may not be the object of aesthetic contemplation. To speak of medium in this case becomes exceedingly difficult, because the medium is, I presume, one's very self—as, in fact, it is in certain

siano artificialmente ricuciti per l' illusione extraestetica di raggiungere l' unità. Con particolare evidenza i suoi saggi recenti, specie quello su Goethe, mostrano la volontà di sottoporre quasi ogni dramma, quasi ogni poema all' analisi disgregatrice che la filologia romantica sperimentò sui poemi omerici." (pp. xix–xx.) G. A. Borgese, *Storia della critica romantica in Italia*, Treves, Milano. 1920.

types of acting: *la plastique de l'âme.* Of this vast realm of sometimes most tenuous and elusive experience, the title restricts us to the most condensed—if I may use a contradiction in terms—the most material, but also the most deliberate and most intentional form of the aesthetic state.

II. Further, I believe it to be important to point out the distinction between *Art in its static* and *Art in its dynamic aspect.* The distinction is important because, though it is constantly overlooked, it affects the relation between mind and medium in several particulars, and incidentally also the conception of a history of Art, the interplay of social and individual factors and the continuity of artistic and aesthetic tradition.

By Art in its static aspect I mean what I cannot do better than describe as "the objective world of Art," i.e. the accumulations of art-treasures in our museums and collections, the art-*objects* as material things, independently of all considerations of their subjective origins, the intentions and ideals of their creators; independently also of our aesthetic attitude to them, regarded merely as objects, as remains of antiquity or illustrations of the past.

Art in its dynamic aspect is Art regarded from the point of view of the creative artist or actively appreciating recipient: as an aesthetic object. There is of course a constant interchange between these two conceptions of Art. An art-object which to-day is dynamic for me, may to-morrow have become static. A picture may one day belong in my eyes to the objective world of Art and the next day may assume a dynamic aspect. To say that as a static object it had no aesthetic existence for me, is of course true—that is precisely the distinction—; yet I knew all along that it was an art-object and was regarded as such by others for whom it had become dynamic. And its claim to become dynamic for me also was there with it and exercised a certain moulding influence upon my aesthetic attitude. This silent, collective and largely anonymous pressure of the "objective world of Art" is probably the most potent agency in the art-education of the individual. It represents in its cumulative force, created by the forgotten but combined verdict of past generations—to whom it also owes its preservation—a tremendous standardising power upon the schooling of appreciation and produces adaptations, expectancies, apperceptions and emotional reactions which in their varying degree of rigidity and durability result in more or less fixed habits of taste. This applies to generations as well as individuals. The "objective world of Art" and dynamic Art are naturally never coextensive. For the majority of persons the vast number of art-objects never become dynamic; but the appeal to become so remains and is satisfied in proportion to the powers of imagination, the emotional elasticity and the extension of the organised interests of the individual appreciator.

The same standardising pressure is exercised upon the artist. The beginnings of every artist are necessarily imitative. What should he imitate but what was there before him? This necessity is reinforced by the capacity of absorption and of admiration which distinguishes the artist as a psychological type. It is further forced upon him by the perpetual technical struggle in which every actively creative artist lives. Defeat in this struggle means incapacity or mannerism. Indeed, it is precisely on the technical side that static Art exercises its strongest influence upon the artist, for it represents the continuity of technical development—distinct from, though not unrelated to, the aesthetic discontinuity of Art history—and technique is the one thing in Art that can be taught—up to a certain point. Incidentally static Art acts as a kind of background for the ricochet of general public sentiment and its attitude towards Art upon the artist, and as a reaction both to this sentiment and to the technical teaching beyond the point where in the artist's development technique ceases to be impersonal, common knowledge and becomes an intimately personal process interwoven with his artistic imagination, static Art becomes the spring-board of reaction, revolt and revolution. For all great artists have been revolutionaries, both in their technique and in their creative work as a whole.

In this manner, the objective world of Art, by reason of its persistence has in a sense a survival value, and as the expression of certain forms of historical continuity, represents for the artist and the appreciator a kind of collective and socially-conditioned standard and—up to the above-mentioned point—a similarly socially-conditioned, impersonal technical tradition.

As constituent elements of the objective world of Art we encounter the various 'arts' and crafts, as historically they have arisen, developed and differentiated themselves in their materials, techniques, styles and ideals, and have created different professional habits of thoughts and tastes and distinct expectations and anticipated standards in their respective devotees. Art as the formulation of a unique and distinct psychological attitude is unquestionably ONE, and these different art-forms, as I should prefer to call them, appear largely as the outcome of accidents of history, climate, habitat, economic conditions, inventions, cultures, and in the last resort, of our physiological constitution. But they are for all that aesthetically not irrelevant, however little result the past discussions on their aesthetic differences and relations have yielded. The error lay in regarding them as fixed, predetermined and logically deducible species, each with its own 'laws' and 'principles.' The art-forms which we know and distinguish conventionally, are not, of course, the only 'arts.' In other countries, at other times, activities which we do not regard as a form of art were reckoned among the arts and *vice versâ.* It is the subjective attitude which recognises certain activities as art-forms, and the equally subjective repercussion exercised by them, once (and while) they are so recognised, which make the 'arts' aesthetically relevant. Even at the present time we are witnessing in the development of the cinema-film, however much prostituted for the moment, the rise of an entirely new 'art,' and there is no limit but that of our physiological constitution to aesthetic imagination and its interaction with non-aesthetic inventions for the production of material art-forms. Nor is there ultimately any limit at all to the range of subject-matter of the aesthetic psychosis itself. The 'arts' are merely the forms of 'exteriorisation' which in the course of history have been relatively distinct and continuous strands in the art-tradition of mankind.

The influence of the media in producing changes in the technical procedures and thereby in the historical styles of the art-forms is so well known as to require but mere mention. The effect of small units of building materials upon the development of architectural forms and eventually upon the growth of the Gothic style; of Roman cement upon Roman vault construction; of reinforced concrete upon the structural problems and architectural forms of the present day are notorious instances in Architecture. Equally well known is the influence of tempera upon certain characteristics of Italian Renaissance painting and the changes introduced into the art by the use of oil-pigments. Comparable to the ease and freedom of oil painting have been the effects of the development of the piano on music, and the similarities of the consequences, both good and bad, have frequently been noticed. The differences between the use of marble and bronze in Sculpture, whether in the round or in relief, are to be found in every text-book on ancient Sculpture. The peculiarities of French verse in contrast to English, German, Italian and Russian are due for the largest part to the absence of a marked stress accent in the French language, and the use of intricate classical metres in English poetry is practically impossible because the language is so largely monosyllabic. Changes in the shape of the stage and in its relation to the audience, in decoration, *mise-en-scène* and personnel reacted on the nature of 'theatrical illusion,' types of play and the whole style of acting. The transition from the Commedia dell' arte to the regular drama had its repercussion on the psychology of the actor.

It would be easy to multiply *ad infinitum* such influences of media upon the art-forms. The point I wish to make here is that in changing the forms and traditional styles of the arts the effects of the different media are crystallised and incorporated by means of their techniques in the objective arts and, as stated before in more general terms, come inevitably to form the basis for the technical rudiments of the budding artist and set up new standards of technical handling in the mind of the recipient. One cannot but be impressed—whether favourably or not—with the profound imprint, for instance, of the 'classical orders' upon the mind of the architectural student, ever since Vitruvius has become the bible of the 'classical' architect. The extent and depth to which the student's vision is moulded by the shape, dimensions and proportions and formal functions of these orders is enormous with the cumulative force of century-old training on these lines. This is perhaps an extreme instance of the pressure of historical style, but similar results, if in smaller degree, will be found in other art-forms.

Mr Watt points out that the musical instrument and its technique is an important condition of 'style' in music. Indeed, 'style' has often been defined as the consistent treatment of a particular medium. This is true, if by style is meant what I have here called 'historical style.' But style in the purely artistic sense is not the consistent treatment of an impersonal, physical medium any more than technique is Art, but a consistency in the artist's personal vision, "consistency of distance" as I have called it elsewhere[1]. The manipulation of the material medium enters into this vision, no doubt; but it is not, either directly or by itself, the basis of 'style.'

III. What is the process by which the medium enters into this vision of the artist? This question leads to the consideration of Art in its dynamic aspect, i.e. to artistic creation and aesthetic appreciation.

What is the dynamic relation of the artist to his medium?

By way of apology for much that I have said above, I venture to submit that nearly all aesthetic theory of the past and of the present seems to me exposed to the serious criticism that it deals with the artist, so to say, *in vacuo*, psychologically and philosophically, without any reference whatever to the social unit, social relations and interactions in which he finds himself. This treatment reached its height with the Romantic Movement in Europe, and, as if to redress the balance against this romantic survival in Aesthetics, the type of theory called 'sociological Aesthetics' came into being, with its equally one-sided insistence upon Art as a social manifestation, as an 'expression of society.' This view, usually going back to Taine in some form or other, was supported by the development of scientific thought in the nineteenth century and reinforced by the progress of economic and anthropological studies. The truth seems to lie, as usual, somewhere midway. The artist is not a solitary being, living metaphorically upon a desert island, but is born into a community whose influences he undergoes in common with his fellows. In a sense he does so even more than the others, for a great susceptibility to ideas and the ambient atmosphere and a great power of absorbing them, exceptional both in range and rapidity, are part of his artistic psychological equipment. It is this impressionability which makes him so much more often than his duller brethren the unhappy victim of the herd and, by a natural reaction, into a really solitary person. Yet while *humanly* his contacts with his environment are at least equal to those of his fellows and are usually much more numerous and much less superficial, he is, *quâ artist*, undoubtedly infinitely less subject to social pressure than any other member of society, stands infinitely less in need of social co-operation for the exercise of his calling and is almost independent[1] of society in his artistic creation, development and ideals. As artist, he is a kind of imaginative solipsist. The relation of the artist to the Group appears to me consequently as very peculiar and very complex. It has never to my knowledge been satisfactorily dealt with either psychologically or sociologically. Yet it is of fundamental importance to Aesthetics both from the point of view of the artist's psychology and from that of Genetic Aesthetics which has to take count of the historical fluctuations of this relation for an understanding of the history of Art and the development of the aesthetic consciousness formulated in it. Regarding the special problem now before us, I must confine myself to saying that I believe the so-called 'art-conventions' of different ages which play so important a part in the treatment of media, to be a direct product of this relation.

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[1] This *Journal*, v.

(*a*) In its mere material aspect the medium plays a part in this relation, besides the technical tradition which represents one of the chief social influences upon the mind of the artist. The sheer materialness of a medium such, for example, as stone, its durability and conspicuousness, place upon the architect an artistic responsibility towards his contemporaries and posterity which is not borne by other artists in the same way or to the same extent. Building St Peter's or Westminster Cathedral, building for eternity so to speak, is psychologically a very different proposition to making an etching, composing a song or writing a poem. The permanence or evanescence of a medium invests a work with an atmosphere which reacts upon the psychology both of its creation and its appreciation, and while its *social* importance seems to increase in proportion to the crudeness of the material and the physical labour of its handling, the *individual* preciousness appears to rise to the evanescent tenderness of the human voice or the poignancy of a dramatic scene.—Again the power of experimenting, for instance, evidently decreases with the durability of the material with all the consequences for the psychology of the artist and the technique of the art-form, as in the well-known case of fresco-painting or lithography.—Even the personal relation of the artist to his work is bound to be involved. The poet may destroy his poem, the musician his song, the painter his canvas; they may be forgotten by the public; but building or statue remain to be seen, if not by the artist, yet by the public and become the point at which public feeling impinges upon the self-regarding sentiments of the artist. In a multitude of ways Architecture, Sculpture, Painting, Music, Poetry, Acting and Dancing appear as a series of art-forms in which the individual factor assumes ascendingly greater sway over the social determinations, in the freedom from technical tradition, in artistic responsibility, in public resonance; and the medium as sheer physical material seems to play a not inconsiderable part in the complex situation thus created.

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[1] Apart, of course, from economic considerations.

(*b*) I venture to touch in passing upon a point which affects the relation of mind to medium in its more external aspect, viz., the question whether the artist is distinguished by a special sensory sensitiveness towards the medium, superior to that of normal persons. Do painters possess a greater acuity of vision, sculptors a finer sense of touch, etc.? It would appear doubtful, if we allow for the specialised refinement of discrimination due to interest and practice[1]. The problem is obviously difficult to test, but without actual experiment we shall not be much wiser. It would certainly be well worth while to attempt special research on this point as well as on the superiority which training and practice unquestionably confer upon the artist.

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[1] Heine und Lenz, *Über Farbensehen, besonders der Kunstmaler*, Jena, 1907.

(*c*) On the psychical attitude of the artist to his medium, I had occasion in the course of some experiments on the aesthetic appreciation of colours[1] to make the interesting observation that painters and others professionally interested in colour assumed two entirely different relations to it, according as to whether they viewed it, so to speak, aesthetically or pictorially. As artists they displayed the same attitude which they would assume towards a work of Art; as painters they treated it with a kind of objective detachment as a mere, almost indifferent, instrument. The same dualism of attitude will, I think, be found in all arts. The architect or sculptor will feel a thrill of purely aesthetic enjoyment at the beauty of a finely grained stone and will lovingly caress the surface of a beautiful marble; the literary artist will gloat over the sonorousness and rhythm of a word; but each will, when the occasion arises, treat the same object of his delight as part of a completed whole with apparent indifference and will ruthlessly discard it from an unwanted place. And yet something of the intrinsic aesthetic value of the medium will persist as an almost imperceptible diapason in the full harmony of the work. The indifference of his professional attitude is not the indifference of the heedless layman, but the higher detachment which removes the medium from the grossly practical function of a 'materialisation' of his vision into an integral part of it.

(*d*) The connecting link between the medium and the vision of the artist is *Technique.* The relation described by that term is the adaptation of the medium to the vision, and, *vice versâ*, the adjustment of the vision to the possibilities and exigencies of the medium.

A. (1) Under the first section of this definition we find that Technique consists in the first place in a *knowledge of the medium and of its behaviour.* What is the limit of stress of an arch constructed of a particular material? To what extent will a marble or bronze permit the free detachment of a limb from the body of a statue? What is the range of the human voice? How far will a particular pigment darken in course of time? At the present moment reinforced concrete construction has introduced problems of its own under this heading which are far from solved. Much the greatest part of 'art-education' is concerned with the transmission of the wholly impersonal knowledge of this kind.

(2) As a special part of this knowledge appears the familiarity and practice with the instruments and tools for dealing with the material—a special part, because the invention of new tools has notoriously played

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[1] "The 'perceptive Problem' in the aesthetic appreciation of simple colour-combinations." This *Journal.* III, 444 note.

so important a rôle in the history of Art. It was for that reason that the mythical Daedalids were equally famous as mechanical inventors and as the first artists who "made men to walk," and we need but scan the technical sections of Vasari to realise the profound preoccupation of the Renaissance artists over the mechanical problems presented by the media of their day. A History of Art dealing pre-eminently with the modifications introduced by the invention of new tools and processes—in its repercussion upon the mind of the artists—would fill a gaping lacuna in our present knowledge and understanding of the development of Art, especially in its earlier phases.

(3) Knowledge of the nature and behaviour of the medium and of the functions of the tools for its manipulation will remain a mere impersonal 'knowledge by description' unless and until it is converted into active and personal familiarity by practice. The object of all practice is the acquisition of *manual dexterity*, if I may use 'manual' generically to indicate the muscular and motor-training whether actually of the hand, or of the control of voice, gesture, facial expression or even the remoter accuracy and spontaneity of auditive imagery, whether musical or linguistic. The ideal is "la man che ubbedisce all' intelletto" of Michelangelo, the perfect and minutely correct adjustment of muscular action to impulses of the will and even its unconscious control. This manual dexterity is the first prerequisite of any artist and is, in distinction to mere knowledge, transmissible only in the smallest part. The degree of perfection in the control of muscular movements that can be achieved is well known from the numberless stories told of most of the great artists of the past. Manual dexterity of this kind is one of the main differences distinguishing the artist from the merely appreciative layman. Though apparently nothing more (though often wellnigh incredible at that) than quasi-mechanical tricks—and a good deal of it is constantly mistaken for Art—it is the foundation, practically and psychologically, upon which great Art must necessarily build; practically, because it is the condition of the power over the material; psychologically, because it constitutes a personal knowledge of the limits and the possibilities of the medium which through the formation of technical habits comes to modify the artist's vision itself.

(4) Manual dexterity would be inconceivable without what I should like to call 'technical memory.' What I mean by technical memory is the accurate persistence in memory of the—presumably muscular—adjustments which enable for instance the actor to reproduce, perhaps after years, the same voice-inflection in a particular sentence, or enabled

Giotto on a famous occasion to draw a perfect circle free-hand. I have heard it said that in the last works of Beethoven the use of the human voice occasionally shows signs of a slight hesitation, due to the years of deafness with which he was afflicted. This would be an instance of the weakening of what I mean by technical memory. The existence of a technical memory, apart from its obvious necessity for the development of manual dexterity, is an essential condition to the exercise of technical imagination (see below).

B. If we consider the relation described by 'Technique' from the other, the mind-end, so to speak, we find, I believe, that it consists principally of the *knowledge of the effects* which can be achieved by the handling of a particular medium, a knowledge acquired, perfected and refined, of course, by the combined familiarity with the medium and the practice in its manipulation.

We encounter at this point a problem which I can mention only in passing. It appears in its acutest form in such art-forms as Architecture and Sculpture, though it seems to arise equally in the other arts. What is the difference between Building and Architecture or between a cast taken from a living body and a piece of Sculpture? The matter has been dealt with in the well-known book of A. Hildebrand, *Das Problem der Form in der bildenden Kunst*[1]. The point here is that in Architecture, for instance, the structure, the material form with all its technical problems, is, as it were, merely the substratum of the *spatial* forms which Architecture endeavours to produce and render effective. These two forms, the structural and the spatial forms, are by no means identical. The structural form may at times coincide with the spatial form, but very often the spatial form is quite different from, though produced by, the structural form. A column, structurally, carries a load and is the transmission of a *downward* stress; spatially, it is a *rising* form. Well-known instances are the usual geometrical-optical illusions, and, while Building is the science of structure, Architecture is the art of three-dimensional optical illusions. The architect aims deliberately at the production (or correction of undesirable) optical spatial illusions by means of the structural forms. Hence the fundamental importance of knowing what effects will or will not be produced by particular material forms in particular positions, perspectives, foreshortenings, lights, colours, etc. The same applies, *mutatis mutandis*, to Sculpture and all the other arts.

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[1] Jena, 1905 (5th ed.). There exists an American translation of the book, but the translation introduces an eye-movement theory foreign to the intentions of the original.

This is the point at which the vision of the artist is profoundly affected by the medium, for unless *the vision is conceived in terms of the medium*, it would be almost an abuse of language to call it vision at all. This influence penetrates the vision in its minutest details, in proportion to the perfection of the adaptation between the mind and the medium, i.e. of the technique of the artist. It is a crude but well-known illustration that a statue cannot be conceived in marble and executed in bronze. As has been pointed out by Max Klinger[1], it makes all the difference whether a picture is designed as an etching or as a mezzotint, i.e. whether you work from light to dark or from dark to light. The sculptural vision depends on whether it is to be carried out by modelling or by chiselling—"per via di porre" or "per forza di levare," as Michelangelo called it. You cannot write a play in prose and later turn it into verse without disturbing the whole relief of it, the interdependence of its parts, the colour of its sentiment and in the last resort the fundamental conception from which it sprang.

A failure to achieve this adaptation in the first instance is responsible for the constant corrections and alterations of works in the course of their technical execution, a source of the "développement par transformation" and "développement par déviation" of Paulhan[2]. Similarly the reproduction of works in media other than that for which they were designed is bound to be unsatisfactory and can at best have a merely illustrative value.

Lastly the sketches, notes, fragments and uncompleted works of great artists is the most valuable material at our disposal for the study of this interpenetration of medium and mind and are our best evidence for the rôle played by the medium in the formation of the artist's vision.

Having secured the fact of this interpenetration, we have to raise the question of its mechanism. By far the most illuminating conception here is that of the "images d'interprétation ou de traduction," suggested by L. Arréat[3]. He writes: "Un peintre figuriste, qui ne *sait* pas l'animal, a des chevaux à placer dans une composition. Il s'applique alors à l'étude du cheval; il prend des croquis, et se met dans la tête des 'images.' Un cavalier expert pourra ensuite critiquer son tableau en connaisseur. Il garde donc en mémoire, lui aussi, des images précises, auxquelles il a comparé celles du peintre. Il ne serait pas capable cependant de dessiner un cheval ni de le peindre. A quoi tient précisément cette différence? Il ne suffit pas de dire que c'est faute d'exercice, car la faculté même d'apprendre marque un véritable privilège. C'est d'abord faute d'*images d'interprétation* ou *de traduction*: j'entends par là des schémas visuo-moteurs laissés par l'étude dans le cerveau du peintre, et grâce auxquels sa représentation mentale peut prendre figure aussitôt sur le papier ou la toile—des symboles actifs, en quelque sorte, qui sont comme les idées générales 'pittoresques.'"

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[1] Max Klinger, *Malerei und Zeichnung*, Leipzig, 1907 (5th ed.), p. 34 ff.

[2] F. Paulhan, *Psychologie de l'invention*, Alcan., 1901.

[3] L. Arréat, *Mémoire et Imagination*, Alcan., 1904, pp. 28–29.

The interest and value of this conception appear to me to lie in the following points:

(1) It seems to me to provide a simple and lucid explanation of the process whereby practice and study succeed in developing, in the artist as distinct from the layman, both the wealth of memory-images and the facility for executing them.

(2) It gives a rational account of the fusion of the vision and technique and of the effect of technique upon—in the case of the painter—the visual imagery. For these schematic images or, as it were paradigms, forming the stock of the artist's imagery, have been developed in the course of practice and study by the accumulation of images *framed in terms of his medium.*

(3) It emphasises one of the main functions of art-education, viz. the formation and refinement of such 'images de traduction[1].'

(4) It offers an interesting explanation of what the French call 'la ligne de l'artiste,' the 'manner,' the almost impalpable peculiarities which distinguish the work of one artist from that of another. They would seem to be reducible to differences in their 'images de traduction[2],' their individual 'pictorial general ideas' under which each subsumes his isolated experiences and impressions. As M. Arréat remarks, a failure to develop, multiply and enrich his 'images de traduction' by constant exercise of observation (and especially imagination) leads to a sterile repetition of these images without individual elaboration: the 'manner' turns to 'mannerism.'

(5) The 'images de traduction' of the painter find their obvious analogies in the other arts, and the same psychological mechanism applies equally to them. Certain attitudes, certain turns of phrase, the use of epithets, the technique of character-portraiture, peculiar musical cadences and harmonies, peculiarities of gesture and voice-

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[1] Lecoq de Boisbaudran, *Training of Memory in Art*, Macmillan, 1911.

[2] Ugo Ojetti, the distinguished critic, suggests that the colour-schemes also form part of these 'lignes de l'artiste' (see Corriere della Sera, on the occasion of the 400th anniversary of Raffael's death).

inflection for the interpretation of particular emotional states, certain dramatic types—especially the old types of the Commedia dell' Arte—are instances of such 'images de traduction' in Sculpture, Poetry, Music and Drama.

So far the relation of mind and medium in regard to the artist up to that point at which he begins to develop his own individual art, his own imaginative powers, to become a creative artist and to leave the stage of a mere learner and imitator. Up to this point his accomplishments are not yet truly his own. The greater part are both socially determined and socially transmissible, for even his first 'images de traduction' will be derived from 'objective Art' and will be taken bodily from it, just as our—the laymen's—'pictorial general ideas' are far more extensively inspired by pictures than by reality. The foundations of his personal and individual powers are laid by the development of his manual dexterity and by the first imaginative elaboration of the original stock of his imagery. On the basis of these accomplishments, presenting the first fusion of the vision and the technique demanded by his medium, his creative processes are built up. The imaginative complication and enrichment of his conceptions is accompanied by an imaginative refinement and development of their technical formulation, and the farther his artistic imagination breaks away from the traditional stock of his art and becomes more and more his inner personal creation, the deeper and the more sweeping the changes which the simultaneously growing power of his technical imagination introduces into his handling of the medium. Thus new technical processes, new tools and methods, new ways of achieving effects and new solutions of material difficulties are discovered and minister in their turn to the wealth of his artistic imagination.

At this point we run up against perhaps the most formidable and fundamental problem of Aesthetics, that of the artistic imagination. In spite of much writing on the subject, we can hardly be said to have advanced beyond the fringe of the unknown. The chief obstacles to progress seem to me to have been a long standing confusion of imagery with imagination and the attempt to deal with imagination on far too narrowly intellectualistic a conception of novelty and originality as a characteristic feature of imagination. Imagination is clearly different from and in contrast to normal consciousness, but at the same time overlapping it to a large extent. The imaginative experience such as the artist in particular lives in, is more nearly the imaginative counterpart of actual experience than imaginings wholly outside the range of normal experience—which is true rather of people generally called 'unimaginative.' The novelty and originality of the artistic imagination lies far less in its excentricity to normal life than in its being the reflexion of an intensely and intimately individual experience, transferred to the sphere of imagination, thereby removed from its personal reference and rendered accessible to and effective for the sympathy, understanding and appreciation of others. This curiously dualistic, yet unified psychosis I have attempted to render intelligible as 'distancing[1].' And the medium, its treatment, its very limitations and the fusion with the artist's vision in Technique is one of the chief factors of artistic creation forcing the distancing process upon the artist while at the same time it facilitates the maintenance of distance.

IV. Concerning the relation of mind and medium in *aesthetic appreciation* little need be said. If the principle be accepted that, ideally, appreciation is a re-creation of the work in the mind of the recipient, it follows that the relation in his mind should not differ widely from that in the mind of the artist. The qualification implied in the term '*re*-creation' must not be lost sight of. For I confess that I disagree with the pleasant theory that "all men are artists." If we agree upon calling 'artists' those capable of creating Art, the best that we can say for the non-artist is that he is capable of following in appreciation the creative process of the artist, and his capacity even in this respect is notoriously limited.

Again, if, as I believe, the attitude to medium and its technique forms an integral part in the creation of a work, it may be contended that perfect appreciation should also be conscious and appreciative of this function. I cannot therefore quite follow Mr Watt in his statement about "the great and spontaneous joy that the greatest art gives even to the inexperienced provided they are 'pure in heart.'" Every appreciation of whatever kind requires a certain experience and education in the matter to be appreciated. I do not, of course, mean a reasoned judgment about it. But I am a little suspicious about the purity in heart. It savours to me of Ruskin and Tolstoy and suggests an uncritical admission into the effect of a work, as aesthetic enjoyment, of all kinds of pleasant impressions which may have nothing whatever to do with it.

I readily admit that you may aesthetically appreciate a work without knowing anything about its technical aspects. But I cannot persuade myself that an appreciation of this side would not materially enhance the appreciation of the whole. To revert to the example of Architecture: an appreciation of the spatial forms is not necessarily dependent upon an appreciation of its structural excellence, as has been and is still contended by Viollet-le-Duc, Ruskin and their 'veristic,' moralistic and intellectualistic followers[1]. Structural excellence as such has nothing to do with aesthetic effect. What matters is the *relation* of structure to the space-forms, their subservience to spatial effects and the use made of them to achieve these effects; and I am convinced that an understanding of this relation will considerably increase and deepen the aesthetic appreciation of the whole, not least because it surmounts the dualism of these two sets of factors. But I equally admit the danger of technical knowledge for the layman and this is, I believe, what Mr Watt alludes to when he demands "purity in heart" of him. The medium and its handling have tremendous attractions for the layman, as all technical procedures have. The desire to *know* "how it is done" and all the unholy curiosity stirred by the relative ease of satisfying this desire by a purely intellectual knowledge about it are destructive of all aesthetic appreciation, unless they can be kept subordinate to the attitude of contemplation. The layman has generally neither the respect which the artist naturally feels towards his art, nor his consciousness of the true function of technique, nor his imaginative and emotional control. And it is, after all, so much easier to understand than to appreciate that it is little wonder if in the layman's mind technical knowledge takes precedence over aesthetic appreciation and thereby breaks up the unity of the aesthetic state. But this failure is no argument against the assertion that real appreciation of technique should enter into the total effect of the work with all the enhancement it carries with it, just as it entered into the creative act of the artist.

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[1] Edward Bullough, "Psychical Distance as a Factor in Art and an aesthetic Principle." This *Journal*, v, 87 ff.

V. In conclusion I offer a few detached observations on the relation of mind and medium from the point of view of Genetic Aesthetics.

Genetic Aesthetics is the necessary complement to any psychological formulation of aesthetic facts. Psychological Aesthetics can give us an account only of what *we*, at the present time and with our largely local experience, mean by 'Art,' aesthetic appreciation and artistic creation, since only our own experiences are directly and to some extent introspectively accessible to us. What the Renaissance, the Middle Ages, the Greeks, Egyptians, Chinese, or the primitive peoples meant when they spoke (many never spoke) of Art is ascertainable only indirectly, on their own critical and theoretic statements[1]. Their works are no unambiguous testimony, doubly obscured as their evidence is by the confusing interpretations on our own analogies through which we have been accustomed to see them. That 'Art' has not, in its history, retained the same significance is beyond doubt. How far was the art-experience of the Greeks, Egyptians or Chinese the same as ours? How has art-experience in general come to be what it is? That is the problem of Genetic Aesthetics. No principle of Aesthetics can, I believe, be regarded as valid, which is not itself capable of evolution and thereby avoids the fallacy of applying to past periods of Art and human society terms which can have meaning only in reference to our present conceptions.

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[1] This point has been admirably dealt with under the name of "intellectual and moral fallacies" by Mr Geoffrey Scott in his *Architecture of Humanism.*

A. As regards Art in the objective sense, it seems to me that the importance and prominence of the material and of the technical tradition have noticeably decreased.

The material itself clearly plays a very considerable part in the estimation placed upon art-objects (if they can always be so called) in primitive 'Art' and much later, even latest, periods of Art-history. The value attaches apparently either to the intrinsic preciousness of the material—something of the kind survives in some of the crafts such as gem-cutting and work in precious metals—or to its magical efficiency, or to the mere difficulty of working it. None of these criteria are in our sense aesthetic, but entered, it seems, largely into the appreciation of former 'Art.' The extreme hardness of Egyptian porphyry probably added much to the value of works executed in it and the reference to it by Vasari[2] suggests that the difficulties of working it even in his day contributed to the estimation of this medium. White jade was reserved in ancient China for the exclusive use of the emperor, on account partly of its rarity, partly of its magical associations, and came to be regarded as the most precious kind of this highly prized material. The chryselephantine statuary of the Greeks must have enjoyed the reputation it had for similar reasons, for we cannot, I think, avoid the suspicion that artistically it must have been an error of judgment.

Technical tradition was notoriously rigid at certain periods to an extent which we would consider inimical to the production of anything that we should call Art. The reasons are very multifarious and cannot be dealt with here. Stereotyped patterns—stereotyped even so far as to be transferred to media intrinsically unsuitable to them—stereotyped arrangements of colours, of formulas, phrases, cadences, etc., are too well known to be more than mentioned. Ecclesiastical and any kind of hieratic influence has always been responsible for such an unbending maintenance of technical tradition, originally no doubt from magical motives. Wherever present, such motives inspired the worker with a quasi-religious awe and helped to prevent the mergence of the medium in his own conceptions, provided he were allowed to entertain such at all. Few artistic phenomena are in this respect more illuminating than Russian Ikon-painting with its rigid adherence to traditional attitudes, subjects, apparel, facial expressions, colours and media. The consequences of convention went so far as to develop a most carefully observed division of labour in the painting of Ikons: one painter specialised in gilding, another in painting dresses, another in backgrounds, another in faces, etc.[1] This specialisation carried with it a technical training of the most exclusive kind. Comparable to it, and in addition permeated with magical motives, is the initiation which was customary among the Bushmen for the training of their painters: apparently the preparation of colours was a magical guild-secret like the preparation of poisons in the same tribe[2]. A similar institution has been suggested by the Abbé Breuil in connexion with the palaeolithic paintings in the Altamira Caves[3].

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[1] This constitutes the chief interest and value of past literary and art criticism.

[2] *Vasari on Technique* (ed. with introduction and notes by G. Baldwin Brown), Dent, 1907, pp. 26 ff.

The medieval guilds—including such as the Meistersingers—though more definitely economic and artistic in essence and object, betray the same preoccupation in the maintenance of technical tradition.

B. In the development of Art from the dynamic point of view, i.e. in the history of creation and appreciation the outstanding fact is the differentiation, first, of the craftsman from the public, and later the differentiation of the artist from the craftsman.

The differentiation of the craftsman from the public is in itself a very complex process which, however, even in primitive societies appears to have at least begun, curiously enough, it seems, especially in connexion with music. I mention it as a peculiarly important development for the question before us, inasmuch as it marks the transition from manual dexterity as a *personal* qualification to dexterity as a *professional* qualification of the craftsman. The change is the outward reflexion of a psychological shifting to a point of view which is a pre-requisite to an aesthetic attitude. Once the distinction of the craftsman is secured, several other social distinctions develop of which the above mentioned guild-formations with their consequences for technical tradition and teaching are only one.

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[1] В. Н. Успенскій: Очерки по исторіи иконописанія. СПЪ. The painters who dealt exclusively with dresses, had the delightful technical name of "Доличные" (up-to-the-facers), p. 15.

[2] G. W. Stow, *The native races of South Africa*, 1905, p. 76.

[3] Cartailhac et Breuil, *La Caverne d'Altamira*, Monaco, 1906, p. 135.

The differentiation between the craftsman and the artist is one of the characteristic features of Art since the Renaissance. Indeed, the Greeks appear just to have reached this same point in the development of their social and aesthetic culture, when its course was interrupted by political collapse. The stage was reached again in the fifteenth century. It is, of course, true that Art has never recruited its devotees wholly from the artisan class. Both poets and to a lesser extent musicians were drawn chiefly from other sources, and it is one of the distinguishing traits of Chinese pictorial art that most of its representatives were men of letters rather than craftsmen, a fact which is reflected in the close connexion between painting and calligraphy both in China and Japan with far reaching results for the technique and aesthetics of Chinese and Japanese painting[1].

The shifting of the social status of the executant from craftsman to artist is again merely the outward symptom of a much deeper and more fundamental alteration in the relation to Art of the Middle Ages and modern times respectively. The decay of the apprenticeship-system, the rise of a new Art-education, signalised by the foundation of academies, the removal of Art from its fixed social and economic functions in medieval society to the exercise of a new, economically indetermined and ideally wholly unfettered vocation are collateral phenomena of this profound change. Psychologically and aesthetically, the nucleus of the new situation is a new conception of Art which since the Renaissance has become the basis for our modern attitude to it. Nothing could be stranger to this modern idea than the position of the medieval craftsman working to specifications of such minuteness as some of the contracts for pictures reveal, as for instance that for the Coronation of the Virgin by Enguerrand Charonton[2]. Nor could any contrast be more striking than that of a comparison between for example the *Schedula diversarum artium* of the Monk Theophilus (probably eleventh century) and the first manifesto of the modern conception: the *Della Pittura* of Leon Battista Alberti, of 1436; especially when you compare the Prologue of

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[1] R. Petrucci, *Le Kie Tseu Yuan Houa Tchouan*, *T'oung-pao*, XIII, p. 82 note; also Sei-Ichi Taki, *Three Essays on Oriental Painting*, 1910, p. 25 ff.

[2] See Thomas Okey, *History of Avignon* (Medieval Town Series), pp. 375–379.

Theophilus with the famous opening of Alberti's third book[1]. As an illuminating illustration of this new conception relevant to the present discussion, I quote Alberti's condemnation of the use of gold in paintings, from the end of the second book of his treatise:

> Truovasi chi adopera molto in sue storie oro, che stima porga maësta; non lo lodo. Et benchè dipigniesse quella Didone di Vergilio, ad cui era la pharetra d' oro, i capelli aurei nodati in oro, et la vesta purpurea cinta pur d' oro, i freni al cavallo et ogni cosa d' oro; non però ivi vorrei, punto adoperassi oro però che nei colori imitando i razzi del oro, stà più admiratione et lode al artefice[2].

The importance of the passage lies, I submit, in the fact that it records the definite loss of the intrinsic value of the medium and the renunciation of the effects of mere material preciousness in the interest of a purely aesthetic homogeneity of style.

By the end of the fifteenth century the Art of the Italian Renaissance had triumphed over the difficulties of its media and it is significant that one of its greatest representatives, Leonardo, allowed himself to be carried away by his enthusiasm for the rendering of light and of shadows so far as to remark:

> Sola la pittura si rende (cosa maravigliosa) ai contemplatori di quella per far parere rilevato e spiccato dai muri quel che non lo è, ed *i colori sol fanno onore ai maestri che li fanno, perchè in loro non si causa altra maraviglia che bellezza, la quale bellezza non è virtu del pittore, ma di quello che li ha generati*...[3].

Since then the commercial production of media has still further contributed to the loss of their practical importance. The modern painter no longer grinds his own colours and spends years in experiments with different pigments and varnishes. The present-day sculptor orders the block of marble he wants; he no longer selects and even quarries it himself as Michelangelo did. As likely as not he will leave the transfer of his model to marble or bronze to a firm of contractors.

It is true that there are two media which cannot be standardised, commercialised, contracted for. They have consequently never lost and never will lose the peculiar position which the uniqueness of a medium secures for it in artistic creation and appreciation. They are the human voice, and the human body as the vehicle of its soul. Singing and acting are the two arts which achieved their distinctive aesthetic value, the former almost from the beginnings, the latter relatively very early in the history of Art.

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[1] A concise and useful summary on several of these points will be found in the Introductory Essay of G. Baldwin Brown's edition of *Vasari on Technique*, Dent, 1907. See also the French edition of *Schedula div. art.* by Ch. de l'Escalopier with an introduction by J. M. Guichard, Paris, 1843.

[2] P. 139 of the *Della Pittura* in the edition by H. Janitschek (*Quellenschr. für Kunstgeschichte*), 1877.

[3] Quoted from the "Trattato della Pittura" in Lionello Venturi, *La Critica e l' Arte di Leonardo da Vinci*, Zanichelli, no date, but 1919.

That the growth of mechanical aids constitutes a great danger to Art which, I venture to think, has not failed to make itself felt in our times, will probably be admitted. All aesthetic psychoses are very complex states poised in a highly unstable equilibrium. This applies to the individual attitude as much as to that of whole periods, and aesthetically the functions of the medium, though subordinate to the mind of the artist, cannot be decreased beyond a certain point without the disturbance of this balance, without emasculating imagination, allowing dilettantism to usurp the place of art, and ultimately without the loss of the most precious aesthetic quality of a work: its intrinsic style.

MIND AND MEDIUM IN ART[1]. (V.)

By C. W. VALENTINE.

THE best known theories of the beautiful seem to err in the following respects: they take one aspect of an aesthetic experience, or one or more characteristics of certain selected beautiful objects, give quite convincing illustrations of these isolated factors, and then generalise as to beauty or the aesthetic experience. Very varied theories can easily arise thus; for so complex are aesthetic experiences that many varied elements may have a share in such experiences. Still more readily can we generalise if we limit our field of discussion to one of the arts.

The error mentioned above is exemplified in the theory that beauty is "the sensible appearance of the idea." True a beautiful object is often the expression of an idea: possibly it always is if we include under the term idea those 'images de traduction' to which Mr Bullough refers. But the difficulty is that beauty is usually more than the sensible expression of an idea, and that many sensible expressions of an idea, even if true, are not beautiful.

So with Croce's theory. That beauty is in some way or other an expression is doubtless true. But the theory is incomplete, as I have tried to show elsewhere[2]. The words 'intuition' or 'expression to self' (rather than to others) may cover experiences that are unworthy of being called aesthetic, though undoubtedly they do also describe one aspect of many aesthetic experiences and thus all experiences which they cover have something in common with aesthetic experience, which fact lends plausibility to the theory.

In his most interesting paper Mr Marriott has shown the importance, more especially in plastic art, of the characteristic use of the material—an aspect of art to which probably too little attention has hitherto been paid. Valuable, however, as this contribution is, Mr Marriott's thesis seems to me to err in the way described above. There is no doubt that the using of materials in a manner which is in some way suitable to their nature, is one condition of beauty in an object to those who know the nature of the material. It is still more generally true that the using of a material in a way unsuitable to its nature may spoil the object: that is, Mr Marriott's formula has wider validity in a negative form. Indeed when faced with the objection that "we can enjoy works of art without knowing how they are done," Mr Marriott himself adopts the negative form and replies that directly the means are used out of character we do become conscious of them, whether we understand their characteristic use or not.

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[1] A contribution to the Symposium presented at the Congress of Philosophy in Oxford, 24–27 September, 1920.

[2] In a critical notice of E. P. Carritt's *Theory of Beauty* in *Mind*, 1918.

Hence to Mr Marriott's assertion that "practical and aesthetic reasons are at bottom the same thing," I would reply, "No: but suggestion of practical difficulties may spoil aesthetic appreciation." And even this is too simple a statement. For Mr Marriott's quotation from Ruskin that "In every branch of art, only so much imitation of nature is to be admitted as is consistent with the ease of the workman and the capacities of the material" must be read in conjunction with Ruskin's own doctrine that the sign of infinite painstaking, when there is adequate return, gives us one of the Seven Lamps of Architecture.

In Mr Marriott's theory there is merely one element essential to the very complex aesthetic experience. And even in considering that element he lays perhaps too much stress upon the nature of the material itself and too little upon the nature of the man who sees it. "The laws of painting," says Mr Marriott, "are as surely derived from the nature of paint and brushes as the laws of cricket are derived from the nature of bat and ball." But the laws of cricket are equally determined by the nature of man. If men acquired the power of revolving the arm ten times as fast as they can at present (without otherwise changing), either the laws of cricket or the nature of the bat and ball themselves would have to be changed. In his final paragraph, however, Mr Marriott modifies his position by applying to art Reynolds's saying about nature which comprehends under nature the "internal fabric and organisation of the human mind and imagination" and only *adding* to these words "the nature of the tools and materials used by man" in order to get a complete conception of art.

Mr Walkley criticizes Mr Marriott for externalisation, and by inquiring within leads us to Croce's Expressionism. This again, in my view, leaves us with one aspect only of the aesthetic experience. In so far as I understand Croce's 'intuition' and 'expression to self' I agree that the aesthetic experience is always of the nature of intuition: it certainly is not reasoning, or an activity devoted towards a practical end. But the term 'contemplation' or 'immediate apprehension' seems to me equally true (and inadequate). The whole world of objects and ideas may be objects of intuition equally. Croce's expressionism, so far as I can find, cannot show why one object is beautiful to us and another not, even when an object of intuition. Here again then we have a case of one quality of the aesthetic experience being lifted into the position of the sole characteristic of beauty. The ugly for Croce is incomplete expression. But what fails to be expressed in some ugly things is not clear. And why does the alteration, by a mere shade, of the tone of one colour in a design spoil the beauty? What fails to be expressed here? True I find by introspection that I can adopt, if I will, towards almost anything (e.g. even the scribble in which I am writing these words) an attitude which appears to be identical with Croce's 'intuition,' though Bullough's word 'distance' seems to me to express it better. But if we are to call *every* such mental state aesthetic, then we need quite other words for that warm experience in which we really enjoy the beautiful. I accept Croce's dictum that all aesthetic experience involves 'expression' in this sense; but not that all expression is aesthetic.

The whole question as to what is beauty, or what is the aesthetic experience, has been unnecessarily complicated, in my opinion, by the undue attention given to the activity of the artist. If it be granted that we can have a truly aesthetic experience, without ourselves creating (externally) an object of beauty, it seems unnecessary to introduce at first a discussion of the artist's creative activity in our search for a theory of the beautiful. At least it gives us an additional problem to solve, and in facing it there is usually a failure to recognise the extreme complexity of the mental processes involved. It has seemed paradoxical to deny the term 'aesthetic' to the artist's creative activity, and hence the revolt of some against Croce's description of the painter's painting as mere externalisation and practical activity. The truth seems to be that we may have in some moments of the artist's working a blend of the aesthetic with the attitude of the craftsman. In the case of the real artist the aesthetic experience is so intense, his creative imagination 'expresses to himself'—to use Crocean language—so vividly, that it endures even while he seeks to depict it with a stroke of the brush; or at least the aesthetic attitude is only momentarily effaced. This seems to be still more true of the musician extemporising at the piano. He feels for the chord to express his mind's anticipation and demand, and the technical activity is so simple and easy to him at times, that it involves no interference with the pure aesthetic enjoyment. Probably also even when the externalising activity is more prominent, it is, if successful, so enjoyable to the artist that it readily blends with the more essential aesthetic elements.

The artist's creative activity, then, and through this the influence of the medium, may enter into the aesthetic experience of the artist to a very varying degree. When the medium is relatively unfamiliar, and the exercise of technical skill becomes prominent in the whole process, the aesthetic element falls into the background and may for a time disappear. When the technique is very familiar (as in the case of an expert musician), the interference with pure aesthetic enjoyment becomes infinitesimal and the enjoyment of successful creation or interpretation adds an element of satisfaction to the whole experience which may be almost indistinguishable introspectively from the appreciation of the music itself. I should then agree or disagree with Croce's absolute distinction of the externalisation of art from aesthetic experience, according to the nature of the case as indicated above.

At this stage I may illustrate the point of complexity on which I have been laying stress, and here let us leave the artist for a moment to consider only the appreciation of a picture. In this many elements may contribute to the total enjoyment of the picture. In the course of investigations on the appreciation of pictures by large numbers of adults, it has been to me amazing to note the very different degrees to which the various elements enumerated are of influence in determining whether the picture is regarded as beautiful or not by the subject. In a given picture the following aspects may all contribute to a person's enjoyment of it—the colours used, their inherent pleasingness and their suitability for the object depicted; the individual forms and shapes; the composition or arrangement of a picture—in a good picture, as Ruskin says, "everything helps everything else"; the degree of novelty or familiarity; the significance of the picture; any associations, both conscious and subconscious, which may inevitably cling to it; its 'truth'; the perception, possibly only subconscious, of the artist's skill; and so on. Each of these factors varies in its influence according to the individual and of course according to the picture concerned. Indeed they vary according to the mood and mental attitude of the subject at the time. A mere difference in tone in the reproduction of the picture may spoil its beauty for us, a mere trivial association may subconsciously determine our attitude. The individual experience of the person concerned may make or mar the aesthetic appreciation. The Virgin Consolatrix—beautiful to one of my subjects before she had a child of her own—was intolerable to her after the birth of her child as the significance for her of the picture had been modified. In some persons the consciousness of the artist's skill contributes much more to the enjoyment of the picture than is the case with others. I believe it is especially important in the appreciation of, say, a fine portrait of an ugly person; but with many persons this aspect cannot be relied upon, and even the finest portraiture of an ugly person cannot be appreciated by them. The results of careful introspective records in the appreciation of pictures lead one to a profound conviction that the factors involved in aesthetic appreciation vary greatly according to the individuals concerned, and the same applies to the appreciation of the *elements* of music and pictorial art, as is shown in experiments with colours, lines and musical intervals. What seems to be essential for aesthetic appreciation is that some or all of the various elements should hold us entranced. This principle at least seems fundamental in aesthetic appreciation, namely that there must be at the same time stimulation and facilitation of attention[1]. "If the aesthetic experience is to be enjoyed to the full, there must be no rude interruption in the course of the mental apprehension of the object. Adequate change or complexity to maintain interest and to stimulate mental activity to a certain degree there must be. But as in a curved line there must be no sudden change entirely inconsistent with what the line as a whole pretends to be, so in a picture there must be no element which has no connection with the picture as a whole. That is the negative side of the principle. Stated positively it is that in the apprehension of a beautiful curve one part of it gives us a clue to the nature of the rest; in a good picture 'everything helps everything else.'" "In music, when the rhythm is simple, variety is supplied by the movements in the melody and the changing harmonies. In some Malay music, with an absence of melody and harmony, change is supplied by much more varied rhythms. And as in the apprehension of a beautiful curve one part gives us a clue to the nature of the rest, so the apprehension of the rhythmic form of a piece of music enables us to receive the rest of the music in a mental attitude of repose which would be impossible if the notes followed in irregular order, or if a new rhythm appeared every few bars. Music, thus, in a very marked degree satisfies the formula which one writer has offered as the definition of aesthetic experience—namely, the combining at the same time of stimulation with repose."[2]

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[1] In the case of the artist, and sometimes with others, the mind may create for itself the stimulating imaginative object.

[2] Quoted, with some verbal alterations, from the writer's *Experimental Psychology and Beauty*, 2nd edit. pp. 97 and 123.

Seen in the light of introspective records of aesthetic appreciation, the question as to whether the medium is used in a fitting or unfitting way becomes only one element among many which contribute to the total aesthetic effect; and usually, I should suppose, its influence is of that negative type to which Mr Marriott himself ultimately seems to refer it. This will especially be the case, of course, when the appreciator is relatively ignorant of the nature of the medium and of its capacities. The sight of a pastel drawing made to look like a water colour may please the uninitiated but will worry the expert in pastels. Expert knowledge of means is not only unnecessary for aesthetic appreciation, but it may cause at times a descent from the true aesthetic attitude to that of a technical expert—a cold judgment. Mr Bullough criticizes Dr Watt for asserting that a knowledge of means is unnecessary for the full appreciation of music. If by full appreciation is meant the most intense and enjoyable aesthetic experience under the influence of music, I agree with Dr Watt. But if we take the enjoyment as a whole of music, I should say that knowledge of all kinds about music (not only of means, but also of composers), by making our interest more complex, makes it possible for it to be more sustained. At least it affords moments of change from the maintainance of the pure aesthetic attitude, and so brings that back later with renewed intensity; witness the more rapid fatigue or boredom, in a long concert, of the layman as compared with the expert, though the latter may be the first to drop out of the aesthetic and into the critical attitude for a time. Furthermore, in a subconscious way, as I try to show later, the thoughts of method, skill and technique may actually coincide with a properly aesthetic experience and make it more complex.

The phrase quoted above—"stimulation with repose"—is not given as a complete description of the aesthetic experience. It covers, I think, too wide a ground. In reading an interesting yet easy book of history or of science, we may approximate to a state of 'stimulation with repose.' Yet even here we often have a state which in one important aspect resembles the full 'aesthesis,' namely, that aspect which seems to be covered by Croce's 'intuition.' But it is only the dry bones of full aesthetic enjoyment, the transition to which one may note in the variations of mood which come in reading the more argumentative passages, followed by the finest literary passages, of a great historian. Here, for the aesthetic experience to be enjoyed, we must have a contemplative and not critical or even approvingly reasoning attitude towards the subject matter. And so in the case of poetry, even for pure aesthetic appreciation it is not necessary, as Mr Marriott suggests, that the "dictionary meaning of words" must be excluded. True the appeal of the word-sounds themselves is much. But significance adds to the richness of the experience and carries us along over intervals in which the beauty of mere words fails. The old dispute as to whether substance or form is the more important in poetry, and the assertion that the poet must not try to enforce his philosophy upon us may be answered thus: that when a poet philosophizes—as is done even in some of the finest passages of literature—the reader, if he is to enjoy it aesthetically as literature, must accept, if only for the moment, the poet's point of view, so that he can simply apprehend or contemplate the philosophy and submit himself to the hypnotic influence of the poet.

And so in painting: as Dr Watt says "The arts of vision in the greatest creators have known no limitation to the purely sensuous range of data." Yes, significance counts. But it must not carry us far away from the sensuous presentation itself. The unity of the two must be very close. Or if it does lead us away, the subject matter must itself be capable of becoming a new object of aesthetic appreciation. Hence the difference between a picture which 'tells a story' of trivial moment, and one which depicts an event which itself, even as a thought, can more readily rouse the aesthetic attitude.

In conclusion I may try to sum up the main conclusion of this Symposium, though in doing so I must, I fear, give inadequate weight to the wealth of detail in the papers by Dr Watt and Mr Bullough. All Mr Marriott's successors would, I think, agree with him that in some sense an appropriate use of materials is one element that goes to make a work of art. They would maintain, however, that it is impossible to make it the sole criterion of art, as indeed Mr Marriott himself seems to admit in some passages. That the perception of beauty is dependent on the mind as well as on the object is an old platitude. Mr Walkley goes further, in admitting, with Croce, that it exists in its fullest degree in the mind of the artist before the beautiful material object is created, and apparently the mere appreciator of art must be capable of this artistic 'expression to self,' if he is to have the true aesthetic experience. This view seems to minimise the direct appeal to our feelings of sensory elements (e.g. sounds and colours), which I believe to be of essential importance in most works of art, especially in music and the visual arts.

Mr Bullough approaches to Croce's position in his assertion that even abstract thought can be the object of aesthetic appreciation, and I imagine that here he probably means something like that which I have described as the receptive contemplation of a philosophical idea, a 'simple apprehension' (in Stout's sense) as applied to an idea, instead of to a material object; and I imagine that Mr Bullough would agree with me in finding in this aspect of mental process one common trait of all aesthetic experience, however varying their sensory elements or significance may be. But I go further than Croce (and, I suppose, than Mr Walkley) would go, by asserting that often when aesthetic appreciation occurs, sense stimulation from without is absolutely essential—that is when the appreciator is quite incapable of such aesthetic 'expression' if it is not provided for him from without. He has the true aesthetic appreciation and yet is not capable of creating it, as Croce seems to teach.

Hence the medium is not only a real medium of communication between the artist's mind and the appreciator, it is a means of producing for the latter the aesthetic experience. Anything in the nature of active 'expression to self' is absent, I believe, in much appreciation of music, especially in moments in which we yield ourselves completely to the impact of the sounds. It is absent, too, sometimes in the appreciation of pictorial art, especially when pure colour is the dominant factor. It is more usually present I think in poetry. But to say that "nothing which we cannot clearly express to ourselves by actual or imagined sound, colour, word or shape or in some other way is beautiful" is, if I judge from my own experience, utterly wrong; yet I think it is a fair statement of Croce's view[1]. And even for the artist, I can scarcely believe that the richest experience is always in the creative imagination; but rather that normally, especially in visual art and in music, the artist embodies his imaginations in a medium not only to communicate them to others, as Mr Clutton Brock holds, but to make fuller and more intense his own aesthetic enjoyment. The testimony even of some poets goes to show that the impulse to write is largely dependent on a craving to enjoy more fully than can be done in mere imagination that which they cannot find in real life. "If we had life," wrote Wagner, "we should have needed no art."

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[1] The quotation is from E. P. Carritt's exposition of Croce's views in *The Theory of Beauty*, p. 182.

IS THINKING MERELY THE ACTION OF LANGUAGE MECHANISMS[1]? (I.)

By F. C. BARTLETT and E. M. SMITH.

1. *Statement of the problem.*
2. *The distinction between vocal and language habits.*
3. *The distinction between explicit and implicit language.*
4. *Thinking considered as a form of expression.*
5. *Thinking and its conditions.*

1. Statement of the Problem.

Since this Symposium is intended to take its starting point from Prof. J. B. Watson's recent discussion of "Explicit and Implicit Language Habits"[2], we will begin with a very brief statement of what seem to us to be the main points of his argument:

> 1. Thought is nothing more than a highly integrated bodily activity (page 325), chiefly identified with the action of language mechanisms (page 316); but not all thought is "laryngeal" (page 324).
>
> 2. Vocal habits are distinguished from true language habits, the former being mere repetitions of sounds, and the latter being vocal habits "associated with arm, hand and leg activities and substitutable for them" (page 319).
>
> 3. In general, language habits undergo development from overt to whispered and implicit expression: thought may be reasonably assumed always to involve implicit expression (pages 323–4).

We have to ask whether propositions 2 and 3 justify proposition 1. Let us consider one of Prof. Watson's own illustrations: "When we study implicit bodily processes we are studying *thought*; just as when we study the way a golfer stands in addressing his ball and swinging his club we are studying *golf*" (page 326). But to say that we are studying 'golf' in the second case is to assume that 'golf'—the structure and character of the game itself—is identical with how a given player *plays* golf. Similarly Prof. Watson assumes that 'thought' is, for scientific psychology, identical with its expression. This involves the central assumption of Prof. Watson's system, and raises the following questions:

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[1] A contribution to the Symposium presented at the Congress of Philosophy in Oxford, 24–27 September, 1920.

[2] See his *Psychology from the Standpoint of a Behaviorist*, 1919, chapter IX.

(i) Should the psychological study of thinking be confined to a determination of the varieties of thought responses in relation to their conditions?

(ii) Assuming this to be the case, can Prof. Watson's solution of the problem be regarded as satisfactory?

The second is the narrower problem and will be attacked first.

2. THE DISTINCTION BETWEEN VOCAL AND LANGUAGE HABITS.

This distinction emphasizes the fact that from the first the use of language is bound up with the substitution of one mode of response for other modes.

That both language and thinking do, in a sense, involve substitution is obvious. Both are acquirements gained during the life-time of the individual, and represent modes of response to a situation which, in the course of development, may replace earlier types of reaction. But substitution is an ambiguous word. The *mere* replacement of one set of movements by another is certainly not enough to constitute a language habit. For if it were, every conditioned reflex, for example, would be a language expression. According to Prof. Watson himself the "conditioned reflex level of functioning" gives us, at most, vocal habit; for the "true language habit" we must have this, "*plus*[1] later associative connection of the word when learned with the bodily habits connected with the object for which the word stands" (page 320). Precisely what "later associative connection" means, in this context, Prof. Watson never attempts to say.

Bearing in mind that, for the present, we are not questioning Prof. Watson's general psychological standpoint, we may attempt to work out what is really involved in the idea of substitution.

The consideration that language represents a mode of response to a situation to which earlier adjustment has been made in a different manner, will not carry us far. Not only must there be a capability of substitution, but the substituted modes of adjustment must definitely function as 'signs.'

Now if we stop short at substitution, the new modes of adjustment might be regarded as 'standing for' those alone which they have replaced. But signs involve a necessary reference to consequent adjustments, as well as a *possible* reference to those modes of responses which their present expression has replaced. A sign may be defined, from the present point of view, as any mode of reaction which is succeeded, within the total adjustment to the same situation, by other specific modes of reaction. The sign, that is to say, 'leads on to' a further series of responses, where 'leads on to' may be taken to mean 'is in the majority of cases found to be accompanied by.'

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[1] Italics ours.

But many modes of response which do not come under the head of speech are certainly signs, and, whether they are to be regarded as language or not, do not serve specifically as instruments of thinking. Any series of responses whatsoever, which are arranged in a relatively definite order, constitutes a series of signs in this sense. For example, the adjustments made by an engine driver in becoming aware of a held-up signal are signs for a further series of adjustments made by him in putting on brakes.

At this stage a further point should become clear. Supposing we specify language as a series of substitute signs; all signs are not of the same nature, and do not fulfil the same function. The most important consideration which follows is that, while some signs may have place in series in which the other adjustments are made by response of the 'special sense' organs, or of the larger muscles of the body, others lead to forms of expression for which the organs involved in the production of words alone appear to be entirely adequate. Thus to specify language as a substitution form, or even as substitute-sign forms, is inadequate.

3. THE DISTINCTION BETWEEN EXPLICIT AND IMPLICIT LANGUAGE.

Can Prof. Watson's treatment of the development of implicit language be regarded as any more complete or satisfactory than his specification of the character of the language response? He suggests that, in its growth, language makes "transition from overt to whispered and then to implicit" expression. This is not regarded as strict chronology however, for all three forms may "go on together from the start" (page 322). The two important points are, first, that practically all developed forms of language expression include responses which are of the implicit order; and, second, that particularly when we begin to study implicit expressions we are reminded that many other bodily processes besides movements of the vocal musculature may take part in expressing language (page 274).

Implicit language is simply language expression of an extremely "abbreviated, short-circuited and economized" type (page 323) and the movements it involves are excessively difficult to observe, to recognise, and to interpret. That there certainly is a tendency towards ever more and more abbreviated forms of language expression we believe every one will be prepared to agree, and also that the thinking response frequently expresses itself in these abbreviated forms.

Moreover, no doubt Prof. Watson's claim that many other bodily processes besides the vocal may play a part in the expression of thought is sound. Their occurrence is demonstrable in many instances, and probable in all. Particularly when it is remembered that Prof. Watson clearly states that he has no desire to "abstract language overt or implicit, or other implicit thought processes, from their general setting in bodily integration as a whole" (page 324), it is perfectly clear that the total thinking reaction must probably in all cases involve other than merely vocal movements. Prof. Watson candidly admits that the evidence for the existence, and function, of these 'implicit' processes is extremely incomplete (pages 326–7). For our part, however, we are willing to agree both that processes of the kind described accompany most, or perhaps all, thinking, and that their development is of importance.

4. THINKING CONSIDERED AS A FORM OF EXPRESSION.

We have already suggested there is no valid reason for holding that *all* employment of substitute signs involves thinking. A movement, or set of movements, present at any stage of the performance of a well-established habit is, from the point of view of expression, a sign, or set of signs. Both itself and subsequent movements belonging to the habit are likely to be abbreviated, and hence in a definite sense to be substituted for an original response, which was more complex and may have been different in nature. Yet we cannot agree that the simple unrolling of a habit is rightly termed thinking. Habitual response is not limited to non-vocal movements; for many of the word reactions of the ordinary man are simply habits. Catch words are repeated; the same stories are told over and over again in the same words and with the same appearance of originality. But all this is no more thinking than the repetition of a perfectly learned series of nonsense syllables is thinking. The truth is that while *all* substitute signs may properly be treated as language, some only function as expressing thought.

The thought response sign is essentially one which remains constant amid the greatest divergency of setting. From the point of view of expression, signs may certainly occur at the level of sensing, since any part of a process of sensing may bear a definite and fixed relation to other parts which precede and follow it. But sense signs are tied to immediately presented physical stimuli in the absence of which they do not occur.

Similarly signs are present in the process of perceiving but are tied to presented combinations of physical stimuli. In imaging, again, the mode of response always involves the use of substitute signs, which are frequently of an abbreviated character, and yet we rightly distinguish thinking from imaging. The fact is that the thinking response occurs, not to a physical stimulus alone, or to a combination of physical stimuli, but to some characteristic of a presented object or situation which is common to that and to many other objects or situations. Not only is it a response *to* such a characteristic, but in so far as the reaction is one of thinking, it is made precisely *as if* the characteristic were presented in an entirely different setting.

We may take one of Prof. Watson's own illustrations (page 330). "I start in to build a bridle path to the top of a hill with no special instruments and never having built a road before. If the grade is too steep I build my road around the side of the hill, taking advantage of various level places to approach the top; where the stones are too large or cannot easily be dislodged I build the road around them, finally coming out at the top. The process of constructing such a path requires manual labour almost ? wholly....Nevertheless, it is constructive work....In the process, though, parts of many previously organized habit systems have been used momentarily in slightly new connections. Just so in simple thought work...." Now it is not "just so," either in simple thought work or in the construction of a new road in so far as this is a relevant illustration. If it were and we inspected the road Prof. Watson had made, we should be surprised at the number of false starts to be found. It sounds perfectly simple to say, "if the grade is too steep I build my road around the side of the hill." But the real fact of the case is concealed in that statement. In so far as the response is a thought response it is definitely a response to *steepness*; not merely to a particular set of visual reactions, because that would not lead on, of itself, to the further set of muscular and other reactions involved in making the path round the hill; not merely to the steepness of *this* hill, because that also would not take me round it; but essentially to *steepness* as a quality common to this and to other situations and independent of any particular context. My construction of a path having a certain direction may, at least partially, be treated as solely the working out of habitual motor series. But what, in this instance, switches me off from the series 'going in this direction' to the series 'going in that,' is the response to a universal quality or relation. That, and that alone, gives us the peculiar characteristic of thinking.

Here also
necessary

Can we find any mode of expression specialised to the thinking response? Here we return to language and particularly to vocal language. Prof. Watson remarks that "the anatomy of the neck and upper thorax regions is the most complicated of the whole body" (page 313). Certainly vocal expression, together with its immediately associated movements, offers the most extraordinary possibility of varied combinations within which identical elements recur. Moreover, while sensory responses and manipulative responses involving the grosser muscles of the body are normally largely tied to particular forms of stimulation or to particular concrete objects, vocal response is much more readily conditioned by any stimulus or combination of stimuli whatever, and by the qualities or relations of which we have just spoken.

It may also appear as if emotional expression frequently occurs under much the same conditions. But emotional responses are, on the whole, themselves tied to particular characteristics and typical situations; the resulting bodily adjustments tend, consequently, to be more strictly limited and predictable. This we should expect in view of their close relationship to instinct. The emotional response is therefore less suited to express reactions to characteristics that must both combine with all possible settings, and remain unconstrained by any particular setting. It remains that language alone is perfectly well adapted to the expression of a reaction to universal characters in our environment.

But words may equally well constitute the response to other characters of our environment, and consequently the question arises as to whether there is any peculiarity about those word expressions which are really thinking responses. The characteristics suggested by Prof. Watson are abbreviation, short-circuiting and economizing. These no doubt apply, but they are not sufficient. With familiarity a single glance at a complicated object may do as much as, with unfamiliarity, may be effected by long and intricate manipulative processes. It is not unlikely that abbreviation is carried to a higher degree in thinking processes than in any others; but this must at best be a very indefinite criterion.

Two suggestions seem possible. First, word-thinking reactions might be catalogued, and then we could identify an instance when it occurred in any concrete setting. But this will not work; for what is in origin thinking may, in repetition, become habitual. Thus the same word response either may, or may not, indicate thinking, and there seems to be no expression difference between the words as such, which can be used to discriminate the two cases.

Secondly, Prof. Watson truly regards thinking as a very complex process; and reminds us that it must not be abstracted from its general bodily setting. Can the manner in which a certain word response is related to other reactions amid which it is found be used as a criterion of thinking? Two remarks may be hazarded. The first is that if we confine ourselves to a single occasion of thinking we can find no such peculiarity of relation. The second is that (*a*) if, taking a large number of word responses, we find that the expression in question has practically the same antecedents and consequents arranged in practically the same order, the reaction is purely automatic in character; but if the surroundings of the response, and their order of arrangement, differ, thinking may be present; and (*b*) if we *arrange* a number of instances varying in many respects but alike in some feature, or in general alike but differing in some particular aspect, we shall find that the true thinking response to these is characterized both by a minimum of delay[1] and by relatively little 'false starting.' Either (*a*) or (*b*) *might* give us a true expression criterion of a thinking process. But the test could be made reliable only after a detailed study of the conditions of the combination of habits, and it must be admitted that, at present, even at the best, we cannot, from a study of expression alone, say with more than a high degree of probability that a thinking reaction has occurred. This however is no very alarming consideration, for as a matter of fact it may be doubted whether we ever can say more.

5. THINKING AND ITS CONDITIONS.

Throughout the whole of his treatment of psychology Prof. Watson assumes that the study should be confined to a determination of the conditions of human responses, and of their adjustments. For our part we are willing to share this assumption, only remarking that it gives us no warrant for supposing, as Prof. Watson is prone to do, that problems of their conditions and variations exhaust all the important questions relevant to human responses. But this admission does not justify an identification of a response with its expression, whether the latter is overt or implicit. The danger of the reference to implicit expression becomes manifest at this point. Implicit expression only differs from explicit expression in that it is usually slighter, subtler and far more difficult to observe. It gradually undergoes attenuation until it becomes a mere assumption, whereupon it becomes easy, and appears plausible, to identify the expression with the response. We maintain, however, that the response and the expression of the response are, not only in thinking, but in all human reactions whatsoever, two different things. Throughout his discussion Prof. Watson almost invariably speaks of 'thought,' scarcely ever of 'thinking,' and in this way the distinction between response and expression appears to be slurred over. It may well be the case that whenever we think, we employ language in some form or another, but obviously this does not prove that the act of thinking and the language movement are the same.

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[1] The overt expression of thought, however, may be considerably delayed.

Can thinking, considered apart from its expression, form a subject of psychological study? Only in the most meagre manner. It might perhaps be said that thinking always involves response to relations and qualities as such, and it might even be possible to indicate to some extent its inter-relation with other processes. But any further progress would speedily demand an examination of the different modes in which thinking finds expression.

One further question remains: if we agree that psychology is, in so far as it is properly scientific, confined to a study of the conditions under which responses to a situation are made, what must we be willing to include under conditions? The answer is that we must include (1) a study of the character of the external situation, and (2) an account of the conditions which have place within the life history of the individual concerned. Among the latter, and of extreme importance in the case of thinking, are needs, wants, desires and affective processes which are all no more identical with their expression than is thinking itself.

We therefore conclude that so long as attention is confined to a study of the conditions of expression and the variations of the thinking response, Prof. Watson's treatment is broadly upon the right lines, but must be regarded as inadequately developed in respect to its details. At the same time the psychology of thinking, while it must be treated as a scientific study of the conditions and varieties of the thinking response, is only partially covered by an examination of the modes in which thought is expressed.

IS THINKING MERELY THE ACTION OF LANGUAGE MECHANISMS[1]? (II.)

BY GODFREY H. THOMSON.

1. *Introduction.*
2. *Body and mind.*
3. *Thinking as substitution.*
4. *Thinking as a response to general qualities.*
5. *Thinking as trial and error by proxy, and the discovery of general qualities.*
6. *Conclusion.*

1. INTRODUCTION.

MR BARTLETT and MISS SMITH, in opening this discussion, have distinguished two general problems which are involved in the question given as title, problems which they set out in their own words on page 56, and which may be roughly paraphrased as (1) Should we be content with a Behaviourist point of view in studying thinking, and (2) If so, do we as Behaviourists agree with Professor Watson's treatment of the subject?

First, they assume for the time being the behaviourist standpoint, and on this platform they discuss how one is to know those bodily activities which 'are' thought. Professor Watson, according to them, says that the essential criterion is 'substitution.' They protest firstly that the entities in question must not be *mere* substitutes, but also signs. They point out secondly that not all 'substitute-signs' imply thought, even when these substitute signs are words. They then urge that the peculiar characteristic of thinking is that it is a response to a universal quality; and to discriminate the 'thoughtful' signs from the 'thoughtless' they ask whether the subject's response persists under different arrangements of antecedents and consequents similar to Mill's Joint Method. If so, then it implies thought; otherwise not.

For only a brief while they then return to the discussion of the behaviourist platform as such, and their opinion in this connexion appears to be that though in theory the behaviourists are restricting the psychological field, yet in practice the restriction is small.

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[1] A contribution to the Symposium presented at the Congress of Philosophy in Oxford, 24–27 September, 1920.

Now I agree that there are these two general problems involved in the question set for the Symposium, but I think they are still further separable than this. I believe the 'substitution' question can be discussed not only on the behaviourist platform but on any platform—say on a purely introspective platform. I believe the question to be, at bottom, 'Is thinking a shortened form of trial and error?' and I have no hesitation in answering it in the affirmative. The other of the two general questions is ultimately the question, 'Is there a bodily state *b* which always occurs whenever and if ever the mental state *m* occurs and never at any other time?'

2. BODY AND MIND.

Reversing the order followed by Mr Bartlett and Miss Smith, I shall take the larger question first. Obviously it is not the intention of its promoters that this Symposium should become a discussion of the Body and Mind question, and like the authors of the opening paper I shall merely state a position with regard to it.

Personally I answer this question in the affirmative; but everyone knows that any reply is an affirmation of belief, not a statement of logical conviction. Clearly, however, all behaviourists must make this affirmation, for I take it that the behaviourist creed is that the causal sequences can be followed with less chance of error on the bodily side than on the mental side (since there many examples can be studied by many, but here only the self by the self), and that theoretically all predictions which are made from mental data can also be made from physical data. At the very least the behaviourist must believe that if there are any mental states left over, then these not only never find expression, but never influence any of the other states; and this is pragmatically tantamount to denying their existence.

Provided there are no mental states thus unaccounted for, it does not seem to matter whether the relationship be one of parallelism or of interaction. This distinction only appears to enter if causation is conceived of in some anthropomorphic fashion and not as mere inevitable accompaniment.

And of course a behaviourist does not deny that mental states exist. He merely prefers to ignore them.

When Professor Watson objects[1] to making a mystery of thought (page 324) and says that it is not different from tennis playing (page 325) I agree in a fashion. I think tennis playing is just as mysterious as thought, and do not see any advantage, scientifically, in dwelling on the point; just as I think the phenomenon of Röntgen rays passing through a stone no more mysterious than that of ordinary light passing through glass. We have become familiar with glass, and with tennis playing.

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[1] *Psychology from the Standpoint of a Behaviorist*, London, 1919.

The apparent paradox is disconcerting, of thought increasing in value, intensity and complexity, as the bodily processes become more and more sketchy. But this is *because* the sketchy processes are handier to organize, and quicker: and a mathematician has no difficulty in realising x as a function increasing always inversely with a, yet suddenly disappearing (or at least becoming unreal) when a becomes zero.

The practical difficulties of the behaviourist standpoint appear to me enormous, and I do not think there is any reason for giving up introspection. No doubt it is necessary for the advance of science that a school of behaviourists should arise, who will devote themselves to the study of one side of the phenomenon and check wild statements by the other party. No doubt too their methods are essential in studying animals and in studying children. But as we approach adult psychology it seems unnecessary to avoid too strenuously the examination of our own minds.

3. THINKING AS SUBSTITUTION.

Turning to the other question, it is to be noted again that Mr Bartlett and Miss Smith, while agreeing that thinking involves substitution, do not agree that substitution necessarily involves thinking, unless 'substitution' is more carefully defined than is done by Professor Watson. The distinction, however, which they draw between a substitute which is a *mere* substitute and one which is a *sign* does not appeal to me. I cannot think that Professor Watson ever intended to speak of *mere* substitutes; I feel convinced that he means exactly what Mr Bartlett and Miss Smith mean by 'substitute-sign.' The whole point of substitution is that the substitute functions like the absentee *in a relevant way*. If one soldier goes on guard as substitute for another, he need not make up to look like him, but he must do the guard. If he looked like him but did not do the guard he would be a *mere substitute* and not a *sign*. On the other hand, if it is not a question of taking the absent soldier's place on guard, but of impersonating him so as to prove a false *alibi*, then the essential thing is that the substitute must look like the missing man. If he went on guard for him but admitted that he was Private X and not Private Y he would now (reversing the former case) be a *mere substitute*, not a *sign*. What Mr Bartlett and Miss Smith's criticism amounts to is, that they emphasize that the substitution must be relevant substitution; and to that no one can object except perhaps Professor Watson, who, unless I mistake him, will protest that he never meant anything else. His whole account shows, it seems to me, that the idea of 'leading on to' something is the essential of a 'true language habit.' See for example pp. 320–321. 'Box' is not true language when it merely accompanies the thing, but only when "the child learns that the uttered word is a sufficient stimulus to cause the attendant to hand him the objects he names without actually having to execute bodily movements with respect to them."

4. THINKING AS A RESPONSE TO GENERAL QUALITIES.

As a criterion to distinguish substitute-signs which imply thought from those which do not, Mr Bartlett and Miss Smith put forward the suggestion that thought is a response to a general quality and that it can be distinguished therefore by a kind of logical method of agreement among differences, or difference among agreements. This fails entirely to satisfy me, indeed it appears to me to be a criterion of what is *not* thought; for thought is surely not an inevitable response. I have two objections to urge, therefore. (1) The criterion would be inadequate even were it partly true. This objection, however, must I fancy be based on a misunderstanding on my part of Mr Bartlett and Miss Smith, similar to what I conceive to be their misunderstanding of 'substitution.' At all events, my objection can be made exactly on the same lines. Clearly, *mere* response to a general quality is not thinking. For a bull responds very consistently, I believe, to *redness*, even as did the roadmaker to *steepness*.

A possible reply would be that the bull does not respond to *redness* but to a red rag or a red cloak. To this I would only say that the response persists in many settings as they require, and that it is difficult to see how the redness becomes a general quality except by having a sign substituted for it; and that is Professor Watson's criterion.

(2) The criterion is erroneous. I should on the contrary say that when a response to a universal quality has become so inevitable as is suggested by the comparison with the methods of formal inductive logic, then thinking has ceased. A *habit* (on a high level no doubt) has been formed. Logical machines can be and have been made to do this part of the work. Mathematical symbols and all the machinery of mathematics are devices to short-circuit thought. Instead of the *response to* a general quality being the important thing, I should have said that the effort *to discover* a

general quality was the main point. This can be done ultimately, I believe, only by trial and error, and I prefer to look upon thought as a procedure of trial and error in which substitute-signs are used instead of actual bodily trials being made.

There is in this as in all discussion a very considerable danger of ambiguity in the use of words. Such ambiguity seems to be present here in the use of the word *habit* which is employed in a very general sense by Professor Watson, whereas it is I think more commonly used to mean what he calls a "well established" habit. No harm is done if we understand one another, and to assist such understanding is the object of the next few lines.

To Professor Watson thought is an "implicit language habit." I have just above expressed the opinion that thought ceases as habitual responses crystallize. The possibility of these differences in the use of the same word is due at bottom to the continuity of mental phenomena. When a psychological school proposes to extend the word attention to cover what is ordinarily termed inattention there is considerable justification for their action, inasmuch as there is clearly something in common all along the line from the one end to the other. Similarly with *habit*. Professor Watson protests that "many regard the term as implying something that is inevitable, invariable" (p. 269). To this one can only reply that while of course it would be wrong to restrict the word habit to acquired reactions which are absolutely invariable, yet one cannot leave out altogether the fact that the fixing of a habit means an approach to this invariability. 'Habituation' is a name for a certain direction in which responses may move, and at what point on this line of direction we begin to call the response a habit is left very indefinite. All that I wish to urge is that progress in that direction is progress away from thought, although all thought must use habits fixed at lower levels.

One other point would appear to fall under the present section. Mr Bartlett and Miss Smith say: "If we confine ourselves to a single occasion of thinking we can find no such peculiarity of relation" as will serve for a criterion of thinking. To that statement I would like to take exception on grounds of experience. Teachers, speakers, lecturers, barristers *do* to a very considerable extent know by the accompanying physical signs whether the pupil, hearer or witness is thinking, and even what he is thinking. But no doubt it is exceedingly difficult to say by what criterion one judges. Largely it is by responding to the slightest facial expressions, and to differences of very subtle order in the tone of the voice.

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5. THINKING AS TRIAL AND ERROR BY PROXY AND THE DISCOVERY OF GENERAL QUALITIES.

The idea of substitution serves, to my mind, to connect thought with that method of successful response which we call 'procedure by trial and error.' Indeed, I should like, temporarily at least, to say that thought *is* trial and error, but that the trials are not complete but sketchy and abbreviated, finally being so abbreviated as to be unrecognisable. A book which was popular during the South African War was *The Defence of Duffer's Drift*, in which Lieutenant Blank, sent with his half company to defend the Drift, dreams six or seven consecutive dreams as to the dispositions he would make. On the first occasion he made a pretty camp and was scuppered next dawn. The second time he dug a trench, but a straight and easily visible one, and was again knocked out. In later trials he suffered fewer casualties but let the enemy pass. Finally he hit on the successful plan. If we imagine Lieut. Blank to have actually arrived on the ground, and to be an officer of only moderate experience, we can imagine some of the alternatives passing through his mind (*i.e.* on the bodily side implicit laryngeal and other movements taking place), and others actually leading to overt action which is presently checked. For instance, the straight trench might be begun, to be abandoned as its futility was realised. There would be, in so far as there was thought, false starts, just as Mr Bartlett and Miss Smith say.

If the officer were experienced there would be hardly any thought at all. This drift with that hill beside it and that farm at a little distance would be different from any other drift, but the general qualities of such situations would be so easily recognised and so habitually responded to, that thought would be absent except just in so far as there was difficulty in recognising them in this setting. This 'thinking' would almost certainly consist in the rejection of false starts. A sentry group which had actually been posted here would be moved thither, or it might be that the first false move never got beyond implicit action.

These false starts it is which give to any piece of written work the character of being thoughtful. It is the careful trial and rapid rejection of the alternatives which impresses the reader, who values the work the more highly, the more abbreviated these false starts are, provided their rejection is conclusive. Thinking over an essay which we are reading consists mainly in trying the paths which the writer has declared to be *culs-de-sac*, and in endeavouring to discover others.

A friend of mine who is an accomplished mathematician has more than once remarked on his changed attitude of mind now towards two lecturers under whom he sat at Cambridge. Then he thought A (whose lectures were useful and flawless) much superior to B (who floundered among mistakes on the blackboard). "Now," he says. "I realise that I learned far more from B, for in his lectures *we were seeing him think.*"

The false starts, then, appear to me to be of the essence of thinking. The rejections are due originally, no doubt, to the affective states associated with first the actual action, later with the substitute sign; so that there is a very intimate connexion between thought and feeling. The wry face which we make when we cannot solve our problem is the expression of disgust.

6. CONCLUSION.

If finally I return to the definite question 'Is thinking merely the action of language mechanisms?' then I must confess that two words stick in my throat—*merely* and *mechanisms.* If that question means what it would appear to mean to a plain man, then my answer is in the negative. But if it means, 'Is there always something going on in our body when we are thinking, which as uniquely defines our thinking as does our consciousness of thinking, and is that something especially distinguished by being, in normal persons, chiefly compounded of incipient speech expressions or impressions?'—then I can agree. I have for many years, and especially during the last few weeks, spent a good deal of time in making observations in a school for the deaf and dumb, and nothing impresses one more with the importance of language for thought. When free communication is difficult to establish with others, as it is with deaf and dumb children, it is of course very easy to say that *we* think and that *they* don't; and one must guard against such injustice. But nevertheless the conviction is forced upon one that, whatever the potentialities of the imprisoned mind—or the inaccessible brain—may be, the actual thinking is in fact tremendously hampered by lack of a mother-tongue (for these children have no mother-tongue).

As against Mr Bartlett and Miss Smith, I agree with what I imagine to be Professor Watson's criterion of language (or other 'implicit function') namely that the essential point is 'substitution.' But the substitution must be relevant substitution, and this, which I take it is what Professor Watson meant, really covers all the other points. For sooner or later in the sequence *mere* substitution will prove not to have been substitution; and the search by trial and error for the relevant point among diverse settings is the same thing as the discovery of and response to general qualities. Only I think that when the search is finished thinking has ceased, and thereafter a well established habit appears, which may itself be used in other thought. Moreover, the whole process is a continuum, and before the one level of thinking has ceased, the other has already begun.

IS THINKING MERELY THE ACTION OF LANGUAGE MECHANISMS[1]? (III.)

By T. H. PEAR.

1. *Introductory.*
2. *What is thinking?*
3. *The distinction between thinking and its expression.*
4. *The rôle of imagery in thinking.*
5. *Is the behaviourist's explanation of 'imageless thinking' adequate?*
6. *The appeal of behaviourism to certain types of mind.*
7. *Behaviourism and the problem of meaning.*

1. INTRODUCTORY.

IN considering Professor Watson's views, I have taken them from his two published books[2], and not from his later work only. For this decision there were two reasons. The first was that the 1919 statement of his beliefs has already been criticized by my three predecessors; the second, that this later account seems to bear traces of the "abbreviation, short-circuiting and economy"—to borrow his own terms—which all thought processes inevitably suffer on frequent repetition. Indeed, while reading the 1919 book, one sometimes runs the risk not only of forming language habits but even of substituting them for thinking, and of allowing to pass, in their telescoped form, some of his statements, the earlier and fuller presentation of which is far more challenging.

2. WHAT IS THINKING?

To define thinking rigidly and precisely is difficult. It may even involve the use of terms which, standing for ultimate things, are themselves incapable of definition. It is therefore probably more satisfactory to describe or to indicate common experiences which have fallen to the lot of most of us; to say "this is, and that is not, thinking," and thus to distinguish between thinking and the mere revival of experiences.

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[1] A contribution to the Symposium presented at the Congress of Philosophy in Oxford, 24–27 September, 1920.

[2] *Behavior, an Introduction to Comparative Psychology,* New York, 1914; *Psychology from the Standpoint of a Behaviorist,* London, 1919. For the sake of brevity in the text the first of these will be referred to as "B," the second as "P."

Let us suppose that, the time of year being March and the place somewhere in the North of England, a jaded business man is told that he must take a week's holiday. Five minutes later, being found in a brown study, he explains that he is 'thinking' whether to go to Cornwall or to the Lakes. Now, such a statement may describe at least two kinds of experience, only one of which is, to my mind, genuine thinking. His 'mind's eye' may have conjured up for him first the swirling greens and purples of the sunny Atlantic, then the peaceful snow-covered Langdales: he may feel again the sun's warmth as he lazily sprawls on Mullion cliffs or the sting of the wind and the pull on his muscles as he negotiates the Pillar. Finally, the appeal of one place or the other toppling him over, he sends his telegram North or South, and our interest in him ceases.

Now, allowing oneself passively to be tossed hither or thither by competing memories until, finally, one is cast up on some shore or other, though a common way of arriving at a decision, is not, I submit, *thinking*. Yet let us suppose that our friend, realising that to him the chief appeal of Cornwall is its lazy warmth, of Cumberland its exhilarating activity, that the first involves a long day's journey, the second an afternoon's, that the one holiday will be expensive, the other cheap, remembers that he has only a week, a slender purse, and an urgent desire for violent exercise. If, then, on the grounds of time, money and health, he chooses the Lakes, he may justifiably claim that he has "thought the matter out." Now what psychological processes does this involve?

In such an experience there seem to be the following salient features:

(i) Recall of past experiences; perhaps a faithful recall, but more probably one which is the result of processes of selection, condensation and the like.

(ii) Abstraction of their relevant aspects, under the guidance of conscious or unconscious directive tendencies, of which logical thinking and the mechanism known as the 'censorship' are special examples.

(iii) Comparison of those experiences, with the discovery of their likenesses and differences.

(iv) Re-comparison, with some aim or aims in view.

(v) Combination of the results of this comparison into some conclusion which is new for the individual himself.

(vi) Expression of this conclusion to others, or to himself, in speech, writing, gesture or action.

I am inclined to believe that unless in (v) the conclusion *is* new for the individual, it cannot without ambiguity be termed a result of thinking.

Like Dr Thomson, and for the same reason, I find it difficult to put thinking and habit into the same category. When an acquaintance says "Good morning" to you, it is unwise to take this as a proof of his 'thoughtfulness,' for (or of) you; as an expression of his earnest wish that during the present forenoon everything will go well. And such caution is certainly to be advised if, as sometimes happens, his remark be made in the evening. To call such a person absent-minded surely implies our belief that in this case his language mechanism, or language habit, did *not* express his thinking.

3. THE DISTINCTION BETWEEN THINKING AND ITS EXPRESSION.

But the conclusion in (v) may be unexpressed, or only partly expressed, verbally by the thinker to himself. The thinking may be 'imageless,' and wordless. Its expression, or the way in which it issues and records itself for others, is commonly in words. But this thinking and the expression of thought are as distinguishable as skating from the figure skated. We do not refer to an '8' on the ice as skating but we call it a skated figure. And for a similar reason I cannot see why we should designate as thinking the language processes which are merely one expression of it. Like Miss Smith and Mr Bartlett, I do not agree that "when we study the way a golfer stands in addressing his ball and swinging his club we are studying golf"[1] in any complete sense of the word 'study.' We are merely observant spectators of a temporary expression, or acute symptom. Moreover, if we think that this is studying golf, are we not exemplifying perfectly the shortcomings of most lookers-on at games, and perhaps, too, at behaviour?

To me it seems that 'golf' may mean:

(1) The exhibition of the player's actions to others.

(2) The 'feel,' 'look' and 'sound' to the golfer of his own temporary activity. (For obvious reasons, I am here avoiding the terms 'introspection,' 'sensation' and 'image.')

(3) The knowledge, which may or may not express itself in action, speech or writing, of the collection of rules, customs and traditions which constitute golf.

(4) That kinaesthetic knowledge which the arrogance of our intellect sets aside from other knowledge, calling it skill.

If we watch the apparently careless ease of a first-class skater, and then inspect the figure which he has traced, are we studying 'skating'? If so, we are adopting a method of investigation which, unless supplemented, will carry us only a little way towards appreciating what the skater himself means by skating, or understanding how the geometrical simplicity of the figure is paralleled by the extraordinary physiological and psychological difficulties in cutting it. So it would seem to be with our thinking; the words which express its results to others, or even to ourselves, are often but ready-made suits which we catch up for want of better ones to hand, which clothe our thoughts but baggily and hide their individuality of outline.

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[1] P. 326.

4. The Rôle of Imagery in Thinking.

I have tried my hardest, and have failed, to find a way of discussing briefly the subject of this Symposium without mentioning the behaviourist's attack upon the doctrine of the image. And the reasons for my failure seem to be these; first, that it seems impossible, at this stage of our knowledge, to discuss thinking without mentioning imagery, secondly, that Professor Watson's account of the commonest type of imagery—visual—seems to me questionable, thirdly, that I am far from convinced that he has made out a case for the dismissal of the image from psychology. I must therefore discuss briefly some aspects of the relations between thinking and imagery.

The raw material upon which thinking operates, and without which it cannot exist, is composed of selections from revived experiences. What shall such raw material be called? I should be inclined to say that it consisted largely of *ideas*, in Professor Stout's sense of the term[1], and *notions*, in Professor Ogden's[2]. But this at once exposes me to criticism from Professor Watson, for an idea is a significant mental *image*, an image *plus* its meaning, and a notion is "a content of experience that sums up in a nuclear form a series of experiences which, *in extenso*, would involve sensations, *images* and affections." And it is just the "fiction" of images, with their aliases—"centrally aroused sensations" and the like—, which behaviourists assert must be destroyed. In 1914, Professor Watson admits that the inclusion of the image weakens the claims of the behaviourist, and adds that "it seems wisest, even at the cost of exposing the weakness of our position, to attack rather than to remain upon the defensive."[3]

The three pages which follow this statement indicate some directions along which an attack might some day be conducted, but, surely, little else. And in 1919 the word image does not appear in his book, because Professor Watson finds that he can "get along without it" (p. viii). Yet in the intervening years scores of investigators have been studying thousands of cases in which the main focus of interest has centered upon what the ordinary psychologist calls images. The obsessive memories, dreams, and hallucinations of the war-neuroses—more especially of the anxiety-neuroses, where the behaviour and speech of the patient often triumphantly succeeded in concealing his thoughts—all these have flooded the literature of psychology. Few other psychologists, especially perhaps those dealing with hallucinations and dreams, seem to have rejected the conception of imagery, and neither those three pages in 1914 nor the 1919 book convince me that Professor Watson can dispense with it.

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[1] *Manual of Psychology*, London, 1904 (2nd edition), 410.

[2] *Introduction to General Psychology*, London, 1916, 90.

[3] B. 17.

I hope it is clear that I am not here asserting or denying the existence of any metaphysical differences between image and percept, nor am I venturing any risky speculation as to whether they differ psychologically in 'texture.' But at present, being concerned merely with the province and methods of psychology, I cannot see that Professor Watson has adduced evidence sufficient to bring about the abandonment of the study of 'imagery' by 'introspection.' Moreover, the events which have occurred since his first book was published would appear to have made it obvious that the introspective approach in psychology cannot yet be abandoned. I am therefore inclined to agree with those writers who hold that behaviourists should widen their scheme in order to admit the image.

5. IS THE BEHAVIOURIST'S EXPLANATION OF 'IMAGELESS THINKING' ADEQUATE?

Let us now discuss the case in which this debatable entity, the image, becomes unimportant for the purpose of discussion or vanishes altogether; viz. that of 'imageless thought,' or, to put it more narrowly, of certain experiences termed 'awareness of meaning,' in which images are undiscoverable even by expert introspection. If such awareness of meaning be no longer 'carried by' images, even by images of words, spoken, heard or seen, this would seem to be the place for a behaviourist's theory of thinking. Such imageless awareness, leading perhaps to imageless thinking, may be 'carried by' or 'go on in terms of' implicit language, or may be identifiable with implicit language itself. I include both possibilities, for I am not always quite certain whether Professor Watson favours one or the other. He says of "reasoning, imagery, etc."...

"such processes are dependent upon language or upon a set of similarly functioning bodily habits put on after language habits[1]"; the latter expressing the means by which we have "short-circuited (substituted for) the word system of thought[2]."

Now it is just the latter which enable Professor Watson to give a negative answer to the straight question "Do we always think in terms of words?" and thereby to escape, at least for the moment, the difficult task of explaining how it is that a man may sit for an hour unable to put his thoughts into words; how we may remember the meaning of an epigram, of a poem, or of a philosophical theory and yet be utterly unable to recall the words in which it was expressed. But of such bodily habits the examples which he gives are simple movements like the nod of affirmation, the shrug of the shoulders, or "winking, which expresses a whole series of words."

Presumably, being gestures, they eke out the inadequacy of the words. We all tend to use them, for instance, when floundering in a foreign language. But is it possible to believe that a political philosopher's awareness of the meaning of 'international relations' might consist in the mere articulation of the phrase, with or without the help of a wink, however subtle? An old-fashioned psychologist, while admitting that the articulation of the words and even the wink, the shrug or the nod might bear some of the weight of the awareness of meaning, would have shifted most of the support to the image. A less old-fashioned psychologist, believing that awareness of meaning might exist without imagery, or at any rate with the most tenuous, unsubstantial shred of it, would probably admit the participation of the words and the bodily habit. But to make them solely responsible for the awareness of the meaning of, say, the theory of relativity seems going too fast. To quote from Professor Watson: "the train of thoughts going on in your mind, according to the upholders of the image, has no adequate behaviour counterpart while it is in transit."[3] The difficult word here seems to be *adequate*. Most people would admit that many trains of thought may have adequate behaviour counterparts, but what is the really adequate behaviour counterpart to the full awareness of the meaning of $\sqrt{-1}$, unless it be the formation of the words or written symbols? But it is just this that many abstract thinkers deny. For if they be thinking in words (not in images of words, for that is not admitted by the behaviourists, but in actual word experiences), why, when they struggle for hours with thoughts which are difficult to express, don't they just write them down and have done with it?

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[1] B. 334.

[2] *Ibid.* 332.

[3] B. 17.

It would really seem that if the words are there we ought to be more conscious of them than, unfortunately, some of us are. For it seems unlikely that the behaviourist, of all persons, could wish to suppose the presence of words which are unconscious, in the sense that we have never yet been conscious of them. Yet how does he explain the common experience of waiting for a suitable epithet to drop off our pen; when we know "how we feel" about a subject, but cannot find a word? One day, in writing of a person's life work, I found myself clearly conscious not only of the aspect which I wished to describe, but also of the fact that the word in my mind—'exact'—was not the word I wanted. Next there came, to my surprise, a visual image of a flat piece of dough, upon which a ring-shaped tin cutter promptly descended, cleanly detaching from it a circular disc. Then, and not till then, my pen wrote the word for which I had been waiting,—'precise.' As far as I know, I had never before noticed the derivation of this word and its connexion with cutting. Now, if the behaviourist will allow that both the image of the pastry-cutter which came first and the word which followed, represented the meaning, we might come to terms, provided we agree that the awareness of this particular meaning, i.e. of the preciseness of the whole of a scientist's descriptive writings, was supported by, but by no means confined in, either of them.

6. THE APPEAL OF BEHAVIOURISM TO CERTAIN TYPES OF MIND.

But this leads on to another question which appears to me to be inevitable,—whether some features of Professor Watson's exposition may be expressions of his own predominant kinds of imagery. If, taking some of his phrases, the reader will study the context in which they occur: the "fiction of visual imagery"; "kinaesthetic *substitutes* for imagery," when what he is describing seem rather to be kinaesthetic *accompaniments* (B. 18); "had the idea of the image not taken such firm hold upon us..." (B. 20); that under "more constructive language types of activities" he includes the plan of a novel, the painting of a masterpiece and the composition of a great opera (P. 225); "dreams are word-reactions"; and his account of his own experiences—that before tearing off the leaf of his calendar he says aloud "tear off leaf," and that if he *thinks* the words "tear off leaf," "the words *must be uttered* silently before the habitual act arises" (B. 333); that hesitation before going to the races is expressed (in the reader, according to the text, but surely rather in Professor Watson!) in "conflicting word processes" and "that the final word act issues" (P. 332); that "in watching a fight we tend to ward off a blow or strike a blow" (P. 108), and compare them with his statement "...closer examination leads me to deny in my own case the presence of imagery in the Galtonian sense[1]," this supposition seems to be justified.

One comment upon this is that it would be interesting to know how much of this knowledge was obtained by any process other than that which psychologists usually call introspection; another that the data of introspection should be verified by observations made upon large numbers of very different types of mentality; still another, that not only has Professor Watson no right to implicate the helpless reader by his use of 'we' and 'you,' but that introspective study of imagery makes most psychologists, in writing upon such a subject, hesitate to use even the impersonal 'one.'

For to a visualiser, though this account of the "fiction of visual imagery" certainly seems stranger than fiction, it is hard to believe that it is truth. Take for instance the statement[2]: "There are probably in most cases kinaesthetic substitutes for imagery. Concurrently with the *articulated word apple*[3], there arise associated kinaesthetic impulses in eye muscles. If these latter are strong, one can see how the fiction of visual imagery might arise."

Comment has already been made upon the first sentence in this quotation. The second apparently assumes (for it is difficult to give it any other meaning) that when we image an apple we faintly articulate the word. But do all of us? The other night I said to a friend that a piece of silk needed 'oiling.' At the time I said it I knew quite well that it was crumpled, and I had a perfectly clear knowledge of the process necessary to restore its shape, viz. ironing. But if my thought of the process of ironing included the "faintly articulated word" 'ironing,' or other "kinaesthetic substitutes for imagery," presumably the articulation of the word and the kinaesthetic substitutes which correspond to ironing should have prevented the mistake.

Concerning the third sentence of the last quoted statement, there seem to be two difficulties:

(1) The consciousness of the movements of the eyes necessary to fixate an apple, or even to follow its outline, is one type of mental process, that of its colours as imaged appears to be quite another.

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[1] *Psychol. Rev.* 1913, xx. 173.

[2] B. 18.

[3] Italics in text.

(2) What little experimental (behaviouristic) evidence we have seems to be against the probability that there is any close connexion even between the eye-movements and the shape of an object, though Professor Watson says (P. 324) "we have learned to draw objects and to *trace them with the eyes.*" This sentence seems to contradict the findings of experimental aesthetics[1].

To sum up this section, I cannot help feeling that the above account may be true of the 'motor-minded' person whose visual imagery is weak, but that it should be supplemented by accounts from the large numbers of persons whose powers have been bestowed in the inverse relation. It would be leading this discussion too far from its subject to speculate to what extent the extrovert type of mind tends to welcome behaviourism, the introvert to consider it inadequate. But a discussion between Professor Watson and Mr Henry James, with, perhaps, Professor William James acting as interpreter, would have been invaluable.

7. BEHAVIOURISM AND THE PROBLEM OF MEANING.

As I have indicated before, I cannot make up my mind whether the behaviourist really claims that the awareness of meaning is 'carried by' (or 'goes on in terms of') these language processes, or is identifiable with them. In either case it would be interesting to read a behaviouristic explanation of those experimental investigations of thinking which make it difficult to harmonize the laws which govern the memory of meanings with those which govern the memory of words[2]; why we can learn fifteen pairs of similar meanings in one repetition, each meaning being expressed in perhaps five words, while no such happy facility is displayed in learning paired vocabularies; why two meanings may be associated with the greatest ease although the words which express them have never been in consciousness together or even in immediate succession; why the second 'half-thoughts' in a series will at once associate themselves, and retroactively, with their partners in a series read some time before; why the subject may give the right thought in the wrong words. I cannot yet understand all this on the supposition that thinking consists merely in the action of language mechanisms.

When we recall Professor James's description of thought as a series of flights and perchings, it seems that the behaviourist has given us an account of some kinds of perchings, and, fascinating as it is, it reads like a description of flying by an aerodrome mechanic, who sees only the last stages of the aviator's descent. But, to the upholders of the image, as well as to the behaviourist, the 'awareness of meaning' still remains an unsolved problem. Even if it turns out to be incipient behaviour, there will, probably, still be room for its study, from the inside, by introspection.

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[1] See Valentine, *The Psychology of Beauty*, London, 44 f.

[2] Cf. Külpe, "Ueber die moderne Psychologie des Denkens," *Internationale Monatsschrift* series, and the work of the Wurzburg school generally.

IS THINKING MERELY THE ACTION OF LANGUAGE MECHANISMS[1]? (IV.)

BY ARTHUR ROBINSON.

1. *The general programme of behaviourism.*
2. *Thinking and language mechanism.*

1. THE GENERAL PROGRAMME OF BEHAVIOURISM.

THE question set as the subject of this Symposium arises directly and inevitably from the programme of the Behaviourist school in psychology, and it is difficult or impossible to discuss the issue raised without briefly considering certain features in the general situation. This is happily rendered all the easier by the circumstance that we are dealing here and now chiefly with the views of Professor Watson, who (unlike some others) does not concern himself with any matters usually called philosophical, but seeks a straight scientific solution of a perfectly definite problem. As a quite necessary preliminary the 'traditional' or 'classic' psychology is quietly moved out of the path of progress. The business of psychology is the prediction and control of behaviour. Adjustment to environment is assumed as the characteristic situation; in adjustment we have a response and a stimulus or situation which calls out or determines that response. Hence "the goal of psychological study is the ascertaining of such data and laws that, given the stimulus, psychology can predict what the response will be; or, on the other hand, given the response, it can specify the nature of the effective stimulus."[2] The terms *stimulus* and *response* are used in their physiological sense, but *stimulus* is also used to include a *situation* (*i.e.* a complex group of stimuli), and *response* to include several complex simultaneous responses (*i.e.* adjustment)[3]. The crucial divergence from the current view lies in this, that Professor Watson holds it irrelevant to inquire whether, when an organism responds, the stimulus responded to is in its field of consciousness or not. It makes no difference, from this point of view, whether the organism does or does not know what it is doing. All introspective material is left outside the inquiry, though introspection is rejected not on speculative but on practical grounds. Progress is held to be impossible unless the introspective method is discarded. "This suggested elimination of states of consciousness as proper objects of investigation in themselves will remove the barrier which exists between psychology and the other sciences. The findings of psychology become the functional correlates of structure and lend themselves to explanation in physico-chemical terms."[1]

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[1] A contribution to the Symposium presented at the Congress of Philosophy in Oxford, 24–27 September, 1920.

[2] B. 10. I follow the notation of Prof. Pear, B. referring to Prof. Watson's *Behavior, an Introduction to Comparative Psychology*, New York, 1914, P. to his *Psychology from the Standpoint of the Behaviorist*, London, 1919.

[3] P. 10, 11.

The upshot of course is that psychology is in no sense a science of mind. We have long been accustomed and hardened to the conception of a psychology without a soul, but a psychology without a mind does seem—*sit venia verbo*—'the limit.' Of course it would be idle to make much of the mere name, and plead the established use and sense of the term psychology. We may agree with Professor Watson that after all the name is not of much consequence. The fundamental point is to know the exact question to which your science is to find an answer; a science is defined by its problem not by the derivation of its name. Let us assume for the moment that adjustment and its conditions furnish a sufficient frame for what we need know about man and his actions, yet Behaviourism, by restricting itself to the physiological conception of stimulus and response in its premises, is quite obviously restricted to the same conception in its conclusions. That is to say, its findings will be about the body not about the mind. My point is that although Behaviourism is clearly right in leaving out of consideration any data which can be proved to be irrelevant to its problem, no one doubts that man is conscious, and this fact, which at least appears to be relevant to his actions, ought not to be discarded without some evidence of its irrelevancy. Such evidence has yet to be produced.

Psychology, then, according to this theory is to study behaviour only, and behaviour is the adjustment of the body to its environment. *Explicit* behaviour "involves the larger musculature in a way plainly apparent to direct observation." *Implicit* behaviour involves "only the speech mechanisms (or the larger musculature in a minimal way; *e.g.* bodily attitudes or sets)."[2]

Professor Watson naturally does not fail to see that to complete his scheme images and affective processes must be disposed of. The former are held to be non-existent or problematic; the place of the latter, it is suggested, may be supplied by physiological processes—changes in secretion, circulation, etc.[1]

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[1] B. 28.

[2] B. 19.

The programme is now in outline fairly complete. Consciousness plays no part in it except the part it plays in relation to all science—that is, it is external to, and absolutely non-constitutive of, its object—or, in other words, it means the awareness of the investigator. All the same, Professor Watson believes that "Psychology as behaviour will, after all, have to neglect but few of the really essential problems with which psychology as an introspective science now concerns itself."[2]

The answer to the question raised in this Symposium, it seems clear, must depend on the acceptance or refusal of the conception of Psychology which has just been outlined. This conception I am unable to accept for the following reasons in chief:

It seems to me quite unjustifiable to rule out data simply because they are 'private' and are therefore less easy to verify than 'public' data. The circumstance is no doubt a disadvantage for scientific investigation, but the data are there, and the universe does not seem to have been constructed with a too careful eye to the convenience of those curious enough to try to understand it. Introspection, therefore, I feel compelled to retain, though I mean by it nothing more mystic than the fact that I can have a headache and know it. Now it is clear that by limiting the subject matter of psychology to bodily states and processes, to physico-chemical changes, the Behaviourist does not solve the problem which the traditional Psychology attacked but substitutes another (though a possibly connected one) for it. The old question is, What processes form the contents of a field of consciousness, what is their nature, what their laws, and how are they related to the bodily processes which accompany them? The Behaviourist asks: To what physical stimulus is a given physico-chemical change in the body a response? In a word the traditional psychologist studies a psychical system to which consciousness is internal, and whose constituents are sensations, images, emotions, etc., the Behaviourist a system simply physical.

The starting-place of psychology is the enquiry: What is present in the field of consciousness when an organism makes a given response? Man is like a machine with an inner dial, we want to know whether the reading of the introspective indicator agrees or disagrees with the direction which the organism is overtly taking. Nor can I help thinking that this information is no less essential than the behaviour of arm, leg, or larynx. When a man insures his life, is his response governed in any way by prevision of an event possibly far distant, by the conscious recollection of the unwisdom of others, etc., or can his every movement be linked to a purely physical antecedent or set of antecedents? If the latter be the case it would still be the most interesting point conceivable to know why or how the index of his consciousness points to quite a different nexus of conditions.

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[1] B. 26.

[2] B. 28.

I agree with Professor Pear in his condemnation of the wholesale rejection of imagery by Professor Watson and his wing of the Behaviourists. There is, as Professor Pear points out, little or no ground for it. Moreover, on any reasonable estimate of the Freudian movement nothing is clearer than the immense importance and significance of the play of imagery in dreams, fantasy, and indeed in the whole field of human adjustment. If the business of psychology is the prediction and control of human action, is it not sheer folly for the Behaviourist to limit himself to only one side of the evidence, and that by no means the most significant?

The treatment of affective processes is also unfortunate, for affection is certainly not identical with changes in circulation and glandular activity. Yet in their reaction against psycho-physical parallelism the Behaviourists rush straight on one of the worst points of that theory. Professor Thomson says the Behaviourists do not deny that conscious processes exist; they only prefer to ignore them. But they can only ignore them on one of two suppositions. They must suppose either (*a*) that conscious processes are inefficient, or (*b*) that they are so 'expressed' in bodily processes that the mental can be adequately studied in its bodily outcome. On the first supposition, all that we commonly call mental—desires, hopes, fears, imagination, reasoning—can have had no effect whatever on the process of evolution; the field of consciousness has not only been without effect: it has not even served as a mere index of the conditions to which the body has responded, but has been simply an uncalled for and inexplicable illumination of a chain of events to which it has no relation that can be intelligibly expressed. The second supposition is definitely rejected by Professor Watson. One of the chief points of his programme is to elevate (or reduce) the study of human to the same level as the study of animal psychology, in which the agent's 'mind' is inaccessible. The bodily processes are therefore not to be studied as the 'expressions' of anything. "It is granted that the behaviour of animals can be investigated without appeal to consciousness. Heretofore the view-point has been that such data have value only in so far as they can be interpreted by analogy in terms of consciousness.

"The position is taken here that the behaviour of man and the behaviour of animals must be considered on the same plane; as being equally essential to a general understanding of behaviour. It can dispense with consciousness in a psychological sense. The separate observation of 'states of consciousness' is, on this assumption, no more a part of the task of the psychologist than of the physicist."[1]

On the general issue, therefore, I answer that the Behaviourist statement (*a*) mutilates the essential problem of psychology; (*b*) neglects evidence which exists, is accessible and relevant to the understanding of human nature; (*c*) leaves the undoubted fact of consciousness an isolated and gratuitous miracle.

2. THINKING AND LANGUAGE MECHANISM.

As to the special question of thinking regarded as the action of language mechanism, my answer follows quite inevitably from the position taken in the previous section. Muscle and gland activity is not thinking, and nothing whatever can be gained by so regarding it; we have seen that the Behaviourist does not welcome the suggestion that the bodily activity may be treated as the 'expression' of the mental.

Mr Bartlett and Miss Smith quite rightly draw attention to the ambiguities of the terms 'substitute' and 'substitution' in the Behaviourist theory of thought. It is quite clear that if we keep strictly within the abstractions of that theory it becomes exceedingly difficult to give an intelligible account of language. Consider Professor Watson's example, which I quote at length to avoid the possibility of injustice in a summary statement.

"A stimulus to which a child often responds by opening and closing and putting objects into it is the box in which his toys are kept. The nurse, observing that the child reacts with his hands and fingers to the box begins at a suitable age to say 'box' when the child is handed the box, 'open box' when the child opens it, and 'close box' when he closes it and 'put blocks in box' when that act is executed. This is repeated over and over again until the conditioned reflexes are thoroughly established. In the course of time the box in front of him, which originally called out only bodily habits, now begins to call out word habits. He says 'box' when it is handed to him, 'open box' when he opens it. The sight of the box now becomes a stimulus capable of releasing either the bodily acts or the word acts, or both. A series of functional connections is established between vision and throat and laryngeal muscles which exists alongside of the already earlier established connections which run from the same receptor to the arm and leg muscles. When the box is presented now, which act will take place? Manual action or laryngeal action? It is at this point that the influence of the environment upon the shaping and forcing of language habits comes clearly to the front. There comes an occasion when the box can be seen but not reached. Action in the hand is blocked. He speaks 'box' and may speak it persistently all over the house. The attendant, hearing the word box, hastens to put it in his hands. In view of the fact that this situation is repeated day in and day out, not only with respect to this object but to hundreds of others, the child learns that the uttered word is a sufficient stimulus to cause the attendant to hand him the objects he names without actually having to execute bodily movements with respect to them. There has been a substitution of the language habit for a bodily habit—now by a word he can cause adults to move—his grunt or growl or infant toy word is law."[1]

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[1] B. 27.

On the ordinary view, which admits the appeal to the consciousness of the agent, the child has learnt that the word 'means' the thing, and the utterance of the word becomes an instrument in the service of a conative trend. But the Behaviourist has no business to know that the child 'desires' or 'wants' the box. The consequence is, that if the Behaviourist is to be faithful to his initial abstractions and keep within the limits he has laid down, he must define his substitute-response simply by the place it occupies in the general 'pattern' formed by all responses implicit and explicit. But what is there in the nature of a laryngeal response as such which can possibly label it as a substitute for a response of 'the larger musculature'? The only differentiating circumstances possible are order in time and frequency of accompaniment, and their insufficiency is too patent to need comment. Consequently the notion of substitution does not seem to work, and it is by no means clear what 'relevancy' could mean in the theory under discussion.

Similarly thinking could not be regarded as a case of trial and error. Error surely means that the agent does not get what he wants, and appetition and conation are outside the Behaviourist scheme of things The Behaviourist is attacking his problem by the aid of only one eye and one hand, having two eyes and two hands at his service.

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[1] P. 320–1.

IS THINKING MERELY THE ACTION OF LANGUAGE MECHANISMS[1]? (V.)

By JOHN B. WATSON[2].

1. *A correction of statement.*
2. *More comprehensive use of the term 'thinking' demanded.*
3. *Illustration of thinking made overt.*
4. *Behaviourist's right to assume that a process of implicit thinking goes on.*
5. *Further elaboration of the process of thinking; some objections reviewed.*
6. *'Conceptual' thinking really a fallacy.*
7. *'Meaning' an experimental problem and not a problem of philosophy or of speculative psychology.*
8. *Conclusions.*

1. A CORRECTION OF STATEMENT.

BEFORE attempting to define further in this Symposium the behaviourist's position on thinking, it would seem best to discuss for a moment some of the statements the behaviourist has already made. In advance of any argument I think we can say that he has never really held the view that thinking is merely the action of language mechanisms. Possibly my own loose way of writing may have lent colour to such a view. I frankly admit that in a number of paragraphs in a recent book I may justly be accused of having given an affirmative answer to the question before the Symposium. I can make only the well-worn excuse that my over-emphasis was indulged in for the sake of sharpness of presentation before elementary students.

In psychology we can rarely make a complete observation of everything that a human being does at any one moment; and in giving an account of what happens we emphasize those points which the experiment was designed best to bring out. This is what I meant to do in my previous discussion of thinking. I gladly emend any statement I may hitherto have made as follows.—A whole man thinks with his whole body in each and in every part. If he is mutilated or if his organs are defective or lacking, he thinks with the remaining parts left in his care: but surely he does everything else in exactly the same way. If one studies a game of tennis, one's observation is taken up with the type of strokes the player makes, his serve, his returns, the way he covers the court, etc. In other words, arm and leg activities are principally emphasized. However, everyone admits that the player is using every cell in his body during the game. Nevertheless if we sever a small group of muscles in his right arm his playing is reduced practically to that of a novice[1]. This illustration serves us very well in explaining why one emphasizes laryngeal processes in thinking. Surely we know that the deaf and dumb use no such laryngeal processes, nor does the individual whose larynx has been removed. Other bodily processes have to take on the function of the larynx. Such functions are usually usurped by the fingers, hands, arms, facial muscles, muscles of the head, etc. I have in another place emphasized the extent to which finger and hand movements are used by the deaf and dumb when they are engaged in silent thinking. Mr Thomson in his paper before the Symposium seems more or less to subscribe to the same view. It would be an easy experiment, but so far as I know not hitherto tried, to bind the fingers and arms of such an individual and then give him a problem in arithmetic, memorizing simple stanzas, and the like, which have to be worked out without exteroceptive aid. It would be necessary probably to tie down eye movements, were such a thing possible, and to restrain even the head and intercostal muscles.

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[1] A contribution to the Symposium presented at the Congress of Philosophy in Oxford, 24–27 September, 1920.

[2] The writer had not the opportunity of seeing the preceding contribution to this Symposium.

While there is no sacrosanct reason why thinking should go on in normal individuals in the muscular fields of the larynx and throat, there are two very practical ones. There is first the genetic fact that, from childhood onwards, organization has been forced in the direction of language activity. From the third or fourth year onwards probably a thousand language adjustments are made to one manual adjustment. There is, too, a biological reason. This arises from the fact that the human being in his early struggles for existence had to have the undivided use of arm, finger and hand musculature when hunting and fighting. If he had had to employ the large muscles in thinking as, I am convinced, deaf mutes do, manual activity would have been interfered with at critical times. I have never had the opportunity of observing deaf mutes in a fight or in a critical situation where both thinking and delicate action of the manual type were demanded.

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[1] There remains to the player of course a large organization—his verbal organization especially is left intact as is the training his eyes have received. He is able to act as umpire, to pass judgment upon playing, write manuals on the structure of the game, etc. Probably he could learn to play with his left hand much more rapidly than a poor player who had suffered a similar loss, in view of the fact that his leg and trunk organization is relatively intact.

2. MORE COMPREHENSIVE USE OF THE TERM 'THINKING' DEMANDED.

The question is often asked what marks off thinking from the mere subvocal unwinding of well-organized language habits. Mr Bartlett and Miss Smith have brought out this question explicitly and have formulated an answer which is not satisfactory to me. I think we ought to make the term 'thinking' cover generally all implicit language activity and other activity substitutable for language activity. [It should be admitted furthermore that under proper stimulation (usually a request is sufficient) the subject can be made to think aloud.] Thinking would comprise then the subvocal use of any language or related material whatever, such as the implicit repetition of poetry, day dreaming, rephrasing word processes in logical terms, running over the day's events verbally, as well as implicit planning for the morrow and the verbal working out of difficult life situations. The term 'verbal' here must be made broad enough to cover processes substitutable for verbal activity, such as the shrug of the shoulder and the lifting of the brows. It must embrace the implicit movements involved in written words or the implicit movements demanded in the use of the deaf-and-dumb sign manual, which are, in essence, word activity. Thinking then might become our general term to cover all subvocal behaviour. It is obvious that this definition can take care of the most mechanical and deeply grounded of our language habits such as those used in the subvocal repetition of childhood verse, the repetition of stanzas of poetry, limericks, etc.; those depending more particularly upon emotional stimuli as day dreaming, as well as those verbal processes not completely habitual such as the working out of a lecture, the planning of a book; and finally those in which new results are brought out. It is clear that if in the interests of systematic psychology we need to sub-divide the whole process of thinking, three lines of cleavage will at once appear.

1. Mere unwinding of vocal habits where the word sequences are invariable: illustrated by hymes, quotations; by many of the responses in mathematics, as 2 and 2 equal 4, square root of 9 equals 3, and the like. Here there is no new work, no trial movements like those we see

in overt manual activity when a new situation capable of solution is presented the first few times. Such thinking corresponds to an extremely simple stimulus and response type of behaviour. Similarly day dreaming would fall under this division. We assume that such dreaming takes place in response particularly to deficiency stimuli of one kind or another; such as the absence of sex activity, lack of food and water, lack of habitual surroundings and companions, lack of drugs, or even under the sway of drugs.

2. The solving of problems which are not new, but which are so infrequently met with that trial verbal behaviour is demanded; illustrated probably by thinking out of stanzas, partially forgotten; in trying to apply one mathematical formula after another in a particular problem at hand. All of the part processes have been met with by the individual and are part of his organization, but he cannot use these part processes with machine-like facility.

3. Finally we have the extreme extension of 2 above. Here the problem is new and the organism when confronting such a problem is in a grave situation. We will suppose, for example, that a man loses his position and wealth suddenly and must be ready in a few hours to act explicitly in a new undertaking. The problem, it is assumed, is of such a character that it must be worked out verbally before any overt action can take place. Hundreds of examples of this type immediately suggest themselves. Most of the real social and moral problems appearing in one's life are exactly of this type[1].

These subdivisions are really guesses as to what may go on. No scientific division is as yet possible. It should be expressly stated, furthermore, that thinking in any of the above forms is not an isolated process. A human animal never gets away from his biography; and the varying organic and emotional states the organism is in must exert a tremendous influence upon the course of his thinking. So that once more we would emphasize the fact that thinking, whatever its type, is an integrated bodily process.

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[1] I am rather startled by the fact that all of the writers in the Symposium seem to find some confusion in my use of the term 'habit.' They maintain that I apparently imply by the term a fixed or invariable chain of responses. Of course there are a few such invariable sequences in every human being but the number is not large. When used in this sense I quite agree with them that it is opposed to thinking if we mean by thinking the solving of problems such as those indicated in my third division. I have generally made the term habit coextensive with that part of an individual's organism which is not hereditary; but surely in all learning there is a display of previous organization—of habits (and hereditary activity) most closely connected with the type of situation confronting the learner. No single response already learned (habit) will bring about the present adjustment—there must be a recombination. But the partial habits forming the new whole adjustments, be they laryngeal or manual, have each had a history and their origin can often be traced.

Probably not many of my colleagues would include 1 and 2 under the term 'thinking.' Thinking has come to be identified with 3 of our division, but for no valid reason. We use the term manual activity when our subject ties his shoe strings in exactly the way we use it when he is *learning* to manipulate (for the first time) the most complicated of machine-gun mechanisms. In our opinion 3 represents a bit of behaviour on the part of the human animal which, when stripped of its unessentials, is exactly like that bit of behaviour which the rat exhibits when put into a complicated maze for the first time. When it gets to the food the autonomic strivings die down and it goes to sleep. The deficiency stimuli, lack of food, lack of usual surroundings, etc., cease to operate—the adjustment is complete. Surely a similar thing takes place in man. He works verbally (that is particularly verbally; many other processes go on of course, such as wrinkling the brow, tearing the hair, etc.) until certain verbal acts ('conclusions') are executed. If, when this conclusion is reached, the driving stimuli (verbal, autonomic, emotional, etc.) cease to operate, the adjustment has been completed.

3. ILLUSTRATION OF THINKING MADE OVERT.

The present writer has often felt that a good deal more can be learned about the psychology of thinking by making subjects think aloud about definite problems, than by trusting to the unscientific method of introspection. Usually a scientific man is quite willing to enter into the experiment with zest. If I ask my subject in 1 (see page 90) to think aloud he overtly responds with his limerick, his day dreaming or his mathematical answer. Similarly if I ask him to think aloud in 2, I notice hesitations here and there, false starts and occasional returns, but in general a fairly ready response occurs with relatively few errors. It is only when we ask him to think aloud in 3 above that we begin to grasp how relatively crude is the process of thinking. Here we see typified all of the errors made by the rat in the maze: false starts appear; emotional factors show themselves, such as the hanging of the head and possibly even blushing when a false scent is followed up. The subject returns again and again to his starting point as shown by his asking, "You say the given facts are so and so?" The experimenter says "Yes" and again the subject starts off. In conducting an experiment of this kind, one has to be careful to impose problems upon his subject which are as far as possible removed from repressed emotional factors. It is never possible of course completely to do this as the analysts have more than once pointed out. The following illustration will make clear some of the points which appear in overt thinking.

A colleague of mine came on a visit to stay in an apartment in which I had rooms. In a passage leading from the shower bath was a peculiar piece of apparatus standing near a sink. The essential features were a curved shallow nickel pan about twelve inches wide by twenty inches long; at one end the pan had been bent in in the form of a half circle, while at the other end the side pieces did not extend for the full width. The pan was mounted on a stand adjustable in height. Furthermore the pan itself was attached to the stand by a ball and socket joint. My friend had never seen anything like it and asked me what in the world it was. I told him I was writing a paper on thinking and pleaded with him to think his problem out aloud. He entered into the experiment in the proper spirit. I shall not record all of his false starts and returns but I will sketch a few of them. "The thing looks a little like an invalid's table, but it is not heavy, the pan is curved, it has side pieces and is attached with a ball and socket joint. It would never hold a tray full of dishes (*cul de sac*). The thing (return to starting point) looks like some of the failures of an inventor. I wonder if the landlord is an inventor. No, you told me he was a porter in one of the big banks down town. The fellow is as big as a house and looks more like a prize-fighter than a mechanician; those paws of his would never do the work demanded of an inventor" (blank wall again). This was as far as we got on the first day. On the second morning we got no nearer the solution. On the second night we talked over the way the porter and his wife lived, and the subject wondered how a man earning not more than $150 per month could live as our landlord did. I told him that the wife was a hair-dresser and earned about eight dollars per day herself. Then I asked him if he did not see the sign 'Hair-Dresser' on the door as we entered. The next morning after coming from his bath he said, "I saw that infernal thing again" (original starting point). "It must be something to use in washing or weighing the baby—but they have no baby (*cul de sac* again). The thing is curved at one end so that it would just fit a person's neck. Ah! I have it! The curve does fit the neck. The woman you say is a hair-dresser and the pan goes against the neck and the hair is spread out over it." This was the correct conclusion. Upon reaching it there was a smile, a sigh and an immediate turn to something else (the equivalent of obtaining food after search).

4. BEHAVIOURIST'S RIGHT TO ASSUME THAT A PROCESS OF IMPLICIT THINKING GOES ON.

Notwithstanding the fact that we can make our subjects think aloud and thereby can observe a large part of the process of thinking, Titchener some years ago raised against an early paper of mine the objection: "How does the behaviourist know there is any such process as thinking since he cannot directly observe it?" Titchener kindly answered this question, to the effect that the behaviourist—*quâ* behaviourist—doesn't know that there is any such thing as thinking. The introspectionist claims that the behaviourist first uses the good old-fashioned method of introspection to find thinking and having once found it shuts his eyes and turns his back upon his original method and begins to externalise the process and to put it in the universal language of science. In other words, he describes it merely as the functioning of laryngeal or other motor processes.

Before coming to closer terms with this question, the behaviourist would like to posit the assumption, without discussing its many metaphysical implications, that in no physical or biological science is the fact called into question that the investigator can make an observation; for example, that he can note that his galvanometer needle has swung two degrees to the right, that when sodium is burned on the end of a glass rod the bright visual stimulus in the spectroscope will be located on the scale at $5800\mu\mu$: that the physiologist can observe that when such and such a thing is done to an animal whose heart rate is being recorded the rate has decreased or increased. He can also make the same observations on the changes in his own heart rate due to the use of different types of drugs. He can do this either by counting his own pulse or better by attaching himself to some form of recording device. In each of these sciences the observer goes on in his care-free way, accumulating a series of systematic observations. He does not do this in any hit-or-miss way. A definite stimulus starts him upon his work—the words of the professor over him, or the written or spoken word of an antagonist, or finally some inward organization exerts its pressure. He works, for example, with the effects of strychnine upon human or animal organisms, because he has had some initial stimulus to drive him to that work. Once started, the changing results he obtains serve as a stimulus for further work. Finally he groups his facts and a bit of organized science is the result, namely a monograph upon the effects of strychnine upon living organisms. If you ask him, or the physicist who has worked up a monograph in a wholly similar way upon the spectroscopic analysis of certain compound substances, "Did you realise that there was an observer implied during all your manipulations?" he would probably not know what you meant and he would certainly be mildly angered if you happened to interfere during his working moments with such a question. In other words he gets along without discussing or even being interested in the fact that there is an implied observer at every moment in science and that a thousand interesting metaphysical points lie behind an individual's ability to make observations.

The behaviourist likewise shuts his eyes to the same metaphysical question and asks only to be allowed to make observations upon what his subjects are doing under given stimulating conditions. On the metaphysical side he asks merely to be put into the same basket with other natural scientists. The introspectionist has never made this plea to the metaphysician. He has assumed that the question of the observer is a psychological one and that he has the answer to it. The behaviourist is not so bold. He is engaged in studying, among other things, the process of observing as it appears in others, where the activity is not complicated by the demands of introspection. He must, as must the introspectionist also, assume that his own process of observing is the same as that of the subject whom he is studying. He hopes ultimately to give an adequate account of the process in this subject, an account which will show how even those phenomena which the introspectionist describes as his 'consciousness' result from the complexities of behaviour.

The introspectionist hopes for a solution of the metaphysical problem through some mystic self knowledge. The behaviourist believes in no such transcendental human power. He himself is only a complex of reacting systems and must be content to carry out his analysis with the same tools which he observes his subject using. I cannot, therefore, agree with Mr Thomson that there is a mind-body problem in behaviourism. It is a serious misunderstanding of the behaviouristic position to say, as Mr Thomson does—"And of course a behaviourist does not deny that mental states exist. He merely prefers to ignore them." He 'ignores' them in the same sense that chemistry ignores alchemy, astronomy horoscopy, and psychology telepathy and psychic manifestations. The behaviourist does not concern himself with them because as the stream of his science broadens and deepens such older concepts are sucked under, never to reappear.

Granting then that the behaviourist is a natural scientist and makes his observations upon his fellow man rather than upon himself, utilising the aid of instruments whenever possible or necessary, like any other scientist—how does he arrive at the concept of implicit thinking? The answer is that he can *at present* arrive at it only by making use of a logical inference. In those cases where the response to the stimulus is not immediate but where it finally occurs in some form of explicit verbal or manual behaviour, it is safe to say that something does go on, and that that something is surely not different in essence from that which goes on when his behaviour is explicit. Let us glance for a moment at a manual illustration. I hand a friend a gold cigarette case which can be opened only by pressing a secret spring. I tell him that he can keep the case if he can open it without violence. I watch him for two minutes, noting his rambling trial manipulatory movements. He fails to open it in this period of time. I then place him in a room alone, and tell him to come out when he has opened it. At the end of thirty minutes he emerges smiling and with the case open. Since there are no marks of violence on the case, the behaviourist, utilising logic, has a right to assume that the subject continued to work at the problem as he had been trained to work at such problems and that his behaviour in the empty room was essentially the same as that exhibited by him when he was under direct observation. Merely because observation of his behaviour could not take place so long as he was hidden from the observer gives no one the right to assume that any different or unusual process went on. I should not hesitate to call this behaviour on the part of our subject manual thinking or non-language thinking. There is no necessity for it, however, since our categories of trial-and-error learning, functioning of habit, etc. are adequate. I suggest manual thinking here to show its complete homology with that type of behaviour described below which is more universally called thinking.

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Suppose instead of giving him a problem which can be learned by manual trial-and-error manipulation I say, "What would be the result on your social and vocational life if through some accident you suddenly had both arms removed?" Assuming, as would be safe in most instances, that such a problem had not hitherto been faced and formulated, he would be unable to give any adequate statement. Suppose we insisted upon a formulation. At the end of an hour he would probably be able to return a fairly comprehensive reply. Surely I have the right to assume, even as a 'despised' behaviourist, that implicit language activity, sensori-motor in character, has been taking place during the hour on as grand a scale as overt bodily movements would have been taking place had I left him in a room from which there was no obvious exit and suddenly yelled "Fire!" from the outside. I infer that language activity from infancy onward has been developed just to meet such situations; hence that during the period of his apparent immobility he was using implicit language processes. Such processes are the only available types of organization which we have any objective right to assume can be used in such a situation[1].

Some unpublished results of experiments by my colleague, Dr Lashley, begin to approach a scientific proof that essentially the same type of responses goes on in implicit thinking as goes on in more explicit types of verbal response. With a delicate apparatus which recorded the tongue movements in two dimensions he was enabled to show that the overt but whispered repetition of a sentence produced a tracing on the smoked drum which was wholly similar except for amplitude to that obtained when he told the subject to think the same thing without making overt movements. He was enabled to verify this again and again. On the other hand if he obtained a standard tracing to a whispered sentence and then gave the subject other work to do and later came back and asked him to think the sentence, there was no obvious correspondence in the two tracings (the original motor set had changed). This is not an argument against our point for I have already shown elsewhere how varied is the musculature of the larynx and the throat. We can write the same word by a dozen different combinations in the holding of the pen. We can speak or think the same word by many different muscular combinations.

I am not afraid, furthermore, of yielding too much to our friendly enemies the introspectionists when I say that the subject himself could observe during the apparent immobile period that he used words and sentences (and that for a part of the time he did not know what he was using!). I am no more afraid to admit this than I am to admit that a person can observe that he himself is laying bricks or playing a piano. I have elsewhere admitted a verbal report method but at the same time I have insisted upon its untrustworthiness for scientific purposes. To know anything worth while for science about my brick-laying I must get a Gilbreth or some other observer to record by motion pictures or otherwise my every act while laying bricks. In other words, scientific conclusions demand instrumentation. I can observe roughly that I have raised a wall four feet high by my day's work, but I cannot determine how many millions of useless movements I have made or how these useless movements could be eliminated by a change in my method of work. Now I hold that the same thing is true of thinking. The subject can observe that he is using words in thinking. But how much word material is used, how much his final formulation is influenced by implicit factors which are not put in words and which he cannot himself observe, cannot be stated by the subject himself.

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[1] In other words, since our assumed explanation is simple and straightforward and adequate to account for all the facts and is in line with what can actually be observed in other activities, the law of parsimony demands that the upholders of 'imagery' and 'imageless thought' should show the need of such 'processes' and demonstrate objectively their presence.

The behaviourist, as well as the psychoanalyst, holds that there are hundreds of such factors involved, some of which require a minute search into the subject's biography as far back as infancy before any adequate answer can be returned. Now two or three years' training in introspection on the observation of thought processes will take our subject no further. It has been abundantly demonstrated, both by the failure of psychologists to get anywhere in the problem of thinking and by psychoanalysts, that such methods simply will not yield results. Such training merely makes him pedantic and insufferably prolix and descriptive of his inward processes. The point I am headed toward here is that if we are ever to learn scientifically any more about the intimate nature of thought other than that which can be obtained by observing the end results—that is, by observing the overt verbally expressed behaviour or the overt ensuing bodily actions—we shall have to resort to instrumentation. The time seems far off when such a thing is possible. While awaiting it the behaviourist has ample with which to occupy himself. Furthermore he is not in such bad straits after all. The physiologists in many cases have to be content with their observations of end results. We know many factors which affect the functioning of the parotid gland. We count the drops of saliva which issue from it under varying conditions of stimulation. We analyse the chemical changes occurring, etc. But what goes on in the gland itself we cannot say. But no one would have the temerity to assume that for this reason there is no physiology of the gland. We can speculate about what goes on inside of the gland, what the function of the unstriped muscular tissue is, why the solution is now thick, now thin, whether the gland would secrete if this or that were done. But those speculations to be of any value must be couched in some kind of terms which will lead not to metaphysical fancies but to some kind of experimental attack. If they do not lead to an experimental attack, no physiologist will long entertain them. I feel that we are in exactly this same position with regard to thinking.

5. FURTHER ELABORATION OF THE PROCESS OF THINKING; SOME OBJECTIONS REVIEWED.

The behaviourist believes that thinking in the narrow sense where new adjustments are made corresponds to the trial-and-error process in manual learning. The process as a whole consists in the organized interplay of laryngeal and related muscular activity used in word responses and substitutive word responses; that is, the motor stage is not always necessarily situated in or even near the larynx. I would write up the process as I infer that it goes on somewhat as follows, drawing my analogy from the wealth of facts we have collected about manual activity. If I hand a subject a mechanical problem box rather large in size and ask him to solve it, I note the movements of the hand, the wrist and even the large muscles of the shoulder as he turns the mechanism from side to side. If, before he finishes solving it, I hand him the same apparatus only reduced to one-tenth of its size he continues his manipulations in approximately the same way, but the amplitude of the muscular response is greatly reduced and many of the movements of the large muscles drop out. The two types of activity are, however, in essence essentially the same. When it comes to thinking we have the following facts: children in large measure think aloud and many adults either think aloud or, if not quite aloud, at least overtly. In others thinking is reduced to such an extent that the bystander can observe only the response of the lips, the jaws and occasionally tongue movements. But the great majority of subjects pass beyond this stage and all observable explicit activity directly connected with the process of thinking disappears (there may still be explicit factors remaining such as walking, wrinkling the brow, sweating, etc.). Having watched in genetic psychology the growth of such processes, having made numerous individuals think aloud in solving their problem, what right have I to assume that the process entirely changes its character when it becomes implicit? Here I call attention to Mr Pear's analysis. He says that the behaviourist catches only the perchings of thought, "When we recall Professor James's description of thought as a series of flights and perchings, it seems that the behaviourist has given us an account of some kinds of perchings, and, fascinating as it is, it reads like a description of flying by an aerodrome mechanic, who sees only the last stages of the aviator's descent." But surely Mr Pear here is hoisted by his own petard. It would be unkind to rob his remarks of their sting by saying that only a well trained aerodrome mechanic can give, after watching a descent, a scientific description of it. The question

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I would ask Mr Pear is, what logical right has he to assume that the flight goes on in any different way when it is not under the observation of the mechanic? Surely if we have enough mechanics stationed along the course to watch the entire flight, their combined report would be a faithful account of the flight as a whole. William James's account of transitional states and perchings illustrates very well a fallacy into which Mr Pear and nearly all other psychologists fall, viz., if any part of the process is beyond the range of the bystander's immediate observation he, the bystander, has the right to assume that something unusually interesting and mysterious may go on at the unobserved points. But since the mysterious never happens when the process is under direct observation, the logical fallacy of assuming that something different does go on is obvious. The motive behind James's classical illustration is not difficult to find. It is the motive behind the resistance to the behaviourist's view of thought and its roots lie in mysticism and early religious trends.

Another similar fallacy runs through both Mr Pear's paper and that of Mr Bartlett and Miss Smith, namely that the *expression* of a thought in some kind of implicit or explicit verbal action or in general bodily movement is not necessarily *thought*. Mr Pear uses the illustration of a skater making the figure eight, whereas Mr Bartlett and Miss Smith show dissatisfaction with my simple illustration of a golf player. The figure eight, Mr Pear tells us, is not skating, but is the result of an act of skating. The roots of these objections lie in the fact that these authors are discussing behaviourism not from the behaviourist's own premises but from those of a structural psychologist. Why should a scientific observer of *skating* stop, upon beholding the figure eight made by some particular performer? He might wonder at its regularity, its smoothness and the like, but he would say, "My quest is the goose that laid this golden egg." In studying skating, he would take up the whole system of responses of the skater from and including the moment of fastening on the skates until they were removed. His observation would concern itself with arm and leg movements, the way the ankles function, the compensatory movements of the trunk, with the effort put forth by the skater as shown by the ease and grace of the movements, with the fact as to whether he was perspiring or whether he showed only signs of exhilaration or other emotional changes, etc. Nor would he neglect the tracings made on the ice by the skater's various movements. He would go further and take up the question of the type of training required for such adeptness, of the length of the training period and the age at which such training should begin. In other words, his final data would be sufficient for answering all questions which might be asked about the whole process of fancy figure-skating. After he had made a complete and searching analysis, what would be lacking? The individual's own account, of course. For the sake of completeness we will take it down. Our claim is that, in the great majority of cases, a report by the subject throws very little light upon the act he is engaged in. Let us ask him the question, however, "What were you thinking about while you were skating?" Holt has brought out in his *Freudian Wish* the reply one usually gets to such a question. I shall take the liberty of rephrasing Holt's example so that it will fit the present case. "What was I thinking about? I was wondering whether that 'queen' over there in the red sweater was watching me"!

In a similar vein Mr Bartlett and Miss Smith object to the following statement of mine: "When we study implicitly bodily processes we are studying *thought*; just as when we study the way a golfer stands in addressing his ball and swinging his club we are studying *golf*." Their objection appears in the following words: "But to say that we are studying 'golf' in the second case is to assume that 'golf'—the structure and character of the game itself—is identical with how a given player *plays* golf." I fail to see any special force in this objection. What I want to know when I have a given individual under observation is how *he* thinks or how *he* plays golf. Perhaps I should have worded it differently: When we study an individual's implicit bodily processes we are studying his thinking; and when we study the way a golfer stands in addressing his ball, in swinging his clubs, etc., we are studying the way he plays golf. But let us study many other individuals, both their implicit bodily processes (thought) and their golf playing. Let us write down what we see, and record movements in motion pictures and use all possible methods and instrumentation in our quest. In the end we shall arrive at a monograph on thinking and at another on golf playing. Destroy all books on golf and a man from Mars in a month's time, never having seen the game, could, by watching individuals play, write a decent manual on the rules, structure and *technique* of golf. After having made as searching an analysis as we like upon several players' playing of golf, what will be left out? The individuals' own accounts. Again suppose we take down their overt responses to any questions we may ask and incorporate them into our record. They are of relatively little value. No one since objective studies upon golf have been made trusts the verbal report of a golf player. He will tell you that he never takes his eyes off the ball when making a stroke. The cinema shows that he is a prevaricator. I have never been able to get one valuable scientific statement from a golf player. He does not know how he holds his hands, he cannot tell how he stands, nor the arc he makes with his club, nor whether the arc can vary within wide limits and not affect his stroke. He knows practically nothing about the condition his body is in. To verify this, one needs only to play with a man whose driving has gone off a bit and who has to resort again to trial and error for correction. He asks after every failure, "What did I do that time? Did I bend my body? Did I move my foot?" and so on. A most interesting illustration of the failure of a tennis player to be able to give any worth-while verbal report came to my hands while preparing this paper. *A* took up tennis again after a ten-year period of non-practice. He played against *B*. On the first day his form was pitiful to behold. He stooped at every stroke and twisted his body in every conceivable way. He played five sets and failed to get a game in any set. The score was deuce on only two occasions. On the second day the score was deuce several times and he won one game. He put over several good serves and his form showed great improvement. On the third day there was again steady improvement in form. The returns were swift, and fully fifty per cent. of his first serves were good. On the fourth day he won three games in succession but he was still unable to win a set. All the way through he was terribly discouraged. He had formerly been a fair player with a good serve. He kept saying to his opponent, "I play worse than I did the first day, my wrist is not flexible, I can't get the knack of serving the ball the way I used to, I have forgotten how and when to play the net and to place my balls." It was not until *B* pointed out the objective facts indicated above that *A* was convinced that his playing had improved.

It would be folly to say that in no case is a verbal report wholly without service. To enumerate the places where it is of service is not particularly pertinent to our present discussion.

6. 'CONCEPTUAL' THINKING REALLY A FALLACY.

Mr Bartlett and Miss Smith have, I fear, harkened too much to the logician in their treatment of so-called general relations. They find fault with my simple illustration of building a bridle path. The statement they object to is as follows: "...if the grade is too steep I build my road around the side of the hill." I quote their criticism: "But the real fact of the case is concealed in that statement. In so far as the response is a thought response it is definitely a response to *steepness*; not merely to a particular set of visual reactions, because that would not lead on, of itself, to the further set of muscular and other reactions involved in making the path round the hill; not merely to the steepness of *this* hill, because that also would not take me round it; but especially to *steepness* as a quality common to this and to other situations and independent of any particular context." From the whole history of the way responses grow up I cannot yield this point, and yet it probably would be assented to by most psychologists: Mr Thomson I think has come to my rescue upon this, and I believe he would assent to my further elaboration. One of the first stumbling blocks I had in structural psychology was its treatment of concepts and general ideas. Long before behaviourism took me in tow, I came to the conclusion that such things were mere nonsense; that all of our responses are to definite and particular things. I never saw anyone reacting to tables in general but always to some particular representative. When I began to watch how a child learns to react to words denoting (from the standpoint of logic) a class, the process became clear. When he had his arms full of toys and the stimulus for depositing them was present, his mother would say, "Put them on the table," whether the table was one-legged, an extension table, a library or dining table. The word thus becomes conditioned. The word *table* (any class or abstract word such as animal, justice, mercy, infinity has the same history) becomes thereafter a single individual object, a part of his world of objects, ready to call out a *single definite response* (appropriate to the situation he is in) when he speaks it himself, thinks it or hears it spoken.

In a similar way the definite reaction to the word 'steepness' grows up. The lad takes a walk with his mother over stretches where there are no paths. When he goes up a hill he pants and blows and sweats. His mother says, "Steep, isn't it?" *Steep* becomes substitutable for panting and blowing and sweating. They come to another hill. The mother says "Steep, isn't it? You are tired; let's go round." He learns by trial and error that the word *steep* is followed by sweating, hard work and tired limbs and that this exertion can be avoided by turning to the right or left and circling instead of keeping straight on. When, interested in constructing a bridle path after reaching adult life, he comes to a hill, his whole organization is such that the hill itself (the situation) calls out the word *steep* (conditioned) and steep in turn calls out "turn right or left and circle." I can see nothing in his reactions not explainable by conditioned word responses and simple trial-and-error learning.

As Mr Thomson points out, after reaction to such situations has become habitual, merely being in a situation where he is confronted by a hill leads him to the correct response, namely, circling up its side. Thinking in the sense of implicit word processes need not go on at all. I think, then, we need not agree with Mr Bartlett and Miss Smith when they say, "But what, in this instance, switches me off from the series 'going in this direction' to the series 'going in that,' is the response to a universal quality or relation. That, and that alone, gives us the peculiar characteristic of thinking."

7. 'Meaning' an Experimental Problem and not a Problem of Philosophy or of Speculative Psychology.

This type of argument brings us perilously close to the so-called problem of meaning. I should like to say frankly and without combativeness that I have no sympathy with those psychologists and philosophers who try to introduce a concept of 'meaning' ('values' is another sacred word) into behaviour. At every point we would describe all of psychology in terms of what we see the organism doing. The question of meaning is an abstraction, a rationalisation and a speculation serving no useful scientific purpose. In our seminary at Johns Hopkins University during the past year we went over the various formulations of meaning of the psychologists and philosophers. A more barren wilderness of words it has never been my lot to meet. From the bystander's or behaviourist's point of view the problem never arises. We watch what the animal or human being is doing. He means what he does. It is foolish to ask him while he is acting what he is meaning. His action is the meaning. Hence, exhaust the concept of action and we have exhausted the concept of meaning. It is a waste of effort to raise a problem of meaning apart from actions which can actually be observed. To answer what the church means to men it is necessary to look upon the church as a stimulus and to find out what reactions are called out by this stimulus in a given race, in a given group or in any given individual. Parallel with this query we can carry out another as to why the church calls out such and such responses. This might take us into folk lore and into the influence of the code upon the individual, into the influence of parents upon children, causing the race to project the father and mother into a heavenly state hereafter, finally into the realms of the incest complex, homosexual tendencies, and so on. In other words, it becomes like all others in psychology, a problem for systematic observation and experimentation. I have emphasized these general statements about meaning in this connexion because it is often said that thinking somehow peculiarly reveals meaning. If we look upon thinking as a form of action comparable in all its essential respects to manual action, such speculations concerning meaning in thinking lose their mystery and hence their charm.

8. CONCLUSIONS.

Thinking is then largely a verbal process; occasionally expressive movements substitutable for word movements (gestures, attitudes, etc.) enter in as a part of the general stream of implicit activity. Thinking, in the narrow sense where learning is involved, is a trial-and-error process wholly similar to manual trial and error. Verbal manipulation along one line is checked and stopped and a new line is begun for exactly the same reasons that such processes are checked and begun in manual learning (so-called processes of control[1]). The thinking adjustment is completed when the final word-grouping (sentence or judgment) or overt bodily reaction which comes as the end result of the process of thinking makes the initial stimulus to thinking inoperative or inert; that is, the final reaction, verbal or other, so changes the general state of the organism as a whole that the original stimulating factor can no longer affect the subject. A crude illustration which can properly be carried over to thought is to be found in the hungry hunter's eager search for game. He finds it, captures it, prepares and eats it, lights his pipe and lies down. The hares and quail may peek at him from every corner of the brush, but their driving power for the time is gone.

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[1] Situations *plus* training and organization (the individual's biography) are the only 'control' factors we need in psychology—either for regulating overt bodily action or implicit thought action.

ON LOCAL FATIGUE IN THE AUDITORY SYSTEM.

By J. C. FLÜGEL.

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  6. *Transference of fatigue.*
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  9. *Does uniaural fatigue influence binaural hearing?*
  10. *Relation between localisation and intensity.*

III. Local fatigue as affecting the intensity of sounds.
  1. *Methods.*
  2. *First results.*
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  4. *Influence of loudness.*
  5. *Influence of pitch.*
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  7. *Influence of consciousness.*

IV. Summary of principal results.

I. INTRODUCTION.

THE phenomena of local fatigue have already been the subject of considerable study from the neurological point of view[1]. Thus it has been shown that some reflexes tire more rapidly than others, that different reflexes may manifest their fatigue in different ways (tremor, phases of strong and weak contraction, slowing and irregularity of rhythm, intermission followed by temporary recovery, etc.), that a reflex in a fatigued state may suddenly give place to an antagonistic reflex as soon as the stimulus is removed or even during its continuance, and that the fatigue is often of an extremely specific character, dependent on the particular area of skin that is stimulated (though not in this case necessarily arising in that area). On the psychological side, too, analogous observations have been made, notably by McDougall[1] in the case of binocular rivalry and in illusions of reversible perspective as manifested in Sinsteden's windmill experiment; while the work of many other investigators, though less specifically devoted to this subject, is not without bearing on it.

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[1] *Vide, e.g.*, Sherrington, *The Integrative Action of the Nervous System*, 214 ff.

Although the study of local fatigue may well appear to be of less obvious and immediate interest than the study of fatigue of a more general nature, it is nevertheless calculated to throw light indirectly on many important problems of work, *e.g.* inhibition and recuperation, and it is perhaps a necessary preliminary to a true understanding of general fatigue (should such a thing exist). Furthermore, when we possess a sufficient number of careful observations on the different kinds of local fatigue both from the physiological and from the psychological sides, a proper classification of the results will probably afford us a considerably increased knowledge of important elementary characteristics of nervous and mental action and of the correlation between them. Further observations on this subject should therefore, it appears, be welcome.

Having myself become familiar with the psychological manifestations of local fatigue through a previous study[2], in which I had repeated and extended McDougall's observations on the windmill, it appeared to me, while assisting in some experiments on the localisation of sound, that the process of binaural localisation was liable to be affected by fatigue in much the same way as illusions of reversible perspective. The experiments in question were undertaken in connexion with the war research then being conducted in the Psychological Department of University College under the direction of Professor Spearman. On the conclusion of this work I was able to take up the problem more systematically than had before been possible, using a portion of the apparatus which had already been devised for the previous researches and which Professor Spearman kindly placed at my disposal for this purpose.

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[1] "The Physiological Factors of the Attention Process," *Mind*, 1906, xv. 329. Especially 340 ff.

[2] "Some Observations on Local Fatigue in Illusions of Reversible Perspective," this *Journal*, 1913, vi. 60.

My conviction that the process of auditory localisation was a suitable one in which to study the manifestations of local fatigue was strengthened by the observations of Urbantschitsch[1], in which the fluctuations in localisation of a tone in the head appeared to behave in a manner not unlike that in the case of the windmill. Being under the impression that such fluctuations in localisation probably depend ultimately upon fluctuations in the intensity of the sensations concerned, I intended to compare such results as I might obtain by the localisation method with those of a second set of experiments directed to studying the effect of fatigue upon the intensity of sounds—a line of research in which Urbantschitsch had again been a pioneer[2], and whose interesting observations on this point had, so far as I knew, not yet been repeated or confirmed. It was at first thought that the two methods might turn out to be merely different ways of measuring the same fatigue phenomena, but as the work proceeded it became abundantly evident that the results obtained under these different conditions were totally independent of one another—fatigue occurring in both cases, but in each case being quite distinct in nature. The results of the two series of experiments may therefore be considered separately. My observers (most of whom took part in both series) had all enjoyed some degree of psychological training—some of them a very high degree. My sincere thanks are due to them for their very willing collaboration.

II. LOCAL FATIGUE AS AFFECTING THE LOCALISATION OF SOUNDS.

1. *Apparatus.* The apparatus used in this part of the work was as follows (see Fig. 1). Into either extremity of a long cardboard tube *A* (containing in the centre a small aperture *Z*) were fitted short rubber tubes *B*, *B′*. At the further end of *B*, *B′* were taps *C*, *C′*, beyond which were further rubber tubes *D*, *D′*, ending in short glass tubes *E*, *E′* which were firmly fastened to the table on which they stood. From *E*, *E′* there extended highly elastic rubber tubes *F*, *F′* which fitted on to the extremities of an apparatus which for the sake of convenience may be called the adjustor. This consisted of two metal tubes *G*, *G′*, soldered together but not intercommunicating, curved at their ends and fastened by means of a hinge *H* to a wooden support *I*, which was screwed on to the table. By means of the hinge the whole adjustor could be freely turned in the horizontal plane through an angle of about 140°. When it was so turned,

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[1] "Ueber subjective Schwankungen der Intensität acustischer Empfindungen," *Pflüger's Archiv*, 1882, XXVII. 436.

[2] "Zur Lehre von der Schallempfindung," *Pflüger's Archiv*, 1881, XXIV. 574.

one of the elastic tubes *F*, *F′* became extended; it being so arranged that when the adjustor was in its 'normal' position, at right angles to the edges of *I* and *A*, the tubes *F*, *F′* were unextended, though one of them became extended at the slightest movement of the adjustor. The total distance between *Z* and the beginning of the metal tubes *G*, *G′* was carefully measured so that when the adjustor was in its 'normal' position the paths *Z–G* and *Z–G′* were of equal length, but as soon as the adjustor was moved to a different position, one path became longer than the other by an extent that bore a definite relationship to the angle through which the adjustor was moved. The precise length of *F* or *F′* for any position of the adjustor could of course easily be calculated by an appropriate trigonometrical process or obtained by direct measurement. In actual practice however this was scarcely necessary, as the adjustor was seldom moved through an angle of more than 30° on either side of the 'normal' position, and the movements were for the most part much less than this, so that the actual elongation of *F* or *F′* during the experiments corresponded very closely to the displacement of the

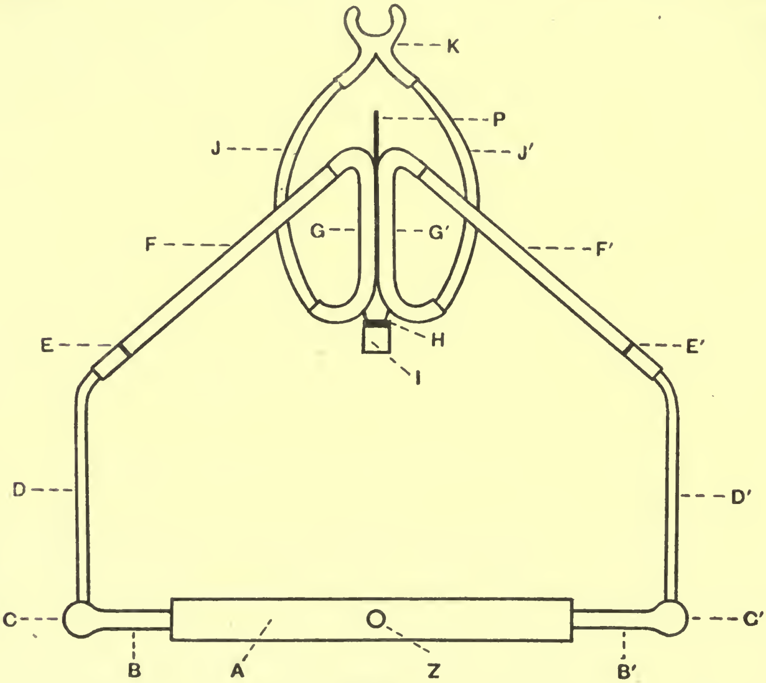

Fig. 1.

adjustor. For this reason, and because of the inevitable roughness of the measurements on the psychological side which made great physical accuracy superfluous, the distance in centimetres through which the adjustor was moved was in every case the measure used in the experiments. To facilitate the taking of the measurements the adjustor was provided with a pointer *P*, immediately beneath which was placed a centimetre scale; the zero of this scale coincided with *P* when the adjustor was in its 'normal' position, so that any departure of the adjustor from the 'normal' could be immediately detected and measured. At the further ends of *G*, *G′* (those nearer *I*) were fitted rubber tubes of equal length *J*, *J′* ending in the head piece of a binaural stethoscope *K*, the ear pieces of which were inserted into the ears of the observer and carefully adjusted in the usual way. The whole apparatus therefore constituted an elaborate double path along which a sound could be conducted from the aperture *Z* to the two ears of the observer; either side of this double path being of such a nature that (by means of an appropriate movement of the adjustor, acting on the elastic tubes *F*, *F′*) it could immediately be made longer than the other side by any desired amount within certain limits.

2. *Procedure.* In a research by Wilson and Myers[1], Rayleigh's statement was confirmed that when a sound travels along two distinct paths to the two ears, it is (other things equal) localised in the middle of the head when the two paths are of equal length, but that if a small difference be made in the length of the paths, the sound is (within certain well-marked limits) localised on that side of the head which corresponds to the shorter path, the apparent displacement of the sound being (again within well-marked limits) roughly proportional to the difference in length of the two paths. This effect has been shown to be connected with differences in the phase of the sound vibration as it reaches the two ears; there being produced an apparent displacement of the sound towards the side on which the vibrations reach the ear in an earlier phase[2]. The problem before us in the present research was to discover whether this function of localisation was liable to be affected by fatigue, and, if so, to what extent.

The procedure in the experiments was as follows. The adaptor having been placed in its 'normal' position, and the observer having placed the ear pieces of the stethoscope in his ears, the experimenter struck a tuning fork and held it while vibrating over the aperture Z. The observer then alternately pinched the rubber tubes J and J' between finger and thumb, the sound being thereby entirely excluded alternately from either meatus. While so doing, he endeavoured to judge whether there was any difference in the intensity of the sound as heard through the two ears. If any difference was detected, one of the taps C, C' was partly closed on the side which corresponded to the louder sensation and the observations were renewed; this process of adjustment being continued until the observer was satisfied that the sound appeared to be of equal intensity on either side. Thereupon the experiments proper were begun. Each experiment fell into three parts, the general method being the same as that employed by me in my earlier observations on local fatigue in illusions of reversible perspective[1]. There was first a 'preliminary' period, in which the observer having moved the adaptor away from the 'normal' position, endeavoured, with closed eyes, to replace it in such a position that the sound appeared to be localised in the head midway between the two ears. Having brought the adaptor to such a position, he opened his eyes and recorded the position of the pointer on the scale. Several such observations having been thus made by the 'reproduction method' (the initial movement of the adaptor in such observations being to the right and left alternately), the second or 'fatigue-inducing' period was begun. During this the observer left the adaptor at the 'normal' position, and throughout the whole duration of the period heard the sound through one ear only, having placed a metal clip on one of the tubes J, J', which entirely prevented the sound from passing through the tube so clipped. At the end of the 'fatigue-inducing' period the observer quickly removed the clip, the sound passed directly to both ears again and the third or 'test' period immediately began. During this the observer made a series of observations by the reproduction method, following exactly the same procedure as he had adopted in the 'preliminary' period.

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[1] "The Influence of Binaural Phase Differences on the Localisation of Sounds," this *Journal*, 1908, II. 363.

[2] We had opportunity to corroborate most of Wilson's and Myers's interesting observations on this point in the course of the previous (war) research at University College already mentioned.

Under these circumstances fatigue, when present, should manifest itself in an alteration in the position of the adaptor required to produce a localisation of the sound in the middle of the head. Thus let us suppose that in the 'preliminary' period when both ears are unfatigued, this position is zero, *i.e.* corresponds to the 'normal' position of the adaptor. If fatigue of the localising function is really present as a result of the stimulation of (say) the observer's right ear[2] during the 'fatigue-inducing' period, this position will, in the 'test' period, no longer give a central localisation of the sound, but a localisation that is displaced from the centre of the head towards the left ear, owing to the preponderant influence of the unfatigued left ear over the fatigued right ear. To bring back a central localisation under these circumstances, the right ear will have to be helped, as it were, by shortening the path along which the sound reaching that ear has to travel, and thus giving it the advantage of the phase difference. The observer will therefore, in this case, have to move the adaptor further to the right, *i.e.* towards the fatigued side, than he did in the 'preliminary' period. The distance through which the adaptor has to be moved for this purpose should afford a measure of the degree of the fatigue, while the time required for the records made by the observer to return to the neighbourhood of those made in the 'preliminary' period should indicate the duration of the fatigue; the successive measurements of the 'test' period giving in this way a fairly accurate picture of the whole course and nature of the fatigue effects produced.

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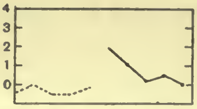
[1] *Op. cit.*

[2] The observer faced the apparatus throughout the experiments.

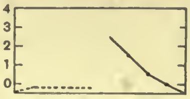
3. *First Results.* Figs. 2–13 record the final results obtained from several observers after a considerable amount of practice (in no case less than three and sometimes as many as ten previous sittings). The dotted lines indicate the scale readings in the preliminary periods, the continuous lines the scale readings in the test periods. The ordinates represent distance in centimetres from the zero point of the scale, *i.e.* the 'normal' position of the adaptor, while each point on the curves corresponds to a scale reading taken at the completion of a judgment made by the observer, so that the abscissae roughly indicate the passage of time (not exactly, since the individual judgments were not necessarily completed at regular intervals). The duration of the fatigue-inducing period in these experiments was in every case three minutes (this period is not represented in the figures). The fork used was $e' = 320$ vibr. per sec.

Inspection of the figures shows at once that the manifestations of fatigue are not uniform. There are marked differences in this respect between one individual and another and in some cases between the two ears of the same individual. The records of observers Co. and Ta. resemble one another pretty closely. Both indicate a moderate degree of fatigue, the two ears being equally affected. Observer Jo. experiences no fatigue with either ear. Observer We. experiences a high degree of fatigue with each ear. Observers Fo. and Fl. show a marked difference in the relative fatigability of the two ears; Fo. experiences no fatigue with the left ear, but very intense and prolonged fatigue with the right ear; Fl.



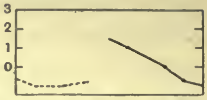
Obs. Co. Right Ear. Duration 2′ 25″.

Fig. 2.



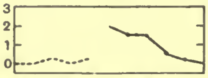
Obs. Co. Left Ear. Duration 2′ 13″.

Fig. 3.



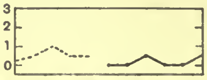
Obs. Ta. Right Ear. Duration 1′ 42″.

Fig. 4.



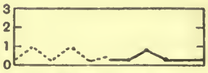
Obs. Ta. Left Ear. Duration 1′ 51″.

Fig. 5.



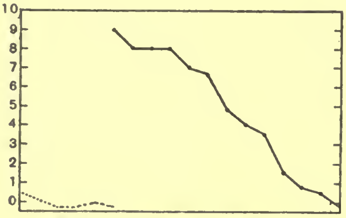
Obs. Jo. Right Ear. Duration 1′ 32″.

Fig. 6.



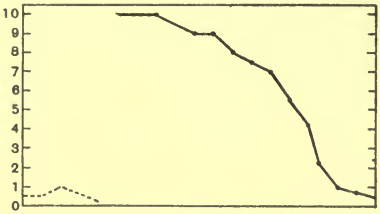
Obs. Jo. Left Ear. Duration 1′ 40″.

Fig. 7.



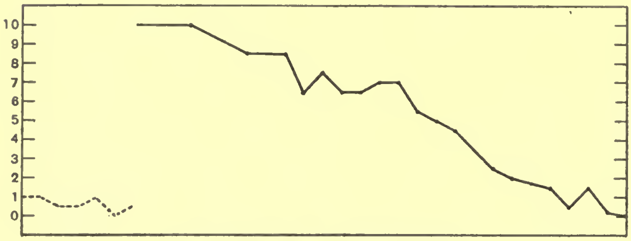
Obs. We. Right Ear. Duration 3′ 15″.

Fig. 8.



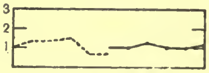
Obs. We. Left Ear. Duration 3′ 22″.

Fig. 9.



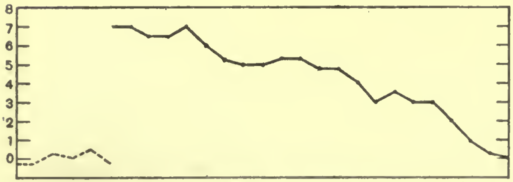
Obs. Fo. Right Ear. Duration 9′ 26″.

Fig. 10.



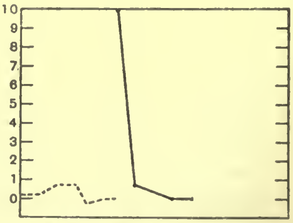
Obs. Fo. Left Ear.
Duration 1′ 2″.

Fig. 11.



Obs. Fl. Right Ear. Duration 6′ 18″.

Fig. 12.



Obs. Fl. Left Ear. Duration 30″.

Fig. 13.

Figs. 2–13 show displacement in localisation produced in various observers after 3 mins. uniaural stimulation. ------ before fatigue. —— after fatigue.

experiences fatigue of very high intensity but very brief duration with the left ear, and fatigue of more moderate intensity but of long duration with the right ear.

That these curious differences between individuals and between the two ears of the same individual are really characteristic of the behaviour of the individuals or the ears concerned is shown by the close agreement between the results here recorded and those of earlier experiments under the same conditions. With Observers Ta., Fo. and Fl., in particular, quite a large number of experiments have been made, always with the same results; each individual and each ear always reacted in its own characteristic fashion, whenever tested, during a period of over four months. We may therefore conclude that the function of sound localisation under the conditions of the present experiments is liable to fatigue of a kind which displays marked individual and local differences—differences which are, however, constant for a particular individual or for a particular ear over considerable periods of time, showing thus that they are, in all probability, dependent upon more or less permanent characteristics of the psycho-physical processes concerned. The fatigue in question is thus very similar to that manifested in the case of the windmill illusion[1].

Having established this result, further experiments were made with some of the observers, with a view to discovering in what way and to what extent the fatigue would be affected by varying some of the conditions under which the preceding observations had been made.

4. *Influence of duration of the fatigue-producing stimulus.* The first point to be investigated was the effect of varying the *duration* of the fatigue-inducing period. A series of observations was conducted with Ta. and Fl. in which (all other conditions remaining as before) the fatigue-inducing periods lasted ¼ min., ½ min., 1½ mins., 3 mins., 6 mins., or 12 mins., two observations being made with each duration of fatigue-inducing period. It was found that with Ta. the fatigue increased with the increase of the fatigue-inducing period until the latter was of 1½ mins. duration, but that after that increasing duration of the fatigue-inducing periods produced no corresponding effect upon the manifestations of fatigue, which were in every case very similar to those already recorded in Figs. 4 and 5. With the left ear of Fl. the variations in the duration of the fatigue-inducing period produced no appreciable effect whatever; in every case very marked fatigue was observed for a few seconds

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[1] This *Journal*, 1913, vi. 60.

followed by a complete and almost instantaneous recovery[1]. When Fl.'s right ear was fatigued however, a fairly distinct correspondence between the duration of the fatigue-inducing period and the duration of the resulting fatigue became manifest. The second of the two sets of observations for each length of fatigue-inducing period is recorded in Figs. 14–19[2]. It will be seen from these that while the maximum *extent* of disturbance in localisation is reached with a fatigue-inducing period of 1½ mins., the *duration* of this disturbance continues to increase steadily with the duration of the fatigue-inducing period throughout the experiments.

A few observations with fatigue-inducing periods of quite short duration were made also with subject Fo. As in the case of the 3-minute fatigue-inducing periods, no fatigue was manifested in the left ear, but the right ear responded with well-marked fatigue after periods of ½ and ¼ min. The intensity of the fatigue in these circumstances (as measured in the usual way by the movement of the adaptor necessary to produce localisation in the centre of the head) was almost if not quite as great as that shown after the 3-minute periods; but the duration of the fatigue was greatly diminished, readings comparable to those of the preliminary period being now recorded after 20–40 secs., instead of after the lapse of several minutes as in the previous experiments.

From these experiments with fatigue-inducing periods of varying duration it would appear probable that fatigue, where manifested at all, shows in most cases *some* dependence on the duration of the fatigue-inducing period[3]. As this period is prolonged however, there comes a point at which the correspondence between the duration of the fatigue-inducing period and the resultant fatigue ceases to be apparent. The two manifestations of fatigue—*extent* of lateral displacement of the sound and *duration* of this displacement—do not however behave similarly in this respect; the point at which the duration of the fatigue-inducing period ceases to exhibit any correspondence with the extent of the resulting displacement is not necessarily the same as that at which it ceases to correspond with the duration of the displacement. In our present observations with Fl. and Fo. the former correspondence

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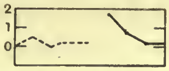
[1] The displacement of the sound was indeed usually so complete that it could not be compensated by any manipulation of the adjustor. It would seem as though the fatigue in this case tended to manifest itself according to the 'all-or-none' principle.

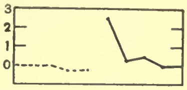
[2] The two sets of observations are in close agreement with one another.

[3] Of the five fatigable ears tested Fl.'s left ear was the only one in which no such dependence was shown in our experiments. Careful observations with very short fatigue-inducing periods would however very possibly have demonstrated the existence of some such dependence in this case also.



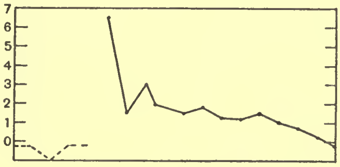
¼ min. fatigue. Duration 37″.

Fig. 14.



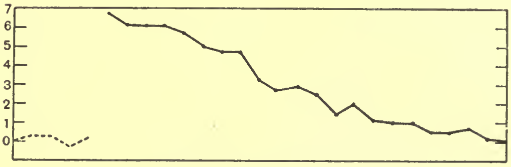
½ min. fatigue. Duration 58″.

Fig. 15.



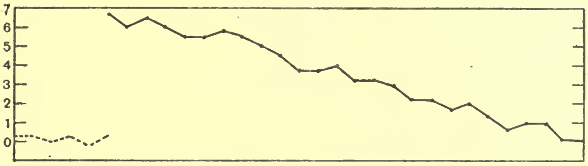
1½ min. fatigue. Duration 3′ 8″.

Fig. 16.



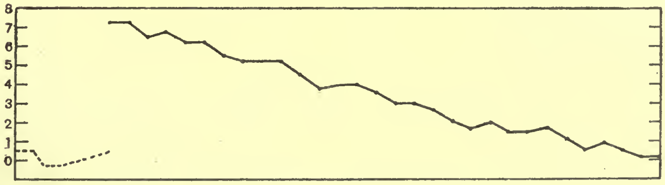
3 min. fatigue. Duration 6′ 9″.

Fig. 17.



6 min. fatigue. Duration 8′ 12″.

Fig. 18.



12 min. fatigue. Duration 12′ 3″.

Fig. 19.

Figs. 14–19 show dependence of fatigue on duration of the fatigue-producing sound. Obs. Fl. Right Ear.

between duration of fatigue-inducing period and extent of displacement shows a tendency to cease at a much earlier point (*i.e.* with a shorter fatigue-inducing period) than does the latter correspondence between duration of fatigue-inducing period and duration of displacement.

To explain these facts it would seem that we must assume the relations between the processes of fatigue and recuperation to vary very greatly in the different perceptive systems involved in our experiments. In some cases (*e.g.* Jo. both ears, Fo. left ear) recuperation can perhaps keep pace with fatigue from the very start, so that no manifestations of fatigue occur (though of course there *may* be subliminal fatigue, and it is possible that even here with a fatigue-inducing period of sufficient length a discrepancy between the two processes would eventually manifest itself in the usual way). In other cases there would seem to be at first an inability of recuperation to keep pace with fatigue, though later on a new equilibrium is established, so that further continuance of the fatiguing stimulus produces no greater preponderance of fatigue over recuperation. This would seem to be indicated by the results with Ta., where increase of the fatigue-inducing periods beyond 1½ mins. produced no corresponding increase of the fatigue manifestations. The establishment of an equilibrium of this kind is doubtless one of frequent occurrence in the nervous system, and is indicated by the results of many experiments on fatigue; very prettily for instance by those of Lehmann with the ergograph[1]. This equilibrium moreover we may perhaps suppose to be of two kinds—sometimes a more or less stable or, as we might say, genuine equilibrium, the prolongation of which shows no after-effects (*e.g.* Ta. both ears); sometimes an equilibrium of presumably a more unstable kind, which is somehow maintained at ever-increasing cost, as shown in the ever-lengthening period required for complete recovery (Fl. right ear, Fo. right ear). In this way we might account for the fact that with the right ear of Fl. there is no increase in the extent of the displacement when the fatigue-inducing period is increased beyond 1½ mins., though there is a steady increase in the duration of the displacement up to 12 mins., and for the fact that with the right ear of Fo. fatigue-inducing periods of ½ sec. or less produce a displacement of almost or quite as great an extent as that produced by 3 minutes' fatigue, though of far less duration.

Finally the relation between fatigue and recuperation may be of such a nature as to produce a partial or entire blocking of a perceptive system after a stimulus of quite brief duration (*e.g.* after fatigue of Fl.'s left ear, when it frequently happened that for a few seconds the sound could not be localised otherwise than on the right, whatsoever the position of the adaptor). In this case no further increase of fatigue occurs with prolongation of the stimulus, and recovery is (at any rate in the case here observed) almost instantaneous[1].

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[1] *Grundzüge der Psychophysiologie*, 62 ff.

A consideration of these effects thus reveals very clearly the peculiar and capricious individuality of the phenomena here under investigation. It would seem that the psycho-physical processes underlying the same conscious states in different individuals or very similar states in the same individual often present marked differences as regards fatigability and power of recuperation. The ultimate nature of these differences —whether structural, functional or trophic—is probably a problem for physiology rather than for psychology and will before long, we may hope, receive a satisfactory explanation at the hands of the neurologists.

5. *Influence of pitch.* The next point to be investigated was the nature of the relation between the fatigue effects and the pitch of the tone heard during the fatigue-inducing period. A number of observations were made with subjects Ta., Fl. and Fo., in which all conditions were precisely the same as in the first set of experiments with 3-minute fatigue-inducing periods, except that forks of 256, 284 and 512 vibrs. were used instead of the fork of 320 vibrs. hitherto employed. In all these observations the results were in every case closely similar to those already obtained for the corresponding ear of the corresponding observer with the 320 fork. We may therefore conclude that, within the limits of pitch difference investigated, the fatigue effects are independent of the pitch of the fatigue-producing sound.

6. *Transference of fatigue.* It remained possible however that while characteristic fatigue effects could be produced for any individual tone,

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[1] It should be noted that in all these cases the fatigue where present does not as a rule manifest itself during the actual continuance of the fatigue-producing stimulus; the effects of fatigue usually become apparent only during the succeeding test period. It is true that on occasion, even during the fatigue-inducing period, the localisation of the sound on the side of the head on which is situated the ear stimulated may become for a few moments less clear and well marked than is usually the case (this was noted especially by Fl. during fatigue of the right ear, not however during fatigue of the left ear, where from the resulting fatigue manifestations such an occurrence would seem perhaps to be particularly likely): but there is nothing at all corresponding to any continuous displacement of the sound towards the centre of the head; at the end of even the longest fatigue-inducing period the sound is often as clearly localised on the stimulated side as at the beginning. In this respect the phenomena here observed are analogous to those of the windmill experiment, where during the fatigue-inducing period (*i.e.* in that case during stereoscopic binocular regard) there is seldom any reversal of the perspective.

these effects might be confined to the perception of the tone in question and would not be transferred to the perception of a tone of different pitch; that, in fact, fatigue might be specific to the processes underlying the perception of a particular tone and not common to the whole unilateral auditory apparatus. In order to study this point, a number of experiments were made in which one fork was used in the preliminary and test periods and another fork of different pitch in the fatigue-inducing periods. Subjects Ta., Fl. and Fo. again took part in these experiments. With Ta. the following combinations of forks were tried (the fork used in the fatigue-inducing period being stated first, that used in the preliminary and test periods second): 256–320, 256–512, 384–320; with Fl. 256–320, 256–384, 256–512, 512–384, 384–320; with Fo. 384–256, 512–320. With Fl. some additional experiments were also made with one of the forks damped by riders so as to produce a more dissonant interval. In every case the results coincided with those obtained in the earlier experiments, each individual peculiarity being exhibited in the same way and to approximately the same extent as when the same fork was used throughout. It thus appears that the fatigue here under consideration is not confined to the processes underlying the perception of a particular tone, but is common to all perception of tone in the ear stimulated.

7. *Influence of loudness.* A further question to be investigated was the possible influence of the loudness of the tone upon the fatigue effect. For this purpose fork 320 was used to produce a tone of three different grades of intensity. For the medium intensity the conditions were precisely the same as in the earlier experiments. For the weak intensity the aperture *Z* was closed by a layer of cotton wool of sufficient thickness and density to make the sound when heard through the stethoscope but slightly above the threshold for continuous audibility. For the strong intensity an appropriate resonance box was suspended above the aperture, the tone being thereby greatly reinforced and appearing as loud as could pleasantly be borne, when heard through the stethoscope. Ta., Fl. and Fo. took part in these experiments, two observations with each intensity being made with Ta. and Fl. and one with Fo. Apart from the variation in loudness, all conditions were as in the first series of experiments. Here again it was found, contrary to the expectation of the observers, that the varying conditions made no apparent difference whatsoever to the fatigue effects, which were manifested to approximately the same extent as usual (and with all their individual peculiarities) for all three grades of intensity. We may therefore conclude that—

within wide limits at any rate—the fatigue effects are independent of the intensity of the fatiguing stimulus.

8. *Influence of consciousness.* In all these experiments the observers had listened with close attention to the tone during the fatigue-inducing period so that the auditory sensation itself was by far the most constant and prominent thing in consciousness during that period. To discover what would happen when the tone ceased to enjoy this prominence in consciousness was the object of a further series of experiments undertaken with subjects Ta. and Fl. The subjects were given (*a*) a passage from a book to read silently and attentively, (*b*) a series of pictures of an exciting character to be attentively contemplated, (*c*) a series of additions in one of Kräpelin's *Rechenhefte* to be performed as rapidly as possible. None of these distractions during the fatigue-inducing period produced any significant effect, the fatigue being manifested just as when full attention had been given to the fatiguing stimulus.

A further step was therefore taken, and it was decided to exclude the fatiguing sound from consciousness altogether. This was done by calling in the assistance of a second experimenter, who was provided with two small drum sticks with which he beat a rapid and continuous series of loud blows upon a tin box throughout the fatigue-inducing period; the noise thus created being sufficient to make the sound of the fork completely inaudible to the observer. Observers Fl. and Fo. took part in these experiments, and with both it was found that the characteristic fatigue effects manifested themselves as on previous occasions, in spite of the inaudibility of the fatigue-producing stimulus. With Fl. an additional series of control experiments was performed, in which a large screen was placed between the observer and the experimenter, and an assistant experimenter manipulated the taps *C*, *C′*, in such a way as to exclude the sound from one of the paths when so desired—thus obviating the necessity for the use of clips (adjusted by the observer), as employed during the fatigue-inducing period in the other experiments. In the absence of any auditory clue the observer was now unable to tell when the stimulus began, which ear was being stimulated or whether there was any stimulation at all. Under these conditions there were made two experiments with stimulation of the right ear, two experiments with stimulation of the left ear and two experiments without any sounding of the fork at all, the observer being in each case ignorant of the conditions of the experiment. On each occasion the appropriate response was made; stimulation of the right or left ears produced all the usual characteristics of fatigue following such stimulation (moderate and prolonged fatigue in the case of the right ear, intense fatigue of very short duration with the left ear), while absence of stimulus was followed by records in the test period that were extremely similar to those made in the preceding preliminary period. These results therefore afford additional evidence of the genuineness of the phenomena observed, and at the same time show that the fatigue resulting from inaudible stimulation is uncomplicated by the rather intense binaural stimulation occasioned by the loud beating on the tin box. We may therefore conclude that the conscious appreciation of the sound is not a necessary condition for the production of the fatigue here under consideration.

9. *Does uniaural fatigue influence binaural hearing?* The problem towards the solution of which the next series of experiments was directed was concerned with the influence of fatigue upon localisation during continuous binaural hearing. In the case of those observers whose fatigability is different in quality or quantity according as the sound is led to one ear or the other, it would seem not improbable that with continuous binaural hearing there might occur illusory alterations in the apparent locality of a tone, due to the differential action of fatigue upon the two perceptive systems on whose co-operation the power of localisation depends—alterations to which observers with more equal fatigability of the two systems concerned might perhaps be less exposed. The question is of considerable practical interest, since the process of binaural localisation of a continuously heard sound is one that was called into play in several military operations during the war, it being therefore a matter of importance to select the most sensitive and reliable observers for this purpose. With a view to studying this point, our observers listened continuously to the sound of the 320 fork for 15 minutes, the sound being led to both ears and the adaptor being placed at the zero point on the scale. Every 30 seconds the observer moved the adaptor rapidly away from this point (to right and left alternately) and then, bringing the adaptor in again towards zero, recorded a judgment as in the previous experiments. Such an experiment was performed twice with Ta., Fl. and Fo. and once with Co. and Jo. It was thought that if asymmetrical fatigability of the two ears were in any way disadvantageous to the process of localisation during continuous binaural hearing, the records of Fl. and Fo. in this experiment would be inferior to those of the other observers.

The results showed no such influence at work. The mean variation of the 30 readings taken during the experiment worked out as follows (using the second experiment, where two were performed): Co. ·55 cm., Ta. ·19 cm., Jo. ·85 cm., Fl. ·58 cm., Fo. ·60 cm. While there was evidence of individual differences in these figures (Ta. exhibited a distinctly smaller variation than do any of the other observers), there was no indication of any distinct superiority of Co. and Jo. over Fl. and Fo. Nor again was there any significant difference of this kind as regards the general shape of the curve representing the course of the successive judgments. None of the curves exhibited any steady tendency for the apparent zero point, as recorded in the judgments, to move continuously to the right or left. It might have been supposed that with Fo. the increasing fatigue of the right ear system, acting in combination with the unfatigability of the left ear system, would have manifested itself during continuous binaural hearing in a tendency to make the apparent zero point move progressively to the left. Similarly with Fl., it might have been expected that the difference in the nature of the fatigability shown by the right and left ear systems would have influenced in some way the curve representing the successive judgments—though the precise nature of the effects upon the curve are less easy to foretell. The absence of any demonstrable effects due to these causes upon the curves for the two observers in question and the close similarity of their curves to those of the other observers seem to indicate fairly clearly that asymmetrical fatigability, such as that exhibited by Fl. and Fo., does not appreciably influence the localisation of tones during continuous binaural hearing. The reason for the non-appearance of any manifestations of fatigue under these circumstances—whether, for instance, it is to be found in the fact that the fatigue only takes place during uniaural hearing, or whether the fatigue is present but is prevented from manifesting itself—is a matter of considerable theoretical interest and one on which some light might perhaps be thrown by further experimentation: it was not possible however to enter into the matter at the time when the experiments here recorded were being carried out.

10. *Relation between localisation and intensity.* As mentioned in an earlier part of this paper, it was originally supposed by the author that the changes in the localisation of a tone produced by fatigue would very possibly prove to be largely or wholly reducible to changes in the relative intensity of the sensations originating from the two ears. Before proceeding to study these latter changes in the second part of the research, it was decided to make a few observations with a view to ascertaining whether intensity differences, in any way corresponding to the localisation differences, could be easily observed with the present apparatus and experimental procedure. As a preliminary step in this direction an

attempt was made to compare the threshold for movements of the adaptor obtained (*a*) by judgments of localisation in the usual way, (*b*) by judgments of intensity differences between the sensations coming from the two ears. This was done in the following manner. For (*a*) the adaptor, instead of being manipulated by the observer as hitherto, was controlled by the experimenter, who placed it in turn over the various points of the scale in the irregular manner followed in the method of right and wrong cases. The observer meanwhile was blindfolded so that he was unable to see the movements or position of the adaptor, and with his two hands he tightly pinched the two tubes of the stethoscope, so that the sound coming through the tubes was normally inaudible. On receiving a signal from the experimenter, he removed his hands from the tubes for a few seconds, carefully listened to the sound and expressed a judgment as to its rightness or leftness. This done, he again pinched the tubes and waited for the next signal; several judgments being thus made for each position of the adaptor. In (*b*) the procedure was the same, except that, on hearing the signal, the observer, instead of opening both tubes, opened first one tube and then the other in rapid alternation, listening alternately to the sounds from the two sides and comparing their intensity; ultimately making a judgment as to which of the sounds was the louder. In both (*a*) and (*b*), to ensure uniformity of sound, an electrically driven tuning fork of 250 vibrs. was substituted for the forks hitherto used.

Observations were made in this way with Fl. and Fo. It was found with both observers that the threshold for localisation (*a*) was far lower than that for intensity (*b*). While in the case of (*a*) errors were very seldom made when the adjustor pointed to more than 1·5 cm. on either side of the zero mark (the actual thresholds, as calculated, were ·49 cm. with Fl. and ·73 cm. with Fo.), in the case of (*b*) errors were made freely at all positions of the adaptor, no satisfactory threshold being obtainable, since 'right' judgments occurred even at the extreme left of the scale and *vice versâ*. This demonstration of the comparative insensibility to intensity differences (which agrees with the recent work of Stewart and Hovda[1]) indicates that, under the conditions of our experiments, the (much finer) process of localisation cannot depend to any considerable extent upon intensity differences coming from the two ears—or at any rate upon a *conscious appreciation* of these differences. It would therefore appear that the factors which underlie the localisation process must probably be looked for elsewhere than in *mere* awareness of differences of intensity—a discussion of the possible nature of these factors being however beyond the scope of the present paper.

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[1] "The Intensity Factor in Binaural Localisation," *Psychol. Rev.* xxv. 1918, 242.

A more important result for our immediate purpose is to be found in the consideration that, owing to the great difference in delicacy between the perception of localisation differences and the perception of intensity differences, the former must almost certainly involve different psycho-physical elements from those concerned in the latter. In other words, our own above recorded experiments on fatigue of localisation can scarcely be concerned with the same processes as those which figured in Urbantschitsch's already-quoted experiments on auditory fatigue as measured in terms of intensity difference. The two processes concerned being thus distinct, a comparison of their behaviour under the influence of fatigue becomes of all the greater interest. With a view to this comparison, we turn now to a consideration of the experiments undertaken to repeat and extend the observations made by Urbantschitsch.

III. LOCAL FATIGUE AS AFFECTING THE INTENSITY OF SOUNDS.

1. *Methods.* Urbantschitsch's demonstration of the influence of fatigue on the intensity of auditory sensations was made as follows[1]. Two rubber tubes led to the two ears of the observer, one tube being partially closed if necessary, so that the tone of a tuning-fork, when held opposite the distant ends, first of one tube, then of the other, produced sensations of equal loudness. This preliminary adjustment having been made, the tuning-fork was struck loudly and held steadily opposite the open end of one of the tubes until it ceased to be audible to the observer. It was then quickly moved to a new position opposite the end of the other tube, whereupon the tone would become audible again and remain perceptible for a brief period before its final disappearance. As an alternative procedure the fork was, after its first disappearance on the fatigued side, moved rapidly to and fro from the end of one tube to the end of the other; when this was done, periods of silence (corresponding to the moments when the sound was led to the fatigued ear) alternated with periods of audibility (corresponding to the moments when the sound was led to the unfatigued ear) until the final disappearance of the tone.

In the present experiments, which followed closely those of Urbantschitsch, these two procedures were designated methods *A* and *B* respectively. Further procedures in studying the same phenomena were adopted as follows.

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[1] "Zur Lehre von der Schallempfindung," *Pflüger's Archiv*, 1881, XXIV. 574.

In method *C*, as soon as the observer reported the first disappearance of the tone, the fork was suddenly damped, moved away from the end of the tube and then gently struck again either with a small and soft mop or with a fleshy part of the finger, and, after a period of four seconds from the cessation of the sound, moved to and fro before the ends of the two tubes as in method *B*, until the observer reported that the sound had again become inaudible.

In method *D* the vibrations of the fork during the fatigue-inducing period (as we may here call the period elapsing between the original sounding of the fork and the first disappearance of the tone) were not allowed to die down as in the previous methods, but were prevented from falling below a certain (fairly high) degree of amplitude by the fork being struck anew at short intervals (usually five seconds). At the end of the fatigue-inducing period (the duration of which could now be controlled by the experimenter) the fork was damped and then struck again (and allowed to die down) as in method *C*.

Methods *C* and *D* have the advantage that two forks can be employed if desired—the fork used for testing the existence of fatigue in the second part of the experiment (test period) being not necessarily the same as that employed for producing fatigue in the first part of the experiment (fatigue-inducing period). These methods were therefore considered especially suitable for studying the question of the transfer of fatigue from the perception of one tone to that of another. On the other hand, for reasons that will appear in a few moments, these methods were not without difficulties of their own. There was therefore added—

Method *E*, in which the period taken for the vibrations of a fork to become inaudible to the observer (when the fork was struck with a given degree of intensity) was, first of all, carefully noted in a number of preliminary observations. In the experiments proper the fork used for testing, on being struck, was held away from the end of the tubes during the fatigue-inducing period; meanwhile another fork (of the same or of different vibration rate, as required) was held opposite the end of one of the tubes to produce fatigue. On the elapse of a period rather shorter (usually seven seconds less) than the average of the disappearance-times recorded in the preliminary observations, the fork to which the observer had hitherto been listening (the fatigue-producing fork) was suddenly removed and the other (test) fork was—without any appreciable intervening interval—moved to and fro as in the previous methods.

In the case of all these methods, the actual experiments were always preceded by a number of preliminary observations in which the fork used was moved to and fro before the ends of the tubes, the observer meanwhile comparing the intensity of the sensations coming from the two ears and ultimately noting whether the sensations disappeared together or, if not, which sensation lasted longer. If (as was generally the case) there was noted any inequality as regards intensity or duration, a small amount of cotton wool was placed in the mouth of the tube corresponding to the louder side and the process was repeated until the observer was satisfied that the sensations were of equal intensity and duration.

Experiments with a fork of 320 vibrs. were made by methods *A*, *B*, *C* and *D* upon a number of observers (notably Ta., Jo., Fl. and Fo. who had taken part in the previous experiments on localisation and upon a new observer In.). Fatigue was shown in the case of all these observers and by all four methods; but not all the methods were found equally suitable or convenient for demonstrating the existence of the fatigue: each method has to some extent its own peculiar advantages and disadvantages.

Method *A* is perhaps the easiest and most satisfactory of all from the observer's point of view; fatigue being, as a rule, easily demonstrable in this way even to a totally unpractised subject. On the other hand, the value of the results obtained is apt to be very dependent on the accuracy of the preliminary adjustments made to obtain sensations of equal intensity in either ear. Thus if the fatigue effect is more marked after fatigue of the right ear than after fatigue of the left, it is often uncertain whether this is due to a genuinely greater fatigability of the right ear or to the fact that the sound as conveyed to the left ear is—quite apart from fatigue—somewhat louder than that conveyed to the right. Nor can this difficulty be altogether avoided by any degree of care in the preliminary adjustments for obtaining equality of loudness; for it soon became apparent that the relative intensity of the sensation from the two ears is subject to frequent slight variation due to causes other than fatigue. The cause and nature of these variations are matters of some interest on their own account (to some extent the variations appear to be connected with pressure changes in the middle ear, for it was often observed that they were affected by swallowing movements on the part of the observer), but they were beyond the scope of the present investigation, in which their influence had merely to be eliminated as a disturbing factor. In method *A* such elimination can only be accomplished by frequently ascertaining afresh whether the sounds from the two ears were of equal intensity or not. Such repetition of the initial testing had necessarily to be undertaken from time to time with the other methods also, but in these methods there was usually some indication from the actual experiment itself as to whether one of the variations in question had taken place. While comparing the sounds from the two ears when the fork was being moved to and fro (as was done in all methods except *A*), the observer would often notice immediately if the customary difference (due to fatigue) between the two sounds was either lacking or unusually intense, and, his suspicions being thus aroused, he would demand a retesting. The indications for retesting being thus given as a rule in the actual course of the experiment, the retesting process had seldom to be resorted to except when really needed; considerable time and trouble being thereby saved.

While method *B* is in this respect superior to method *A*, it has in common with *A* the drawback that one fork only can be used for both fatiguing and testing. The interval of four seconds between the first disappearance and the reappearance of the sound in method *C* was introduced in order to allow of the experimenter changing the forks where necessary, in order to study the transfer of fatigue from one tone to another. This method was however also used with a single fork throughout, for the purpose of obtaining data exactly comparable to those obtained with two forks.

Methods *A*, *B* and *C* all suffer from the disadvantage that the duration of the fatigue-producing stimulus is determined by the period required for the gradually diminishing vibrations to reach the limit of audibility—a period that differs according to the nature of the fork used, the strength with which the fork is struck and the acuity of the observer's hearing. This indeterminateness of the length of the fatigue-inducing period (though subsequently found to be of little importance) is on theoretical grounds obviously undesirable in an experimental procedure. Method *D* was therefore resorted to with a view to overcoming this disadvantage. In this method the duration of the fatigue-inducing period was under the control of the experimenter; except where otherwise stated, this period was forty seconds. Method *D* has the further advantage that the intensity of the fatigue-producing stimulus is kept more nearly constant during the fatigue-inducing period, instead of being allowed to decline continuously throughout that period, as in *A*, *B* and *C*.

The introduction of the four seconds interval in methods *C* and *D*, though it had the advantage of allowing two forks to be used if desired, was found to bring in a serious fresh drawback not present in methods *A* and *B*: it made the observation of the fatigue effects a good deal more difficult and uncertain. It was for this reason that method *E* was devised; by which it was hoped to combine the advantage accruing from absence of interval, as in methods *A* and *B*, with that of allowing the use of two forks, as in methods *C* and *D*. Method *E*, however, has a disadvantage of its own, in that considerable preliminary experimentation is required, and was for this reason only used in experiments bearing on the question of the transference of fatigue.

After this somewhat elaborate discussion of the methods used, it will be possible to deal quite shortly with the results obtained—the more so, since these results presented for the most part a striking uniformity and regularity; the difficulties of the experiment lying, it would seem, rather in the conditions of observation and in the liability to disturbance by irrelevant factors than in the nature of the fatigue phenomena themselves.

2. *First results.* As already mentioned, fatigue effects were obtained for all the observers and for every method used, though by some methods more certainly and clearly than by others, for reasons that we have now explained. Table I gives the number of experiments (out of a total of eighty for each observer, forty for each ear) in which more or less clear manifestations of fatigue were obtained by the various methods.

Table I. *Showing number of observations* (*out of a total of twenty for each ear*) *in which fatigue was manifested by different methods.*

	Right Ear.				Left Ear.			
Method	*A*	*B*	*C*	*D*	*A*	*B*	*C*	*D*
Ta.	10	9	7	6	10	10	7	7
Jo.	9	8	10	7	9	10	8	8
Fl.	10	8	7	8	10	9	7	7
Fo.	8	9	8	6	10	9	6	6
In.	9	9	7	7	10	8	8	8

It will be seen at once that the observers present no such clearly marked individuality as they did in the experiments on localisation; nor is there found any significant difference between the two ears of the same observer, such as was apparent in some cases there. Fatigue is here manifested by every observer and in about the same number of individual experiments. Furthermore, a consideration of the introspections offered by the observers suggests very strongly that, had it not been for the interference of irrelevant factors and the difficulties of observation, fatigue would have manifested itself in every one of the experiments.

Nor was there anything in these experiments to indicate the existence of any considerable difference between the observers as regards the intensity or the duration of the fatigue produced. As regards intensity, the number of times that a tone was heard at the non-fatigued ear after it had ceased to be audible at the fatigued ear was about the same for all the subjects. (The actual number of times varied from 1 to 8 in successful experiments, the average being, for Ta. 3·3, for Jo. 3·4, for Fl. 3·1, for Fo. 2·7 and for In. 2·8.) As regards duration, the greater difficulty that all observers experienced with methods *C* and *D* as compared with methods *A* and *B* (corresponding to the smaller figures recorded for the former methods in Table I), when examined in the light of the observers' introspective comments, appears to be due to the fact that the most marked period of the fatigue effect had already passed during the four seconds interval that occurred in methods *C* and *D*. If this is so, it would seem probable that with all the observers the fatigue effect is of short duration only and declines rapidly in intensity from the start.

3. *Influence of duration of the fatigue-producing stimulus.* With a view to obtaining more precise information on this question of the duration of the fatigue, a second series of experiments was undertaken with observers Ta., Jo., Fl., Fo. and In., in which method *D* was used as before, with the modification however that, whenever the first test gave evidence of fatigue (*i.e.* whenever the sound was heard longer at the unfatigued ear than at the fatigued ear), a second test was immediately carried out in the same way as the first; if this second test gave evidence of fatigue, a third test was immediately undertaken, and so on indefinitely until the sound was heard to die away as nearly as possible simultaneously at both ears. In this way it was hoped to obtain a rough measure of the duration of the fatigue effect. The whole series of experiments was performed twice, at first with a fatigue-inducing period of forty seconds, as in the earlier experiments, then with a fatigue-inducing period of three minutes; the aim being to obtain evidence as to the nature of the dependence (if any) of the duration of the fatigue effect upon the duration of the preceding fatigue-inducing period. Each observer made twenty experiments with each length of fatigue-inducing period (ten with each ear).

The figures in Table II indicate how frequently a given number of tests had to be made before equality between the two ears was reached. Thus the figures in the first column show how often fatigue manifestations failed to be forthcoming even in the first test, those in the second column show how often fatigue was manifested in the first test but not in the second, those in the third column show how often fatigue was manifested in the first and second tests but not in the third, while those in the fourth column show how often fatigue was manifested in three or more successive tests.

Table II. *Showing number of tests made before re-establishment of equality between the two ears.*

	Fatigue-inducing period of 40 secs.				Fatigue-inducing period of 3 mins.			
	No fatigue in 1st test	Fatigue in 1st test	Fatigue in 2nd test	Fatigue in 3rd or later test	No fatigue in 1st test	Fatigue in 1st test	Fatigue in 2nd test	Fatigue in 3rd or later test
Ta.	3	14	2	1	4	12	4	0
Jo.	2	17	1	0	2	14	3	1
Fl.	4	12	2	2	3	16	1	0
Fo.	5	15	0	0	3	14	2	1
In.	4	12	3	1	4	15	0	1
Total	18	70	8	4	16	71	10	3

Two points emerge with some clearness from these figures. In the first place, it is evident that there is no appreciable increase in the duration of the fatigue manifestations when the fatigue-inducing period is increased from forty seconds to three minutes; with all the observers the fatigue has apparently attained its maximum in the former period.

Secondly, it is clear that the fatigue is, in nearly all cases, of quite short duration, being for the most part manifested only in the first test. The actual duration of a test varied of course slightly from one experiment to another (being dependent both on the observer's acuity of hearing and on the strength with which the fork was struck), but in the great majority of cases was between ten and fifteen seconds and very seldom above twenty seconds. Allowing therefore a total period of twenty seconds (to include both the individual test period and the interval of four seconds between the fatigue-inducing period and the test period, or between two successive test periods), we may say that in general the fatigue effect is longer than twenty seconds (because fatigue is usually manifested in the first test) and shorter than forty seconds (because fatigue is usually not manifested in the second test). Introspective remarks by the observers indicate that the former figure is probably nearer than the second: it was fairly often reported that at the beginning of the first test the difference in intensity between the two sounds (and therefore presumably the fatigue effect on which this difference depends) was a good deal more marked than at the end, whereas equality of the sounds is usually reported as being observable from the very beginning of the second test. This evidence in favour of the rapid decline and disappearance of the fatigue effect is in harmony with the conclusions that may be drawn from a comparison of the results obtained by methods *A* and *B* with those obtained by methods *C* and *D* (see above). The greater difficulty in observing the fatigue effect by the latter methods would seem to be due to the fact that the most marked manifestations of fatigue no longer occur after a few seconds' interval. On the other hand, the few observations recorded in Table II, in which a fatigue effect distinctly longer than usual was observed, are in several cases open to suspicion as being due to the more permanent variations between the relative acuity of the two ears to which we have already drawn attention. Readjustment of the relative loudness of the sounds (in the manner already described) was found necessary after eight of the twenty-five observations reported in the last two columns on either side of Table II, while only fifteen such readjustments had to be made following the 141 observations in which fatigue was manifested in one test only.

Summing up all this evidence, we may therefore conclude with some degree of assurance that the fatigue here in question is seldom of more than thirty seconds' duration and that it usually lasts for rather less than this period.

4. *Influence of loudness.* The next question to be investigated was concerned with the relation of the fatigue effect to the intensity of the fatigue-producing tone. Some light on this point is already thrown by a comparison of the results obtained by methods *C* and *D* respectively (Table I). In method *C* the sound was allowed to decline steadily in intensity throughout the fatigue-inducing period until the limit of audibility was reached; in method *D* the tone was kept throughout at fairly high intensity. The results however show no considerable difference in the frequency with which fatigue was observed by the two methods. A further series of forty experiments bearing on this point was undertaken by observer Fl. Method *D* was used in these experiments, but in half the total number of cases the fork was struck quite softly with the finger, while in the other half it was struck with the hammer in the usual way but with considerably greater force. The results showed no appreciable difference as regards the fatigue produced by the soft and loud stimuli respectively, being in both cases very similar to those already recorded in Table I. We may conclude therefore that, within wide limits at any rate, the fatigue is independent of the intensity of the fatiguing stimulus.

5. *Influence of pitch.* The influence of pitch was next examined.

Experiments for this purpose (by method *B*) were made by observers Ta., Fl. and Fo., ten observations being made by each observer with each of the following forks: 256, 384, 512. Again no significant differences were forthcoming, the results being in every case very similar to those recorded for fork 320 in Table I. It appears therefore that, within the limits tested, the fatigue is independent of the pitch of the fatiguing stimulus.

6. *Transference of fatigue.* The next question that arises is that connected with the transfer of the fatigue from the perception of a tone of one pitch to the perception of a tone of another pitch. Urbantschitsch had already made some interesting observations on this point and had concluded that no such transference occurs. Our own methods *C* and *D* were originally devised with this end in view, but the difficulties in observing the fatigue effect by these methods were found to be even greater when two tones were employed than when, as in the experiments hitherto described, a single tone was used throughout. This increased difficulty appeared to be connected with the 'shock' of surprise experienced by the observer on suddenly hearing a tone different in pitch from that to which he had been listening during the fatigue-inducing period; this shock made judgment difficult at the beginning of the test period, precious seconds being thereby lost. Method *E* was therefore devised to avoid the loss of time necessitated by the interval in methods *C* and *D*. The results—which had before been rather disappointing, the observers recording their judgments with some degree of hesitation—immediately improved, and a series of experiments was carried through by observers Ta., Fl. and In. and a new observer Pe., which—so far as can be judged from introspective evidence—was quite satisfactory and reliable.

This series of experiments includes in all 214 separate judgments with all combinations of forks 256, 320, 384 and 512. Of these judgments only 22 (or rather more than 10 per cent.) showed any distinct tendency for the sound to be heard more loudly on the unfatigued than on the fatigued side. That these latter judgments are to be attributed to errors of observation or to the more permanent variations in loudness to which we have already more than once referred rather than to the influence of fatigue, is shown by the fact that there were nineteen judgments in which the *opposite* effect occurred—the sound being heard longer on the fatigued side than on the unfatigued side. We may therefore conclude with very little hesitation that there is no transfer of fatigue from the perception of a tone of one pitch to the perception of a tone of another pitch—our observations on this point being thus in agreement with those of Urbantschitsch. In this respect the fatigue here under consideration differs markedly from the localisation fatigue previously studied.

A question of some interest that arises in connexion with this result is concerned with the degree of difference in pitch that is sufficient to interfere with transfer. Experiments bearing on this question were conducted with observers Fl. and Pe. Two forks of 256 vibrs. were used, one fork being provided with movable riders, so that the pitch could be varied to any desired extent within certain limits. The experiments were conducted according to method *E* and the procedure of right and wrong cases was followed as regards the sequence of the various degrees of difference in pitch between the two forks. It was found that the results were similar to those usually obtained in the attempt to measure a threshold, in that there existed no sudden point of transition, the judgments indicating presence or absence of transfer being on the contrary freely interspersed over a certain area. This area was however a fairly narrow one, a relatively small difference in pitch being sufficient as a rule to prevent transfer. The actual threshold calculated[1] from all the judgments was, in the case of Fl. ·52 vibrs. per second (with a mean variation of ·17), in the case of Pe. 1·46 vibrs. per second (with a mean variation of ·38).

This result is possibly not without some importance for the theory of hearing, since, if we may argue from the absence of fatigue transfer from one process to another that the processes concerned are distinct, it would show that separate and distinct psycho-physical processes are called into play in the perception of two tones as soon as their pitch differs by something in the neighbourhood of 1 vibr. per second. It is interesting to note too that the threshold for the transfer of fatigue is in the neighbourhood of the threshold for the perception of pitch difference. Indeed in the case of the present two observers the correspondence turned out to be extraordinarily close; measurements for the threshold of pitch discrimination undertaken immediately on the conclusion of the fatigue experiments giving thresholds of ·49 (m.v. ·19) in the case of Fl. and 1·38 (m.v. ·36) in the case of Pe. Nothing of course can be safely inferred from observations on two subjects only, but, taken in connexion with the general correspondence in the nature and neighbourhood of the two thresholds, these figures would suggest that the psycho-physical processes concerned in the fatigue are the same as those which correspond to the actual conscious perception of a tone, and are therefore central and not peripheral in situation.

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[1] In accordance with the method described by Spearman, "The Method of Right and Wrong Cases without Gauss's Formulae," this *Journal*, 1908, II. 227.

7. *Influence of consciousness.* Some further light is thrown on this last point by our final series of experiments, in which an endeavour was made to study the effect on the fatigue manifestations produced by excluding the fatigue-producing sound from consciousness. As in the case of the experiments on localisation, exclusion from consciousness was attempted in two ways: first, by the distraction caused by carrying on some mental task; secondly, by drowning the sound by a louder noise. The experiments were in this case carried out with observers Jo., Fl. and In., method *D* being employed throughout.

The distracting mental tasks (for the most part mental multiplication) were found to exercise little influence on the fatigue, it being almost impossible to exclude the sound from consciousness for the whole period, and the tone being usually, as a matter of fact, discoverable by introspection as persisting in the background of consciousness during the greater part of the fatigue-inducing period.

Much more marked however was the effect of the complete exclusion of the sound from consciousness by the noise of beating on the tin box. Out of twenty observations for each person Jo. reported only two observations which could be interpreted as giving indications of fatigue, Fl. one such observation and In. four such observations. When compared with the similar figures for the same method in Table I, it at once becomes apparent that exclusion from consciousness prevents, or at least very seriously diminishes, the liability to fatigue. The dependence of the fatigue on conscious perception (which is, again, in marked contrast to the behaviour of the localisation fatigue studied in the previous section) agrees with the conclusion that may be drawn from the results of the threshold experiments as to the central situation of the psycho-physical processes concerned in this fatigue.

IV. SUMMARY OF PRINCIPAL RESULTS.

The process of binaural localisation of a tone is frequently subject to fatigue when one ear has been previously stimulated. The fatigue manifests itself in an apparent displacement of the tone towards the unfatigued side. Its effects can be compensated by causing the sound affecting the fatigued side to arrive at an earlier phase than the sound affecting the unfatigued side. During the process of recuperation the apparent displacement of the sound diminishes—as a rule gradually—and finally disappears.

This fatigue is manifested to a different extent in different individuals, and sometimes even in the two ears of the same individual.

This fatigue exhibits usually some dependence on the duration of the fatigue-producing stimulus. There would appear to be a point however (varying with different individuals) beyond which continuation of the fatigue-producing stimulus causes no corresponding increase of the fatigue manifestations.

The fatigue is largely—perhaps quite—independent of the pitch and intensity of the fatigue-producing stimulus.

The fatigue produced by a tone of one pitch is transferred to the perception of a tone of another pitch.

The fatigue is unaffected by distraction, and occurs even when the fatigue-producing stimulus is rendered inaudible by a simultaneous louder sound affecting both ears.

This fatigue does not produce any appreciable effect upon localisation during continuous binaural hearing, even in the case of individuals who are more fatigable on one side than on the other side.

Uniaural stimulation produces a second kind of fatigue, which manifests itself in diminished intensity of the auditory sensation produced by stimulation of the fatigued ear, so that after uniaural stimulation a tone of continuously diminishing intensity will become inaudible on the fatigued side at an earlier moment than on the unfatigued side, though before fatigue it had become inaudible on both sides at the same moment.

This fatigue is of brief duration (seldom if ever more than thirty seconds) and would seem to decline rapidly as soon as the fatiguing stimulus is removed.

This fatigue is experienced by all observers to approximately the same extent.

It is largely independent of the duration, pitch or loudness of the fatiguing tone.

Fatigue produced by a tone of one pitch does not affect the perception of a tone of different pitch, except within limits that correspond roughly to the limits of pitch discrimination.

No manifestations of this fatigue are observed when the fatiguing tone is rendered inaudible by a louder stimulus affecting both ears.

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A STATISTICAL SURVEY OF ARITHMETICAL ABILITY[1].

By DANIEL J. COLLAR.

I. *The problem.*
II. *School marks for research purposes.*
III. *Description of the tests and the method of marking.*
IV. *Results of the survey.*
V. *Summary of conclusions.*

I. The Problem.

In common educational practice, it has been customary to regard arithmetic as a single subject, and therefore to assume that a child who excels in any part of it will excel in the whole. The lad who does well, for example, in problematic work has been expected to be quick and accurate in simple computation. The assumption is not an unreasonable one, for arithmetic is a deductive science. Its most complicated applications depend ultimately on its simplest principles, and its practical value disappears when computation is inaccurate. The unity of arithmetic is implied, rather than explicitly asserted, in various psychological researches, such as those of Starch[2] and Carey[3], who have each furnished correlations between it and other subjects of the school curriculum.

While the inter-dependence of the various forms of the subject cannot be ignored, it is manifest that arithmetic has at least two aspects, widely differing in psychological character: (1) the mechanical, as shown in computative skill, (2) the intelligent, as shown in the ability to reason arithmetically. The former appears to have little relation to intelligence, in the ordinary acceptation of this term. It should depend on the acquisition and use of memory habit. The latter invokes the power to manipulate the unforeseen, to formulate hypotheses, to test these critically, and to carry out self-devised plans to their logical conclusions.

Howell[4] declares that arithmetical ability is a 'myth,' *i.e.* there does not exist, as a rule, in the individual child an ability that may be expected to function in all forms of arithmetic.

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[1] A condensation of a thesis approved for the degree of Master of Arts (Education) London University, 1919, and of a paper read before the British Psychological Society (Education Section), March 9th, 1920.

[2] Starch, *J. Educ. Psychol.*, 1912, III. 415 ff.

[3] Carey, this *Journal*, VIII. 170–182.

[4] Howell, *Foundational Study of the Pedagogy of Arithmetic*, 147.

This claim that arithmetical ability is a 'myth' appears ultimately to rest on the pioneer experiments of Stone and Courtis, both of America, who investigated this problem. The former[1] examined very large numbers of children, and found that classes who did well in computation were often inferior in arithmetical reasoning, and *vice versa.* But his methods were such as to test the relative efficiency of the teaching in different schools rather than to discover individual tendencies. Courtis[2] had for his main object the establishment of norms of performance in the various branches of arithmetic. His work in that direction is outside the scope of these remarks. His experimental conditions, however, scarcely warrant his general conclusions about arithmetical ability. He explicitly states that skill in one branch of arithmetic gives little or no indication of skill in another. "If skill in addition does not influence skill in subtraction," he says, "it would seem that there could be little hope of transfer in any other subject also." And again, "It seems as if each operation and part of an operation is learned as a separate and unrelated activity." He has even gone so far as to specify fourteen kinds of arithmetical ability. This view of arithmetic, the writer, adopting Spearman's[3] terminology in another connexion, has been tempted to call 'non-focal.' It is difficult to accept this atomistic explanation. Put in such a way, it suggests a denial of intellective unity in the individual mind—a position some have shown a tendency to adopt since formal training became a subject of acute controversy.

Winch[4] and Burt[5] have, on the other hand, independently found high correlations between computation (rules) and problems. Burt has suggested that there is a special form of ability, operative in arithmetical work generally. The question is one of importance. For the practical teacher, it is convenient to know to what degree ability in any form of arithmetic has prognostic value. For the educational psychologist, the determination of the existence of mental relationships becomes a useful contribution towards a science of education. There thus seemed ample justification for a further inquiry into the whole matter. The writer had the opportunity of doing this in a London elementary school.

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[1] Stone. (*a*) *Arithmetical Abilities*; (*b*) *J. Educ. Psychol.*, 1910, I. 608 ff.

[2] Courtis. (*a*) *J. Educ. Psychol.*, 1910, I. 610; (*b*) *Elementary School Teacher*, Chicago, X. 58–74, 177–199; XI. 171–185, 360–370, 528–539; XII. 127–137.

[3] Spearman and Hart, this *Journal*, V. 53.

[4] Winch, *J. Educ. Psychol.*, 1910, I. 557–589.

[5] Burt, *Educational Abilities*, 58.

II. SCHOOL MARKS FOR RESEARCH PURPOSES.

For the purpose of the research, which took the 'statical' form of a survey, school marks, though available, were not utilised for reasons which may be briefly summarised, and which, it is believed, are of general validity.

(1) For accurate scientific measurement, they are usually far too crude, and often lie within too narrow a range. Teachers have too little time and opportunity for a careful and detailed estimation of the value of school work.

(2) School marks are given to a great extent for disciplinary reasons, and thus represent factors other than mental ability. The writer knows one successful teacher, who prefers to allot marks in composition solely for the effort to improve, and thus does not take into account the absolute value of the exercise.

(3) School marks frequently contain spurious elements, as, for instance, marks for neatness in working problems in arithmetic, and for spelling in composition exercises.

(4) When a series of marks reaching over a considerable period is used, conditions are improved but little, for dislocation is caused by absences; and averaging is not satisfactory, for exercises vary in difficulty.

(5) When, as in composition, a definite objective standard for marking is impracticable, it is very difficult for the marker to preserve a uniform standard while dealing with a large number of exercises.

(6) In the present investigation, school marks did not cover the whole field of inquiry.

Starch's[1] examination of school marking sufficiently reveals its unreliable character for a psychological investigation. In general, the value of the marking for this purpose tends to be discounted by the fact that it is done for the benefit of the pupil. The main object is the detection of errors with a view to the prevention of their recurrence.

III. DESCRIPTION OF THE TESTS AND THE METHOD OF MARKING.

The tests which form the main topic of this article were carried out between Easter and Whitsuntide, 1918, in a boys' school of south-east London. The five classes that took the tests corresponded with Standards 3, 4, 5, 6, 7. As the educational year was just beginning, the children were examined in the work of the standards they had just left. The complete results for about 200 children form the basis of the statistical work done.

The number of tests given was large, and the period over which they extended was 6 weeks. When a child through absences had omitted several tests, his work was excluded from consideration in the final results. To secure uniformity of conditions, and to eliminate, as far as possible, features undesirable in a careful inquiry, all the tests were personally conducted by the writer. As it was necessary for obvious reasons to limit the period of investigation, the exercises could not always be set under uniformly favourable conditions, and therefore the usual effects of practice in producing improvement were not always evident. It was felt essential that the boys should not do the work in a fatigued or listless state. Therefore, all the testing was done in the morning, or early afternoon. In order that the children should be stimulated to do their best, the whole series of exercises was made to become, from their point of view, a school competition for success in which certain small prizes were given. The exercises were first marked as ordinary school work, the results being well advertised to the pupils. Afterwards, the papers were re-marked for the purpose of this investigation. On the whole, therefore, the exercises may be accepted as representing the genuine efforts of the pupils.

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[1] Starch, *Educational Measurements*, Chap. II.

The assumption directing the research was that arithmetic has a trifold aspect:

(*a*) Computation, the mechanical aspect.
(*b*) Rules, the knowledge aspect.
(*c*) Problems, the intelligence aspect.

Each of these was tested five times for each class, and mental or oral arithmetic as a typical class exercise was also included.

(*a*) For computation, each pupil on each occasion was provided with a sheet of paper, containing a number of cyclostyled exercises in simple addition, subtraction, multiplication, and short division. The number of examples of each of these four 'fundamental' processes was so great that no child could finish any one of them in the allotted time of 3 minutes. The tests were so planned that all the possible combinations of figures were included. The order in which the four 'fundamentals' appeared varied from paper to paper. On the first paper for each class, the order was addition, subtraction, multiplication, division; on the second, subtraction, multiplication, division, addition; and so on. The sums in any one fundamental were of uniform length for each class. Each class had its own standard of length of sum for each rule. Below are given typical specimens of the tests in subtraction.

Standard 7	Standard 6	Standard 5	Standard 4	Standard 3
8234759	700814	85463	9307	581
4456382	463572	26048	4546	232

The procedure for each test in computation was uniform. The general directions were, "Work carefully, and try to get as many examples right as possible. Don't revise your work." Before the working of the next test on the paper, an interval of two minutes was allowed, no boy being permitted to look at his next task. During this interval, a little pleasant conversation between the writer and the boys usually took place. In this way, the four fundamentals were tested once each in 20 minutes. Five such sessions completed the computation work for one class.

The exercises were marked separately for speed and accuracy. Each complete sum involves a certain number of operations or steps. In a simple addition sum, for example, the number of steps is usually one fewer than the number of digits. The total number of steps, which the child worked in 3 minutes, was taken as his measure of speed. It was recognised that multiplication has addition steps, and that division has subtraction steps. In subtraction, care was taken to insert a due proportion of steps where 'borrowing' was not required. Errors would have been far fewer, if steps requiring 'borrowing' had been invariably inserted, for children tend to make this process habitual, instead of performing the necessary elementary act of reasoning.

It is legitimate to take, as the measure of computative *efficiency*, the number of correct steps, or operations done in a certain time. But a score thus made depends primarily on the speed of working. On this system, a child who completes 100 steps in computation, and makes five errors scores 95, one who completes 94 steps, all correct, scores 94. Clearly this plan will not do for the measurement of accuracy. In the present tests, accuracy was measured by the frequency with which errors occurred. Thus if a child worked 175 steps, and made five errors, his score for speed was 175 and for accuracy 35, *i.e.* 175 ÷ 5. Infinity thus became a possible score, but as correlations were worked out by Spearman's Rank formula, no difficulty was experienced.

(*b*) In all classes, except the lowest, five tests each were set in rules, the knowledge aspect of arithmetic. These included most of the types of sums that had been taught the children at some time in their school career. There was no rigid time limit. The children were directed to work the examples carefully, and to give up their exercises as soon as they had finished.

Each test included six examples, and five was the maximum mark obtainable for a sum. The assumption was thus made that all the examples were equal in difficulty, as all represented subject matter that had at some time or other been taught the children. Marks were deducted for defective knowledge of method, or of the general facts of arithmetic. A child lost marks, for example, who could not obtain the correct remainder to a sum in short division by factors, or who assigned the wrong number of yards to a mile. In accordance with the plan to consider in marking only the characteristics peculiar to the phase of arithmetic being tested, no notice was taken of errors in calculation.

(*c*) Five sets of arithmetical problems were given to each class. Each set contained six examples, varying considerably in difficulty. As the object was not to obtain norms of performance applicable to the school as a whole, and probably approximating to general validity, each class was provided with its own special work, and again no time limit was imposed.

A more elaborate system of marking than that adopted for rules was deemed necessary. The more difficult problems should receive the more marks. But it is impossible to decide beforehand upon the relative difficulty of the examples. It is not satisfactory to value problems according to the number of steps involved, for the difficulty of a problem depends on many conditions other than this. Stone[1] set his problems to classes outside the main scope of his experiments, allowed unlimited time, and held that the order of difficulty of the examples corresponded in each case with the number of correct solutions. In the present case, as the schools were working under war conditions, it was considered undesirable to impose burdens on other teachers, but it was decided to determine the relative difficulty of the problems from the results of the actual testing, and to allot marks accordingly. The broad principle was adopted that the assessment value of a problem should depend in some inverse way upon the number of children who worked it correctly[2]. The plan of a sliding scale was eventually chosen, and formed the basis for marking. Table I shows the sliding scale for Standard 4 (number of children, 41).

Table I.

No. of correct solutions to problem	Maximum mark
35—41	4
30—34	6
25—29	8
20—24	10
15—19	12
10—14	14
5— 9	16
1— 4	18

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[1] Stone, *Arithmetical Abilities*, 13.

[2] See also Buckingham, *J. Educ. Psychol.*, 1914, v. 204.

Thus, it was possible to recognise that the partial solution of a difficult problem may be more deserving of credit than the complete solution of an easy one. The marking of correct steps towards a solution had to be done in an arbitrary way, but was based on the principle that, in an arithmetical problem, the whole is greater than the sum of its parts. While partial solutions in proportion to their value received credit, the noetic act of thought, as manifested in the comprehension of the whole situation, was considered as the vital factor. Every endeavour was made to mark merely for arithmetical reasoning. Inaccuracy of computation was disregarded, the time element was eliminated, and if a child's knowledge of facts proved defective, for example, if he stated that 2240 yards make a mile, he was not penalised.

Five tests were given to each class in mental or oral arithmetic. The examples included computation and rule-work. They were dictated at a suitable speed, and the children wrote down the answers. One mark was allowed for each correct answer.

It has been stated that one object of the investigation was to determine whether a specific mental factor operates in all arithmetical work, or whether the non-focal views, previously outlined, represent a closer approximation to truth. In order that this point might be dealt with, some means had to be devised for the elimination in the final comparisons of any factor operative in school work generally, such as general intelligence, diligence, and long-distance memory. Thus, in addition to the arithmetic, tests were set in composition, spelling, reading, geography, and history.

Among the subjects for composition were—how we depend on one another; what children can do to help in war time; advantages and disadvantages in war time of Britain's insular position. No maximum mark was fixed. All ideas that contributed to the proper development of the theme were valued, due credit being given for any points that appeared to be of special merit. The sum of such marks was found, and duly accredited for subject matter. For literary style, these marks formed the basis, deductions being made for grammatical faults and other blunders of style for which the child might reasonably be accounted responsible. Thus, a child who scored 30 marks for matter, and who was penalised for 12 blunders, scored 18 for style, his total being 30 + 18, *i.e.* 48. Fertility and originality of thought were taken as the most important factors in composition. It is a task of no little difficulty to assess composition exercises with any degree of confidence, and the adoption of a quasi-objective standard seemed an experiment worthy of trial. It is by no means impervious to criticism, but it had at the time a distinct suitability to the school where it was tried.

For spelling, two sets each of 60 words were dictated. The words ranged from easy to difficult, and the number of correctly spelt words was taken as the score.

Reading was examined as an ordinary school subject. It was tested orally, marks being given for fluency, accuracy and expression. No use was made of the scales[1] for the testing of reading, which have been framed by Thorndike, Gray, Starch and Courtis.

The amount of knowledge of the facts of history or geography which the boys had acquired was measured by two sets of twenty questions each in each subject. The two lowest classes took history only. The questions, generally answered in a word or two, tested memorising power, and not the ability to make deductions from geographical or historical data. Two was the maximum mark for an answer, and 40 for a whole paper.

Furthermore, it appeared advisable to obtain measures of general intelligence, as independent checks to the work. To every class but one, six tests of intelligence were given, each repeated twice. In Standard 7, for certain reasons, there could be but four. All the intelligence tests were of a linguistic character. Simpson[2], describing his researches into the correlations of school subjects, mentions four tests which, he claims, bear a high relation to general intelligence. These are the mutilated text or completion test as devised by Ebbinghaus, opposites, the memory of unrelated words, and the memory of passages of literature. Examples of these four tests were each given twice in the early stages of the testing. The results, however, were not in all cases satisfactory. Some of the tests require very careful adjustment before they can be given with advantage. Slight modifications of procedure may make all the difference between failure and success.

The tests in opposites were not carried out with any relation to reaction times. The children were required to write out the opposites to a number of words presented to them. Though the two tests showed adequate correspondence with each other, the results would have been more reliable if larger lists of words had been presented.

Neither of the memory tests proved particularly satisfactory, and both were abandoned. There is no general agreement about the connexion between memory and intelligence. While Ebbinghaus[1] has denied that there is high relationship between the two, Burt[2] in a series of tests of immediate memory found a close connexion, but, in common with other investigators[3], hesitated to draw very definite conclusions.

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[1] (*a*) Starch, *Educational Measurements*; (*b*) Chapman and Rush, *Scientific Measurement of Class-room Products*.

[2] Simpson, *Correlations of School Subjects*, 8, 9, 10.

Results of greater reliability were obtained in further intelligence tests, including analogies or mixed relations, the re-arrangement of disordered sentences, and a modified form of the completion test.

IV. RESULTS OF THE SURVEY.

For the purposes of the survey, the method of correlation was employed throughout, and the first crude results were treated by means of Spearman's Foot-Rule formula[4]. The R-values obtained from this formula were changed into the r-values of the Bravais-Pearson formula by means of conversion tables, calculated by Spearman from the empirical formula $r = \sin\left(\frac{\pi}{2} . R\right)$. As the number of raw correlations calculated in the survey was over 2000, it will readily be seen that the simpler method, though less exact, was the more suitable for use. The probable error of R may be taken as $0{\cdot}43 \div \sqrt{N}$, where N = number of children tested, and it is thus constant for any one class. As the numbers of children in Standards 7, 6, 5, 4, 3 were 36, 25, 43, 41, 51, the probable errors of R may be taken in order as ·07, ·086, ·065, ·067, ·06. All of these quantities must be raised proportionately when R-values are converted into r-values, and again when the correction formula is applied. To furnish proof of real correspondence between two series, correlation coefficients should be considerably in excess of their probable errors. Generally speaking, correlations in the survey below ·30 are to be viewed with some lack of confidence.

To make due allowance for the child's variability in performance, use was made in the investigation of the Spearman correction formula[5],

$$r(S, T) = \frac{r_1(S, T)}{\sqrt{r(SS_1) . r(TT_1)}} .$$

Here $r(S, T)$ stands for the true correlation between S and T, $r_1(S, T)$, the average of the raw or single correlations between S and T, $r(SS_1)$ and $r(TT_1)$, the average reliability coefficients of the raw gradings of

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[1] Whipple's *Manual*, 386.

[2] Burt, this *Journal*, 1909, III. 143.

[3] (*a*) Winch, *Ibid.*, 1904–5, I. 134; (*b*) Whipple, *op. cit.*, 386.

[4] Spearman, this *Journal*, 1906, II. 89 ff.

[5] Udny Yule, *Introduction to Theory of Statistics*, 213.

S and T. This formula[1] is based on the assumption that the magnitude of errors of measurement between two mental characters can be estimated by comparison with the size of discrepancies found when measuring repeatedly each of the characters in turn. r_1 (S, T) should theoretically be the geometric mean of the raw correlations[2]. But the difference between this and the arithmetic mean is in practice very small, and accordingly, the examples of Spearman and Burt[3] have been followed, the arithmetic mean being selected for ease of calculation. The correction formula has been used freely, and indeed in some cases where reliability coefficients have been scarcely high enough, and the conditions of its legitimate employment barely realised.

It was not thought expedient to pool into one statistical table the results for several groups of children[4]. There are distinct objections to the compilation of the marks obtained from a number of children at all stages of mental development. As a result of the separate treatment of each class in the survey, certain differences between older and younger children are indicated. One of the points that emerges in this discussion is that each class has its own peculiar characteristics, and the presentation of the results of each separately should thus be a powerful check to premature generalisation. On the other hand, a very strong case will be advanced if it can be shown that certain tendencies exhibit themselves in every group, whatever its distinctive features may be.

A typical specimen of the inter-correlations between the results of the tests in the speed of working one of the fundamentals is shown in Table II.

Table II.

A, B, C, D, E = the five tests in speed of multiplication, Standard 7.

	A	B	C	D	E
A	—	·85	·59	·67	·65
B	·85	—	·51	·67	·69
C	·59	·51	—	·51	·55
D	·67	·67	·51	—	·61
E	·65	·69	·55	·61	—

Average, ·63.

An examination of this table shows that, even in exercises as simple and uniform in character as these, the individual child exhibits marked variability in performance. The average reliability coefficients, shown in Table III, are not always high, the lowest being ·56 in the case of multiplication for Standard 6. In a few cases in various parts of the school, single correlations between two measurements of the same fundamental are lower than ·50, though in no single case is there one below ·40.

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[1] Spearman, this *Journal*, 1910, III. 271.

[2] Brown, *Essentials of Mental Measurement*, 83.

[3] Burt, this *Journal*, 1909, III. 110.

[4] See Spearman, *Ibid.*, 1910, III. 278.

Table III.

Average reliability coefficients in the four fundamentals.

	Standard				
	7	6	5	4	3
Addition ...	·62	·68	·68	·67	·64
Subtraction ...	·77	·63	·67	·67	·70
Multiplication ...	·63	·56	·78	·76	·73
Division ...	·72	·63	·75	·70	·62

The correlations generally appear lower than those found in many other investigations. This is due to the comparative homogeneity of the groups tested. These, coinciding with the Standards, were far more uniform in attainments than groups formed on age-bases would usually be. The latter will in general cover a far greater range of ability than a class group, and both the clever and the dull children, despite the factor of variability in performance, will exercise a steadying effect on correlations. In proportion as such influence is removed by effective organization, so will emphasis be thrown on the performance of children of moderate and fairly even ability from the point of view of the intellectual class level, and the natural lack of steadiness in the child's work will produce the greater disturbance.

The average corrected coefficient of correlation between the four fundamentals (see Table VIII) is ·74, Standard 6 having the lowest ·67, and Standard 7 the highest ·78. Standard 6, a remarkably homogeneous group, shows a corrected correlation of only ·35 between subtraction and division. Apart from this, the lowest corrected correlation is ·55 between addition and division in Standard 3.

Table IV.

Percentages of error in the fundamentals.

	Standard				
	7	6	5	4	3
Addition ...	1·4	1·6	2·1	2·5	2·8
Subtraction ...	3·6	3·6	5·0	9·9	8·3
Multiplication ...	2·1	1·7	2·1	3·8	8·1
Division ...	1·5	1·9	1·9	8·5	18·6

The computation exercises were marked for accuracy as well as speed. It will be remembered that the measure of accuracy adopted was the frequency with which errors occurred, or the ratio of error to the work done. Table IV summarises the results considered from this standpoint.

In the progress through the school, as the processes become familiar to the children, the frequency of errors diminishes, and finally, viewed in relation to the work done, the proportion of error may be insignificant. In the top class, almost 98 per cent. of the steps worked are quite correct. Similar results are found in nearly all the computation exercises. This is the main justification for adopting a speed standard only, as a measure of computative ability. It matters little, in most cases, whether the number of steps or the number of *correct* steps is taken as the measure. The former criterion is less satisfactory where in the lower classes the frequency of error becomes high.

Although the proportion of error appears to be so small, the effect has been to make a quarter of the complete sums wrong. The fact that small slips have such great results is one of the most powerful of arguments for making the elementary operations of computation as mechanical and certain as possible. Arithmetical teaching can hardly be accounted a failure when the child gets 98 per cent. of his calculations right. On the other hand, the young clerk who gets a quarter of his results wrong is not a very valuable asset to the business world. The point has little psychological value, but it has an important bearing on the question of the alleged inaccuracy of school children in computation.

Generally speaking, so small is the frequency of error, that the effect of a single slip on the child's rank for accuracy may be considerable. A cursory examination of the original mark sheets would show clearly that any single exercise of an average child, marked for accuracy, is of small value as a guide to other performances. Table V gives a typical specimen of inter-correlations for accuracy.

Table V.

Multiplication (accuracy), Standard 7.

	A	B	C	D	E
A	—	·31	·45	·14	·43
B	·31	—	·25	·14	·17
C	·45	·25	—	0	·29
D	·14	·14	0	—	·14
E	·43	·17	·29	·14	—

Average, ·23.

They are, as a rule, very low, and in many cases approach zero. Even a few negative coefficients have been obtained, and in few instances are the correlations sufficiently in excess of their probable errors to be of significance. That is to say, while a few children may be consistently accurate, and a few consistently inaccurate, it is extremely difficult to grade a group as a whole for accuracy, or to decide, in the general case, whether one boy is more or less exact in calculation than another. In a competitive examination, when the whole future of the child may lie in the balance, it thus appears inexpedient and unjust to penalise him with too great severity for slips in calculation, the tendency to which is in all probability enhanced by the abnormal conditions of the occasion.

This statement is not intended to undervalue the importance of accuracy in calculation. For here work must be perfect or it is useless. Perfection is not a necessary attribute to proficiency in the subjects of the curriculum in general.

So low were the reliability coefficients, shown in Table VI, for accuracy in computation, that the conditions for the legitimate application of the Spearman correction formula were not realised. The final measure of accuracy taken for each child was simply the ratio of the number of steps in computation done in all the twenty exercises to the total number of errors. By the use of this measure, it was possible to make a comparison between accuracy in arithmetic and proficiency in other subjects. The former shows little sign of being a significant mental factor in the intellectual ability of young children.

Table VI.

Reliability coefficients for accuracy in the fundamentals.

	Standard				
	7	6	5	4	3
Addition ...	·20	·16	·40	·16	·40
Subtraction ...	·29	·19	·35	·50	·43
Multiplication ...	·23	·05	·30	·32	·34
Division ...	·10	·22	·18	·32	·45

The relation of speed to accuracy has been the subject of previous experiments. W. Brown[1] found, in three groups of children tested, positive correlations of ·13 ± ·08, ·24 ± ·10, ·33 ± ·10. These correlations are small in relation to their probable errors. The general inference is that speed and accuracy tend to go together. Thorndike[2] has arrived at a similar conclusion.

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[1] Brown, *Essentials of Mental Measurement*, 103, 104, 145, 146.

[2] Thorndike, *J. Educ. Psychol.*, 1914, v. 537.

Table VII.

Correlations between speed and accuracy in computation.
(Uncorrected correlations of the amalgamated totals.)

	Standard				
	7	6	5	4	3
Addition ...	·13	·40	·23	·03	·32
Subtraction ...	– ·13	– ·01	·07	·38	·35
Multiplication ...	– ·06	·13	·08	·13	·64
Division ...	·50	·14	·28	·43	·52

The results of the survey, appearing in Table VII, are somewhat less decisive than the previous ones. In seventeen cases out of twenty, positive correlations exist, supporting on the whole the conclusions of Brown and Thorndike. In so far as speed and accuracy are both saturated with general educational ability, or special arithmetical ability, the quicker will tend to be the more accurate. But there may sometimes occur in a class group, a proportion of the slow-and-sure type large enough to produce a negative correlation. Under the conditions of the experiment, the child was aiming at both speed and accuracy at once. On account of the instruction not to revise his work, the child made more mistakes than would be viewed with complacency in ordinary class exercises. This suited the purpose of the experiment. The children worked at their maximum speed, and betrayed something like their maximum tendency to inaccuracy.

It is interesting to note that the amount of work done by any one class in any one rule varied but little from test to test. Where the greatest slackening in the rate of performance in a class took place, it was usually found due to some unfavourable circumstance immediately preceding the testing. Such conditions too will explain at least partially the reason why there was not found a steady increase in efficiency with the progress of the tests. Again, there was but little variation in the percentage of errors made by any one class in any one fundamental. There thus appears justification for reliance on the results of a single test, when the object is to ascertain the standard of proficiency attained by a class as a unit in both the characters of speed and accuracy.

Generally speaking, the corrected correlations between the fundamentals, appearing in Table VIII, are much too high to support the non-focal view of arithmetical ability. No claim is advanced that the ideal of mental measurement has been reached in the present investigation. More refined methods for the detection of inter-relationships of mental capacities have been suggested[1]. The general effect of such is to increase correlations[2]. Apart from experiment, it seems hardly to be expected that ability in one of the fundamentals should be widely different from ability in another, for in all there are powerful common elements—the mechanical manipulation of figures, the inter-connexions of the arithmetical tables, and the inclusion of the time factor.

Table VIII.

Corrected correlations between the fundamentals.

Standard	Add.-Sub.	Add.-Mult.	Add.-Div.	Sub.-Mult.	Sub.-Div.	Mult.-Div.
7	·85	·81	·64	·90	·69	·80
6	·85	·96	·58	·65	·35	·61
5	·88	·75	·60	·79	·69	·66
4	·80	·81	·71	·77	·73	·78
3	·83	·74	·55	·94	·78	·65

A specimen of the inter-correlations between the separate tests in problematic arithmetic appears in Table IX. The causes that militate against high correlations include the variability in form of the individual child, due to a complexity of conditions. His existing physical state will be more or less favourable to mental testing. Then, assuming that the child's mental store is adequate to the occasion, the recall of the appropriate principle depends on suggestion[3]. The mental act required for the solution of a problem is essentially noetic. The form of the situation must be cognised as a whole. In so far as the child achieves proficiency

Table IX.

Inter-correlations for Arithmetic (Standard 7).

Tests A, B, C, D, E in each case.

Problems

	A	B	C	D	E
A	—	·43	·34	·44	·25
B	·43	—	·45	·74	·75
C	·34	·45	—	·69	·50
D	·44	·74	·69	—	·57
E	·25	·75	·50	·57	—

Average, ·52

Rules

	A	B	C	D	E
A	—	·51	·50	·45	·52
B	·52	—	·45	·54	·62
C	·50	·45	—	·29	·64
D	·45	·54	·29	—	·29
E	·52	·62	·64	·29	—

Average, ·48

Mental Arithmetic

	A	B	C	D	E
A	—	·67	·67	·52	·56
B	·67	—	·55	·43	·70
C	·67	·55	—	·40	·51
D	·52	·43	·40	—	·57
E	·56	·70	·51	·57	—

Average, ·55

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[1] Spearman and Hart, this *Journal*, 1912, v. 55. See also Webb, "Character and Intelligence," *op. cit.*, 36; Carey, *op. cit.*, 172.

[2] Nunn, *Exercises in Algebra*, 511.

[3] The writer has followed Dewey in this use of the term 'suggestion.' There is something casual about the way in which the key-idea to the problem comes into the mind. Dewey, *How we think*, 26, 34; Nunn, *Education*, 47.

in this direction, his activity is of the nature of play[1], the expression of his surplus mental energy. But in a genuine difficulty there is no certainty that fruitful suggestion will occur. It implies, as Dewey says, a "leap into the unknown," for "thinking is primarily something which happens to us."

When suggestion comes to the child, he must receive it critically. The implications of the suggestion must be reviewed in relation to the existing difficulty, and the decision reached whether all the conditions of the situation are congruent with the suggestion or not. A common tendency of the child is to rely upon crude association, the 'part-workingness' of an experience. He is misled by a mere word, or by the partial resemblance of the new situation to an old one. Because of the element of chance about mental suggestion, and the undeveloped condition of the self-critical powers of the young, it is always well to test several times in order to obtain an approximately accurate estimate of ability in arithmetical problems.

It has been suggested[2] that each child probably has a definite and discoverable coefficient of ability in such subjects as problematic arithmetic, that this may at first be obscured by temporary accidental factors, but that, after a number of tests, disturbing conditions will tend to disappear, and, eventually, the particular ability, freed from them, will stand revealed in its true magnitude. This may or may not be the case in the simplified tests of the psychological laboratory. In problematic arithmetic, however, such a speculation is extremely difficult to test. It would be necessary to prepare sets of problems of uniform or uniformly graduated difficulty, and to set these to class-groups under similar examination conditions. In that case, if the speculation were in accordance with fact, the correlations between the later members of a series would, as containing the central 'problem' factor in greater purity, tend to approximate to unity, and a hierarchical arrangement of coefficients would appear. The present tests, given under conditions which could not be made ideal, may be challenged as not conforming to the required conditions. In so far as they go, however, they give little support to the 'steadiness' view. The brief analysis that has been given of the mental factors concerned in working problems will not favour the idea that the child will ever develop 'steadiness' in such a field as this. The great variations which occur between test and test are clearly revealed by the reliability coefficients of the tests, *i.e.* the averages of the inter-correlations, which are shown in Table X.

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[1] Nunn, "Play," *Educ. Times*, Nov. 1912; see also Nunn, *Education*, 79.

[2] An article by Winch describes this view. See *J. Phil., Psychol. and Sci. Meth.*, 1911, 375, 376.

Table X.

Reliability coefficients for higher forms of arithmetic.

Standard	Problems	Rules	Mental Arithmetic
7	·52	·48	·55
6	·49	·41	·68
5	·46	·60	·49
4	·65	·47	·71
3	·65	—	·69

It might by some be thought probable that the two school subjects, arithmetical problems and composition, both of which depend on the capacity of the child for doing independent work involving reasoning, would show on these grounds high correlations. This is, however, not found in a highly marked degree. The two exercises have such divergent elements that proficiency in the one may well accompany mediocrity in the other. There is certainly in this investigation no ground for contesting the modern view that, particularly with children, reasoning power tends to be specific. But neither is it denied that composition and arithmetical problems both take a high place as tests of intelligence. There is a strong probability that the child who excels in both is of high general mental calibre.

We are now in a position to examine the relations between the various branches of arithmetic. Their corrected correlations have been worked out, and a typical illustration of the results appears in Table XI.

Table XI.

Results for Standard 4 (corrected correlations), illustrating the dual character of arithmetical ability.

(Reliability coefficients in brackets.)

	Probs.	Rules	Ment. Arith.	Add.	Sub.	Mult.	Div.
Probs. ...	(·65)	·89	·86	·44	·52	·48	·71
Rules ...	·89	(·47)	·77	·43	·47	·60	·68
Ment. Arith.	·86	·77	(·71)	·47	·53	·51	·69
Add. ...	·44	·43	·47	(·67)	·80	·81	·71
Sub. ...	·52	·47	·53	·80	(·67)	·77	·73
Mult. ...	·48	·60	·51	·81	·77	(·76)	·78
Div. ...	·71	·68	·69	·71	·73	·78	(·70)

An examination of the tables shows that in each class arithmetical ability tends to be represented in two main sub-divisions:

(i) the power to compute with ease and readiness;

(ii) the power to do arithmetical work which involves the application of higher forms of mental activity than are required for (i). Under this heading are included problems, rules, and mental arithmetic.

Table XII summarises the results of all the classes, one of which has been shown more fully in Table XI. In all the five classes tested[1], the evidence is uniformly in favour of the dual character of arithmetical ability. The present investigation thus, up to a certain point, tends to support those of previous researchers, who have not found very high correlations between the fundamentals and arithmetical reasoning. It is therefore necessary to guard against the assumption that the power of facile calculation, to which great importance is attached in these days of commercial enterprise, will necessarily form part of the mental equipment of a child, whose arithmetical attainments may be from other points of view quite satisfactory. There is every justification for some small weekly allotment of time for separate practice in the bare elements of computation with the object of promoting speed and accuracy.

Table XII.

Standard	Average correlation between problems, rules, and mental arithmetic	Average correlation between the fundamentals	Average correlation between (i) fundamentals and (ii) problems, rules, and mental arithmetic
7	·83	·78	·47
	(highest ·93)	(highest ·90)	(highest ·86)
	(lowest ·73)	(lowest ·64)	(lowest ·24)
6	·61	·67	·19
	(highest ·70)	(highest ·96)	(highest ·57)
	(lowest ·52)	(lowest ·35)	(lowest ·30)
5	·78	·73	·48
	(highest ·85)	(highest ·88)	(highest ·73)
	(lowest ·73)	(lowest ·60)	(lowest ·06)
4	·84	·77	·54
	(highest ·89)	(highest ·81)	(highest ·71)
	(lowest ·77)	(lowest ·71)	(lowest ·44)
3	·72	·75	·51
	(highest ·72)	(highest ·83)	(highest ·70)
	(lowest ·72)	(lowest ·55)	(lowest ·29)

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[1] Standard 6, a small group of 25, happened to contain two or three lads, who were unusually good in computation, but who were otherwise poor in arithmetic. It was altogether a difficult standard, from the point of view of the experimenter. On the whole, however, it exhibited the general tendency.

Of the three higher forms of arithmetic, mental arithmetic appears to have the highest relation to computation. This is a perfectly natural result, for though intelligence was required to cope with the varying situations presented in the succession of questions, much of the work consisted of mere calculation. In Standard 7 appears the exceptionally high corrected correlation of ·86 between mental arithmetic and short division. In fact, in most cases, division, the hardest of the fundamentals, has the highest relation of them all to the more intellectual aspects of arithmetic.

In the category of appeal to intelligence, we might arrange the subject-matter of arithmetic thus: (1) Problems, (2) Rules, (3) Mental Arithmetic[1], (4) Division, (5) Subtraction, (6) Multiplication, (7) Addition. Of these forms, those which seem to have the greatest prognostic value are mental arithmetic, and short division. That is to say, the order of merit of a class in either of these two subjects appears likely to be very nearly related to the order of merit for arithmetic taken as a whole.

A comprehensive question arises. Arithmetic, as we have seen, is apparently divided into two sections, with some degree of dissimilarity. Is there any bond between these other than general educational ability—a factor which may be taken as operative in all school work, and the existence of which is indicated crudely by the fact that, with ordinary age- or class-groups of children, correlations in school subjects are usually positive? Is there a specific mental factor operating in all forms of arithmetic, that is, is success in one form of the subject to a special degree symptomatic of success in all?

In order that some answer might be given to this question, tests in composition, spelling, reading, geography, and history were included in the survey. In utilising their results, several methods, more or less satisfactory, are available. In the first place, if the correlations between the various branches of arithmetic are higher than those between any of these and the other subjects of the curriculum, the evidence is in favour of the existence of a special kind of ability common to all arithmetic. In Table XIII, column A shows the average correlation between com-

Table XIII.

Standard	A	B	C
7	·47	·29	·46
6	·19	·0	·29
5	·48	·26	·51
4	·54	·05	·35
3	·51	·26	·46

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[1] In the form in which it was given.

putation and the higher forms of arithmetic, column B between computation and the subjects of the curriculum outside arithmetic, column C between these latter subjects and the higher forms of arithmetic.

In every case, it will appear that computation bears a much higher relation to the other arithmetical processes than it does to the other subjects tested. On the other hand, there is certainly a distinct resemblance between columns A and C, which suggests a further field of inquiry. Similar results might well encourage the belief that there is only the common bond of the central intellective function exhibited in these two columns. But in that case, column B might be expected to show correspondence with the other two columns. This however is not the case. Computation has in general little affinity for anything outside arithmetic. Hence, we are impelled to interpret this relationship as evidence distinctly in favour of Burt's suggestion that there is an essential unity in arithmetical ability.

It is theoretically possible, having given the results of three tests, *a*, *b*, *c*, to determine to what degree any of them is saturated by the same general factor, it being understood that there is no specific relation between the mental characters tested by *a*, *b* and *c*. The formula

$$r^2_{ae} = \frac{r_{ab} \cdot r_{ac}}{r_{bc}},$$

by which the degree of saturation of *a* with *e*, say educational ability, may be estimated, is derived by elementary algebra[1] from Yule's formula.

The course adopted was to divide the subjects into three groups,—(1) arithmetical, (2) English, including composition, spelling, reading, (3) geography and history. The division is on the whole justified by the results of the tests. To apply the formula, one subject was selected from each group, the process was repeated with other selections as often as possible, and the average was taken. Table XIV shows the final results for Standard 5. Line A at the bottom shows the degree of saturation of each subject with general educational ability. Secondly, for checking purposes, a modification of a method used by Burt[2] was employed. The results of this are shown in line B.

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[1] If there is no specific relation between the tests

$$r_{ab\cdot e} = 0,$$

$$\therefore\ r_{ab} = r_{ae} \cdot r_{be};$$

also $$r_{ac} = r_{ae} \cdot r_{ce},$$

and $$r_{bc} = r_{be} \cdot r_{ce};$$

$$\therefore\ \frac{r_{ab} \cdot r_{ac}}{r_{bc}} = r^2_{ae}.$$

[2] Burt, *Educational Abilities*, 53.

Thirdly, use was made of the various intelligence tests to obtain two measures of the influence of the central intellective function, as nearly as possible with no specific relation to each other. By correlating each of these with the various subjects of the curriculum and with each other, the formula

$$r^2_{ag} = \frac{r_{ab} \cdot r_{ac}}{r_{bc}}$$

could be used, *b* and *c* representing the measures of intelligence, *a* the school subject, *g* general intelligence, a more suitable expression than *e* to use for tests which lay outside the field of school instruction. For the intelligence tests represented natural capacity rather than the effects of training. This method has the advantage of offering an independent estimate of ability, against which the school subjects may be independently measured. As in the other cases, however, it was not possible to eliminate entirely specific factors, or sampling errors. It is to be noted also that the particular intelligence tests employed, suitable as they were for group use, have all a linguistic character, and consequently tend to betray a special affinity for the English subjects.

To obtain the two separate measures of intelligence, most of the tests given were called into requisition, every care being taken to avoid the pitfall of the common specific factor. The two measures were not made up in the same way for each class. For Standard 7, the mutilated text, and opposites tests were used. For Standards 5 and 6, the re-arrangement of disordered sentences was taken for one measure, and a combination of opposites and analogies for the other. Opposites form merely a special case of analogies, an exercise where it is required to find a term which stands in a certain relation to another term. In all cases of combinations of intelligence tests, the question of the suitable weighting of marks was considered. For Standard 4, a combination of the earlier and later forms of the completion test was used for the first measure of intelligence, and for the second a combination of the opposites and disordered words tests. Though these two last-mentioned tests do not appear to show any specific relation, there seems to be no objection to combining them for an independent measure of intelligence. A similar procedure was adopted by Webb[1]. In Standard 3, the opposites test was taken as one measure of intelligence, and a combination of the disordered sentence test and the supplementary completion test for the second. Use was made of the two measures of intelligence in each case to find the degree of saturation of each school subject with *g*. The final results appear in line C of Table XIV. All of these estimates of the influence of a central factor are open to objection, but, with few exceptions, they are substantially in accord.

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[1] Webb, "Character and Intelligence," *op. cit.*, 38.

Table XIV.

Complete table of correlations for Standard 5.
(Reliability coefficients in brackets.)

sp. = speed in computation
* acc. = accuracy in computation
pr. = problems
rul. = rules
men. = mental arithmetic
cp. = composition
spl. = spelling
rd. = reading
his. = history
geog. = geography
I_1 = first intelligence measure
I_2 = second intelligence measure
A = correlation with educational ability
B = check
C = correlation with general intelligence

	sp.	* acc.	pr.	men.	rul.	cp.	spl.	rd.	his.	geog.	I_1	I_2
sp.	(·72)	·16	·26	·47	·61	·07	·42	·40	·19	·20	0	—
* acc.	·16	(·31)	·38	·29	·20	·32	·17	·31	– ·05	·20	—	—
pr.	·26	·38	(·46)	·75	·73	·55	·44	·45	·47	·74	·77	·58
men.	·47	·29	·75	(·49)	·85	·27	·55	·49	·59	·54	·52	·47
rul.	·61	·20	·73	·85	(·60)	·51	·39	·60	·47	·62	·47	·35
cp.	·07	·32	·55	·27	·51	(·74)	·55	·63	·77	·51	·46	·74
spl.	·42	·17	·44	·55	·39	·55	(·87)	·57	·50	·47	·50	·74
rd.	·40	·31	·45	·49	·60	·63	·57	(·79)	·50	·44	·47	·61
his.	·20	– ·05	·47	·59	·47	·77	·50	·50	(·71)	·88	·49	·64
geog.	·19	·20	·74	·54	·62	·51	·47	·44	·88	(·83)	·58	·51
I_1	·0	—	·77	·52	·47	·46	·50	·47	·49	·58	(·81)	·64
I_2	—	—	·58	·47	·35	·74	·74	·61	·64	·51	·64	(·67)
A	·32	—	·74	·68	·72	·64	·72	·74	·75	·76		
B	·35	—	·72	·67	·71	·77	·69	·72	·84	·77		
C	·0		·83	·62	·50	·74	·76	·66	·70	·68		

* uncorrected correlations of the amalgamated totals.

The results of A have been selected as probably affording an approximately accurate expression of the influence of general educational ability. Yule's formula for partial correlation has then been used to eliminate this in order to determine specific correlations between the various branches of arithmetic. The results appear in Table XV.

Without an exception, when the general factor has been eliminated, positive correlations, frequently high, are found between the different forms of arithmetic, indicating that, on the whole, there is a special form of ability which functions in terms of number instead of words. The results of the application of the method of partial correlation therefore corroborate those of the simpler Table XIII, which shows that mental arithmetic, rules and problems are closely connected with each other, and that skill in computation is allied to these, but not, as a rule, to other subjects of the school curriculum.

Table XV.

Specific correlations in arithmetic.

C = computation P = problems
R = rules M = mental

	Standard 7			
	C	P	R	M
C	—	·29	·48	·25
P	·29	—	·64	1·00
R	·48	·64	—	·87
M	·25	1·00	·87	—

	Standard 6			
	C	P	R	M
C	—	·23	·44	·04
P	·23	—	·60	·29
R	·44	·60	—	·27
M	·04	·29	·27	—

	Standard 5			
	C	P	R	M
C	—	·04	·36	·58
P	·04	—	·50	·42
R	·36	·50	—	·71
M	·58	·42	·71	—

	Standard 4			
	C	P	R	M
C	—	·67	·63	·60
P	·67	—	·74	·81
R	·63	·74	—	·62
M	·60	·81	·62	—

	Standard 3		
	C	P	M
C	—	·27	·59
P	·27	—	·59
M	·59	·59	—

Brown[1] has stated that "the balance of evidence seems to be in favour of the existence of a special capacity or faculty, underlying mathematical ability, distinct from, and with no essentially close connexion with other forms of intellectual capacity." Burt, as we have seen, expresses the belief that there exists a special form of ability, which manifests itself generally in arithmetical work.

The present survey offers additional evidence in favour of these two views. The connexion between the various forms of arithmetic, even when the effect of the many obvious common elements has been reduced to a minimum by the system of marking employed, is so strong as to suggest a specific factor in addition to general educational ability.

It is impossible, therefore, to accept the view that arithmetical ability is a 'myth.' The 'non-focal' views that have been reviewed here are not supported by the survey. The general trend of its results is to show that there is a single specific factor, operative in all forms of arithmetical work, however unlike these may appear to be in psychological appeal.

V. SUMMARY OF CONCLUSIONS.

1. The variability in form of the individual child is apparent in all kinds of mental testing. It is particularly pronounced in problematic arithmetic.

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[1] Brown, *Child Study*, March 1913, 26.

2. Accuracy in computation, as judged by the frequency of error, cannot usually be measured with a satisfactory degree of reliability.

3. Speed and accuracy in arithmetical work are positively correlated.

4. There appears little evidence to support the theory that 'steadiness' in ordinary school performances may be expected after a number of tests.

5. The intelligence and the knowledge aspects of arithmetic, exemplified in problems and rules respectively, are highly correlated. The fundamentals are highly correlated with each other, but less closely with problems and rules. There thus appears a certain degree of duality in arithmetical ability.

6. Owing to the influence of general educational ability, all school subjects have a tendency to be positively correlated in ordinary class groups. There might therefore be a temptation to ascribe to this general factor the correlations found between computation and the higher forms of arithmetic. Educational ability is however inadequate as an explanation.

7. The evidence of the survey is distinctly in favour of the proposition that there is a single specific mental factor, operative in all kinds of arithmetical work.

(*Manuscript received* 26 *April*; 1920.)

THE CAUSES OF THE WAR IN CURRENT TRADITION

By W. T. WAUGH.

In a recent public examination, the History paper contained the question, "Why did the German Government provoke war in 1914?" The candidates were boys and girls of fifteen or sixteen, and came from various parts of the north and midlands. Their object was to qualify for admission to a teachers' training college, and it may be presumed that they were among the most intelligent and successful pupils in their respective schools.

Very few candidates attempted the question, and those that did generally limited themselves to a bald summary of what they believed to be the events of July and August, 1914. The answers were mostly inaccurate and uninstructive, and several revealed nothing but astonishing ignorance. Two, or three, however, have some claim to the attention of those interested in the systematic study of testimony. They should, in my opinion, be regarded as products of oral tradition, for in 1914 the writers were not old enough to understand the momentous events which were taking place, and their present knowledge of them is probably based for the most part on what they have heard from teachers or parents.

The three following passages seemed specially instructive, and worthy of being quoted at length. Each is from a different candidate:

(1) "Germany thought that if she broke what was called the 'Scrap of Paper,' she would be able to attack England more easily. The treaty which she was breaking said that Germany could not use Belgium as a means of transporting troops in time of war. The breaking of the treaty annoyed France as well as Belgium so she allied with Belgium against Germany....England saw that if she did not help France and Belgium, Germany would more easily be able to attack her when she had conquered France, so England joined France and Belgium. These three allies were known as the Triple Entente." The sequel concerns the murder of the Archduke Franz Ferdinand; it is generally true to fact and calls for no special remark.

(2) "Kaiser Wilhelm II...wished to strike at England and so attacked Belgium and took Alsace-Lorraine—two states which were given to Germany at the close of the Franco-Prussian war, 1871, but which being French states were given back to France later on, by the wish of the

people. After ravaging Belgium the Germans attacked the northern borders of France. England was allied with France and so went to her aid. This brought about the thing that Germany wanted, *i.e.*, war with England."

(3) "The Serbian Crown Prince was murdered by an Austrian, so Serbia declared war on Austria. Russia was bound to help Serbia since it was to her own advantage, and Germany stood by Austria. Then France had to join in with Serbia and Russia because of her long and bitter hatred of Germany, thinking she would retaliate for the Franco-Prussian war of 1870. To invade France, Germany would have to cross through Belgium, and though she was warned not to by the other powers she still persisted in doing so, and England was in honour bound to protect Belgium." What follows reveals a fairly accurate knowledge of the principal treaty which guaranteed Belgian neutrality.

These extracts, with the evidence afforded by other papers, show plainly that, even before the war was formally ended, the rising generation had utterly confused notions of how it began. The political significance of this, though serious, cannot be discussed here. The problem for the psychologist is to analyse and explain the distortions of fact which mark the three passages quoted and indeed all the answers to the question. Their main source is evident—namely, a desire, unconscious it may be, to make things easy, and in particular to bring the events narrated into obvious harmony with certain fundamental assumptions. It was generally taken for granted by the candidates (*a*) that Germany was in the wrong, (*b*) that Germany was the aggressor, (*c*) that Germany's main object was the overthrow of England. These three postulates were clearly accepted by the writers of extracts (1) and (2) quoted above; the writer of extract (3) accepted postulate (*a*) and, so far as Belgium and England were concerned (*b*) also, but does not seem to have been affected by (*c*). A very interesting illustration of the influence of postulate (*a*) is afforded by the remarkable inversion of the facts that occurs in the first sentence of extract (3). The writer knew that the murder of the heir to a European throne marked the beginning of the crisis. She had no doubt that Germany was the villain of the drama that followed. Therefore the murderer must have been, if not actually a German, a subject of one of Germany's allies in the war, and the victim a subject of one of her enemies. Similarly, the astonishing statement about Alsace-Lorraine in extract (2) seems to be due to the influence of postulate (*b*). If Germany was the greedy aggressor, how came it that, at the end of the war, she had to surrender some of her own territory to a state which (*ex hypothesi*) had been fighting in self-defence? Why, because Alsace and Lorraine had belonged to France at the outbreak of war, and, like Belgium and north-east France, had been overrun by the German invaders. But the writer could not forget that Alsace and Lorraine had been annexed by Germany in 1871, and she therefore had to invent a subsequent cession of the provinces to France, the principle of self-determination being ingenuously called in to lend an air of verisimilitude to the story. It is not surprising that she was confused, for the terms of peace are supposed to have involved "no annexations," and in public discussion Alsace and Lorraine have commonly been treated as if they were on precisely the same footing as Belgium.

The third assumption—that Germany's main object was the overthrow of Britain—exercises a strong influence on extracts (1) and (2). The writers evidently felt that Germany's principal motive must have affected her conduct of the war from the outset, and they accordingly adduce it to explain the violation of Belgium neutrality, though how this was expected to promote the destruction of Britain is not made clear. It appears, in fact, that none of the candidates really understood the significance of the invasion of Belgium. Of those cited above, the first two failed to realise that it was the immediate cause of Britain's entry into the war; and the third, while grasping the idea of neutrality and the obligations of Britain towards Belgium, was led away by her ignorance of geography from the true explanation of Germany's action. But in each case, it should be emphasized, the version presented is simpler than the actual facts. In the first two extracts Germany invades Belgium because such a course will directly serve her object of destroying Britain; in the third she does so because there is no other way of getting at France. Either explanation is much easier to understand than the strategic plans of the German General Staff.

The same tendency towards simplification is revealed by the reluctance with which the candidates touched upon the predisposing causes of the war. Few of them felt sure of their ground when they ventured further back than the murder of the Archduke, and most tried to indicate that the events of the last few weeks before the war were sufficient to account for what followed. The writer of extract (1), indeed, goes so far as to make the formation of the Triple Entente a consequence of the invasion of Belgium.

Of course some of the misapprehensions and omissions of the candidates are due to merely fortuitous influences and possess no interest for the scientific student of evidence. The cynicism of extract (3) is probably unconscious, the writer, one may suggest, having imperfectly grasped a lesson given by some teacher of judicial temperament. Very interesting, on the other hand, is the treatment of the "scrap of paper" phrase in extract (1)—a complex instance of a process familiar to all historians, whereby a word or phrase used originally in scorn or irony loses its sting and acquires an inoffensive or even honourable significance[1]. The disappearance of Russia from the Triple Entente—another notable feature of this passage—is natural in view of recent events; it is a further example of harmonization, only in this case the criterion is the situation at the time when the examination was held. It is, however, astonishing that Italy is not named as a party. Apparently the writer knew more about the course of the war than a first reading of her answer might lead one to suppose—a wholesome reminder that most people's memories are capricious to a degree that is seldom realised even by trained investigators of evidence.

It may be urged that the interest of the passages quoted is vitiated by the fact that they were written by children, who, however intelligent, lacked the experience and knowledge essential to a comprehension of international politics, and that grown men and women, even though their information had been derived from precisely the same sources, would have answered the question very differently. That may well be; but it is extremely doubtful whether they would have answered it any better. For there is much to be said for the view that boys and girls in the higher forms of our secondary schools are in general keener on knowledge, more "gleg in the uptake," more critical, and better-informed on matters outside their everyday experience, than the majority of their adult compatriots, while there can be no question as to the normal superiority of their memories. And, in comparison with most of the witnesses on whom, in the last resort, our knowledge of history depends, they have all the advantages just enumerated, together with a faculty of expression which, until the present age, was possessed by few. If, in short, allowance ought to be made for the youth of the writers whose work we have been considering, it is because, as witnesses, they are likely to be above the average.

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[1] Well-known examples are the terms Whig, Tory, Methodist, Quaker. But their development is relatively simple, and I cannot recall a case quite parallel to the one before us. The phrase was first used by Bethmann-Hollweg in contempt, then taken up by Germany's enemies and used in irony, and now, it appears, there is a possibility of its becoming regarded as the name by which, even before the war, the Treaty of London of 1839 was commonly known.

A THEORY OF BINAURAL HEARING.

BY HENRY J. WATT.

IN recent years notable additions to exact knowledge of the binaural functions of hearing have been made, especially by O. Klemm[1]. The evaluation of such results, however, is greatly impeded by the lack of any promising basis upon which to construct a theory of them. In my *Psychology of Sound*[2] I ventured to propound a theory of the direct basis of binaural localisation that arose inevitably out of the interpretation of pitch-differences as ordinal or positional. This theory, as I showed, seems to meet satisfactorily both the main facts and the introspective descriptions of binaural processes. I should like now to show how it is confirmed by various recent delicate observations and to comment briefly on certain outstanding problems.

My theory may be essentially described as the application to hearing of Bernstein's theory of single touch from double contact[3]. As a result of continued consideration of the problems of hearing I am now inclined to look upon the auditory field as the exact psychical replica of its anatomical structure. This view of course, has the merit of bringing hearing into line with the other senses and of satisfying the general postulates of psychophysical parallelism, for the cerebral-psychical parallel at least. As von Frey suggests for the skin sense[4], it seems probable in hearing that a small number of nerve fibres may connect the receptor with the cortex and yet in the process each receptive element may be unambiguously related in the normal organism with a central spot and so receive an individual positional attribute. In its one aspect (longitudinal or 'musical') the anatomical area is a very long strip of receptive elements. In the other aspect (transverse or

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[1] G. F. Arps and O. Klemm, "Untersuchungen über die Lokalisation von Schallreizen. 1. Der Einfluss der Intensität auf die Tiefenlokalisation." *Psychol. Stud.* 1913, VIII. 226–70. O. Klemm, "2. Versuche mit einem monotischen Beobachter." *Ibid.* 497–505. O. Klemm, "3. Ueber den Anteil des beidohrigen Hörens." *Arch. d. ges. Psychol.* 1918, XXXVIII. 71–114.

[2] H. J. Watt, *The Psychology of Sound.* Cambridge, 1917.

[3] T. Thunberg, "Physiologie der Druck-, Temperatur-, und Schmerzempfindungen." *Nagel's Handbuch der Physiologie des Menschen.* 1905, III. 720. C. S. Myers, *Textbook of Experimental Psychology*, ch. XVII. Cambridge, 1911.

[4] M. v. Frey, "Beobachtungen an Hautflächen mit geschädigter Innervation." *Ztsch. f. Biol.* 1914, LXIII. 350 f.

'locational') it is very narrow. The binaural locational function is concerned primarily with the transverse aspect alone.

This consists generally of only some three to five terminals or, if we allow for their alternating arrangement, of perhaps only two or three terminals at any transverse section. Now in Bernstein's theory each terminal is assumed to give centrally a little area of excitation most intense at the centre and decreasing gradually outwards to a subliminal margin. To each such central excitation a spot of sensation corresponds which, as I take it, differs in its ordinal or positional attribute from the spot derived from a neighbouring terminal. This small transverse row of spots may be supposed to yield on full stimulation a single graded mound of excitation with a maximum at its centre.

For the purposes of binaural hearing my theory now assumes that these combined uniaural mounds, in so far as they are related to transverse sections of the longitudinal series of either ear at similar points, again combine by overlapping, namely when the two ears are stimulated simultaneously at these transverse points by any sound. The overlapping of course is not complete, but only partial as in Bernstein's familiar figure of single touch from double contacts. In my *Psychology of Sound*, I showed how the main binaural functions could be deduced from suitable variation of the relative intensities of the uniaural components and their summation. I shall therefore limit myself now to showing how Klemm's recent observations confirm and extend my theory.

Like Baley[1] Klemm finds that the binaural sound has a greater volume than the uniaural[2]. The resultant spot in touch is more extensive than the single one. Similarly the diotic sound is much louder than the monotic, four times as loud by estimation. This is more than is required by my theory, which would commonly yield duplication, if not rather more when we take into account the difficulty of judging accurately what is 'double.' Klemm's reckoning, however, lies on the safe side for me, for there may well be some mutual facilitation, psychological as well as physiological, between the ears. The two ears will bring into psychical prominence the best that either contains, as Dawson showed for vision[3]. Accordingly the diotic sound seems fuller and purer like an organ tone while the monotic seems thin, as many observers have stated. These relations hold for diotic tones, when the excitation in both ears is the same.

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[1] S. Baley, "Versuche über die Lokalisation beim dichotischen Hören." *Ztsch. f. Psychol.* 1915, LXX. 347–372.

[2] O. Klemm, "3. Ueber den Anteil des beidohrigen Hörens." *Arch. d. ges. Psychol.* 1918, XXXVIII. 72.

[3] S. Dawson, "Binocular and Uniocular Discriminations of Brightness." This *Journal*, 1913, VI. 78–108.

In dichotic hearing (when different sounds are led to either ear) similar relations should hold so far as the differences in the longitudinal aspects of the tones allow. But this will be complicated not only, as Baley found, by the possible distinction of these different monotic tones[1] but also by their overlapping in the central (diotic) part of the transverse field of hearing. This overlapping will naturally produce the same sort of hiding or involution of a higher tone by any lower (and even weaker) tone as is familiar monotically. This hiding I have explained in my theory of the longitudinal functions of the auditory field[2].

Klemm also finds that for the discrimination of monotic intensities a higher (but still comparable) difference is required than for the binaural function that is based on intensive difference. This seems to fall into the class of such facts as the greater displacement required for the apprehension of direction or for the distinction of end positions than for mere motion or for mere increase of extent, for the designation of direction of change generally than for mere change-at-all or for the first signs of the integrative product of that change. It is clear that for any and every comparison of monotic intensities the usual unity of the diotic effect must somehow be broken up, *e.g.* by partially or wholly successive presentation. For the combined diotic effect can only be read as "maximal intensity at a certain point of the transverse binaural series." Localisation involves no comparison of the components but is a resultant unity read as it stands.

It may be noted at this point that any ratio of intensities can be made to give an indefinite series of absolute resultants. The maximum of the resultant and its gradients may be raised or lowered without any displacement of its position in the transverse series. A basis for the discrimination of the distance of a sound from the head (or its 'depth') is thus provided. The important experimental analyses of Klemm shew that intensity plays an important part in the impression of the distance of sounds[3]. But it obviously cannot be the only factor, for the ratio of monotic intensities cannot remain constant for all positions on a line (drawn in any direction outwards from the centre of the head) that lies outside the median plane. The binaural ratio would therefore give a starting point lying on a base line ('between the ears'). The absolute intensity of the maximum as at this point would 'project' this base line far out from the head, whereby the binaural positions would now become a point lying in a certain direction from the head. One or more other factors, amongst which differences of blend of partials is commonly held to be the most important, would be required to distinguish between front and back, up and down, and between the ambiguities contained in the above two factors.

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[1] H. J. Watt, *The Psychology of Sound*, 183 f. Cambridge, 1917.

[2] *Ibid.* 71.

[3] *Cf.* G. F. Arps and O. Klemm, "Untersuchungen über die Lokalisation von Schallreizen. 1. Der Einfluss der Intensität auf die Tiefenlokalisation." *Psychol. Stud.* 1913, VIII. 247 f.: "The usual complex limen for distance S_e is dependent on the limen for intensity S_i' and the limen for pure distance S_e'. If S_i' were greater than S_e', S_e would

As regards the possibility of a pure distance factor other than blend of partials that Klemm's work[1] seems to imply, it may be suggested at this point that bone conduction might conceivably produce waves in the excitation of the cochlea in the path of its passage through that organ in its incoming directions. Only the excitation by bone conduction that falls into line with the normal stimulation of the ear by the small bones would produce the appreciation of tones in their full ordinary manner[2]. But this full excitation might possibly be criss-crossed in all directions by anomalous ripplets, as it were, that would produce a colouring of sound that we could learn to recognise but not perhaps to separate off analytically. A basis for the perception of direction of this kind would be available for purely monotic hearing, when the mind is forced to rely on the subtlest indications. Intracranial

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presumably coincide with S_e'; then intensity would have very little meaning at all for the appreciation of depth. If S_e' were incomparably greater than S_i', then again S_e would nearly coincide with the latter; and the impression of changed depth would have to be evoked when S_i' is reached, since intensity would now alone be responsible for the judgments of distance. Now in reality S_e' is notably greater than S_i'; S_e must accordingly lie between S_i' and S_e'. If the distance has changed only by the amount S_i', something is somehow still lacking for the pure impression of change of depth. On the other hand, the limen S_t indicates that with great enough change of intensity the impression of changed distance can be produced despite the resistance offered by the marks of depth that are associated with constancy of distance. The value of S_t will be the greater the smaller S_e' and the greater S_i; the marks of depth that come into operation in the limen S_e' seem to protect the apprehension of distance against the illusion (*Täuschung* = *t*) induced by the change of intensity.

And this psychologically reasonable relation we find confirmed in the figures, in fact in the various cases the values S_t are approximately equal to $S_i : S_e'$" (*cf. ibid.* 255 f.). It is clear, of course, that the judgment of distance on the basis of pure intensity is as open to monotic as to diotic observations (*cf. ibid.* 111).

[1] *Op. cit.* pp. 226–270.

[2] H. J. Watt, *The Psychology of Sound*, 168 f. Cambridge, 1917.

localisation could hardly be founded on this basis, however, as it appears not after learning, but suddenly and forcibly to the observer's surprise.

Nothing need be added to my previous statements regarding the correspondence between the capacities of binaural localisation and the variations made possible by the relative intensities of the overlapping mounds of the theory. As Klemm says[1]: "Certain as is the influence of intensity difference outside the sagittal plane, it is just as certain that by itself it can only procure distinction of the right and left hemisphere and a perception of some degree of departure from the sagittal plane, but not the apprehension of a particular direction." That is just the deduction I made from my theory.

But it may be noted that no abnormal alteration of the natural ratios of intensities that is permanent (*e.g.* owing to wax plugs in the ears or to partial deafness) will noticeably impair the binaural function for ordinary use. We learn to correlate the position on the transverse series that is actually caused by locus in the median plane with our other positional experiences (*e.g.* visual, motor, ideational) relating to that plane, and we make our auditory space correspondingly. Thus in a relatively monotic observer the median plane would not be displaced towards the worse ear as in the case of artificial or temporary deafness on one side[2]. The ratio of intensities actually emanating from the median plane, though very far removed from equality, would be correlated with the median plane as otherwise given. And so the deafened observer would in course of time learn to localise with considerable accuracy. But the result would be subject to all the individual variations and anomalies of such acquired powers.

This is merely a corollary to my thesis that bi-systemic functions such as the binaural and binocular are still only ordinal or concerned with form, and not spatial. But the binaural position is inherently positional from the first, whereas the others (absolute intensity of binaural maximum and blend of partials) can only become signs of positions by being correlated with positions otherwise given.

Very strong support is given to my theory by Klemm's experiments on the variation of the time relations of the two components[3].

(1) "With the smallest time differences the sound stayed in the middle, but as compared with the effect of simultaneity it was broadened

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[1] G. F. Arps and O. Klemm, "Untersuchungen über die Lokalisation von Schallreizen. 1. Der Einfluss der Intensität auf die Tiefenlokalisation." *Psychol. Stud.* 1913, VIII. 254.

[2] O. Klemm, "2. Versuche mit einem monotischen Beobachter." *Ibid.* 501.

[3] O. Klemm, "3. Ueber den Anteil des beidohrigen Hörens." *Arch. d. ges. Psychol.* 1918, XXXVIII. 106 ff.

in a peculiar way. The sound remained unitary and this extension seemed to make evenly for both sides; it no longer lay so neat and small in the median plane as in the case of simultaneity." This change was the only sign of succession to be noticed, whether the left or right ear came first or not.

(2) "With the next step the broadening of the sound was attended by a displacement from the median plane towards the side of the earlier ear." The sound was still unitary, but the earlier ear could now be distinguished by the observer in the shape of this displacement of position, not as a time phenomenon strictly.

(3) Finally: "The unitary sound broke up and the time difference came to consciousness. At first, however, there was an apparent movement from the earlier to the later ear,"—a sort of cinemato-acoustic illusion. The sound seemed to rush through the median plane[1]. This cinematic illusion yielded gradually to the impression of a pure double beat.

A clearer confirmation of theory than these notes provide could hardly be desired. We have here practically a parallel to the steps from one resultant touch to two distinct touches noted by Foucault[2]. The resemblance to the results of studies in visual and tactual movement is also obvious. The earlier ear gives the maximum at first to one side while the later ear pulls it over to the other. As there is a limit to motional rate, the smallest time-difference must at first give only a spread of the maximum and so a broadening. The excitation rises with a slight hump towards the side of the earlier ear. As the time difference is increased this hump will pull further out towards the earlier ear until it is at it, so to speak. Then the component for the other ear will pull the maximal point over towards its side, the farther the greater be the time-difference; for the second excitation will benefit by the facilitation afforded by the earlier stimulation. At a certain rate this will give optimal motion, while with greater time-difference there will be increasing time for the earlier ear to make its position felt as such and to be followed by the second as a distinct beat. Here again we see how motion implies differences of position though not the distinction of these positions as such. The times for these first stages until the greater part of the motion is lost should lie well within the time of auditory excitation increase. Thus we can understand how the third stage was found by Klemm to begin after 5 σ. It is just such results as these that allow us to estimate the time required for optimal interaural motion. When the motion is not so extensive as this but is to cover only a small arc of the horizontal circle round the head, as in Burtt's experiments where the successive positions of the sound were "a few centimetres apart"[1], longer time-intervals would be required. Burtt found the optimal interval to vary from some 10 to 50 σ with an average of 25 to 30 σ. So his results help to provide "evidence for the correlation of separate points in auditory space with separate regions of the sensory cortex" (p. 74). Thus we should expect to find the now fairly general laws of cinematic motion verified in the transverse aspect of hearing. So the total duration of the stimulus should not be long; for the earlier cessation of the first stimulus will allow it freer drainage into the later one. And if the stimuli are too intense, the increase of excitation will be too rapid. But a later intenser stimulus should be able to reverse the illusory movement, etc.

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[1] A. H. Pierce, *Studies in Auditory and Visual Space Perception*, 66. New York, 1901.

[2] M. Foucault, *L'année psychol.* 1911, XVII. 420 ff.

As for Weber's experiment with the two watches to which Klemm alludes[2], the difference noted between monotic and dichotic hearing does not prove that the central connexions of the two ears remain independent. The two watches are heard to coincide and separate rhythmically in monotic hearing because their sounds equally fill the monotic transverse volume. In dichotic hearing either watch can be heard separately by special direction of the attention, because then the combined field of hearing contains a central diotic part with a monotic part (beyond the overlap) on either side. Attention to one ear can fix on the corresponding monotic part and so get a basis for the adjustment of the attention to all the details of the presentation in question. This is quite in line with Baley's observations which I discussed in my *Psychology of Sound* (183 f.). We can see a basis here also for a sort of transverse timbre or blend of partials that could be correlated (without analysis) by 'experience' with positions given through other channels than hearing.

As for the pitch differences between the two ears I have nothing to add to my previous interpretation of them, which still seems to me as direct and satisfactory as one could expect. We must apparently consider these differences as the remainders left by the progressive approximation to perfect inter-adjustment and cooperation of the two ears, certainly not as anomalous departures from a perfection that is presupposed as primary. Klemm's (limited) statistics suggest a normal distribution about a maximal frequency of no difference at all. The imperfection of that elasticity of the basilar membrane or similar structure which my theory of the longitudinal functions of the auditory field rests upon, is not an unnatural or alien hypothesis. It is merely a corollary to any assumption regarding elasticity. Theories which treat pitch as qualitative must first leave the qualitative identity of the two ears (like pitch-quality in general) quite unexplained and must then assume that to some extent the qualities of the two ears are not identical. There is, moreover, no reason why minute pitch differences should make us hear double. That should only come about when the difference is great at any point and extends over many points. When one ear is badly mistuned (by disturbances in the middle ear perhaps), double hearing can be very distinct indeed, as I have myself experienced it.

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[1] H. E. Burtt, "Auditory Illusions of Movement—A Preliminary Study." *J. of Exp. Psychol.* 1917, II. 63–75.

[2] G. F. Arps and O. Klemm, "Untersuchungen über die Lokalisation von Schallreizen 1. Der Einfluss der Intensität auf die Tiefenlokalisation." *Psychol. Stud.* 1913, VIII. 81.

Klemm's experiments on the artificial inversion of the intensity difference[1] belong to the problem, not of the binaural function strictly (binaural position), but to that of binaural space. He himself suggests this by his comparison of them with the familiar experiments of Stratton on the inversion of visual space by the wearing of special glasses. To some extent his experiments demonstrate the presence of alternative effects of attention similar to those demonstrated by Wertheimer and others with visual motion. This implies that under certain circumstances, limited in scope no doubt, the attention is able to alter the sensory datum. How far in the case of hearing we have a real alteration, as in the accents of rhythm, or an apparent alteration due to the inertia of the recognition of alternation, or merely a change of correlation (which the very limited scope of absolutely primary intensity localisation suggests) is not quite clear. Klemm himself puts it down to the spatial misdirection of the complex of associative connexions[2], not to a change in the actual basis of auditory localisation. But, anyhow, these changes will be as real as in rhythm, illusions, motions, etc. And their basis will doubtless be found in an essential kinship of the attentional attitude with, or its ultimate origin out of, the sensory basis such as is altered by it. Such alterations by the attention do not of course imply any permanent obscuration of the primary basis of localisation. For unprejudiced observation can readily get itself into line with the latter again, as the experiments show. Some observers may not exhibit the illusion at all[3]. And any novelties or irregularities in the impressions dispel the distorting effect of attention at once.

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[1] G. F. Arps and O. Klemm, "Untersuchungen über die Lokalisation von Schallreizen. 1. Der Einfluss der Intensität auf die Tiefenlokalisation." *Psychol. Stud.* 1913, VIII. 98 ff.

[2] *Cf. ibid.* 104.

[3] *Cf. ibid.* 105.

Numerous details in the functions of binaural hearing have still to be described exactly and explained. (One of the most surprising of these perhaps is the intracranial[1] localisation.) But I think my theory meets the known details of experimental investigations both intensively and extensively enough to be considered a probable theory of this otherwise (and as even Klemm leaves it) perfectly mysterious and unintelligible bi-systemic function. It has the merit, if I may say so, of being an extremely simple theory from which the whole range of facts can be easily deduced.

At the same time it adds considerable strength to my explanation of the other functions of hearing. The longitudinal aspect of the auditory field might conceivably be associated with a system of irreducible qualities, while the transverse yielded a series of positions. But that is hardly probable, one may surmise. Why two so different affiliations? Besides is not any systematic theory that adequately covers the main trend of facts now known and satisfies the general principles of psycho-physical parallelism, so giving free scope to the spatio-temporal theories of physiology,—is not such a theory preferable to none or to such as deal in vague assumptions regarding qualities or motor associations or the like that are incapable of analysis and complication and so offer no prospect of progress? If the value of theories is either essentially pragmatic or systematic, the answer can hardly be other than assent. The kinship of the longitudinal and transverse differences, moreover, is strongly supported by the fact that we can only localise differently pitched sounds in different places at once. Identically pitched sounds compound to a phantom sound whose apparent position depends upon the resultant intensity. There is of course no proof to be drawn from these relations, but the unity and clarity of explanation could hardly be said to lie with the qualitative theory of pitch. At the same time we see that as the musical dimension is abstracted from the locational so that tones seem to have only that volume which varies in them, namely length without breadth, so the transverse aspect is abstracted from the musical, so that the volume of a tone is never read as a length in space[2], nor as spatial, while the transverse position is. Both sets of positions, longitudinal and transverse, begin and persist as positions, but the transverse enters into integrations that make its positions spatial.

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[1] As such of course, not in its internal differences, which form a replica of the extra-cranial (*cf. ibid.* 88).

[2] *Cf.* Burtt, *op. cit.* 190.

PUBLICATIONS RECENTLY RECEIVED.

Psychology and Folklore. By Dr R. R. Marett. London: Methuen & Co. Ltd. pp. ix + 272. 7*s*. 6*d*. net.

The author has collected in this book a number of his recent addresses, lectures and reviews, having enough in common (for the most part) to give them an effective unity. In nearly every article he dwells on the importance of Psychology, to the understanding of the social phenomena of Anthropology: a point at which, probably, most readers of this *Journal* will agree with him. Some instances of interesting psychological insight will be found at p. 137—on the emotions aroused by crossing a threshold; at p. 139—on the blood-covenant; and at pp. 216–7—on the operation of suggestion.

Generally Mr Marett complains of the prevalence of a too intellectualist method of interpreting savage life, as (for example) in Sir James Frazer's writings, and prefers the more emotional appeal of M. Durkheim (183). Undoubtedly the importance of emotional conditions in determining savage beliefs and institutions cannot be overrated; and savage emotions can in some sort (though perhaps very imperfectly) be appreciated by direct sympathy. Sympathy is necessary to one's own appreciation of the case—there is no other means; and Mr Marett has tried earnestly to enter into the mind and motives of the lowest culture. But when it comes to public explanation, the attempt to describe emotional conditions intellectualises them: explanation can only be intellectual. Our only way of conveying to others a lively sense of the emotions of a savage, and of how they work, is by exhibiting the ideas and practices which excite, or which are exited by them.

A question which brings this opposition to an issue is the definition of 'religion.' For Mr Marett religion is essentially an affair of emotion: it is the sense of 'sacredness' (158) interfused with certain social practices or institutions; for example (according to M. Durkheim) with Australian totemism (188). Sir James Frazer, on the other hand, defines religion as consisting of two elements—"a belief in powers higher than man and an attempt to propitiate or please them": which excludes totemism. But this is intellectualist: for such a definition decides whether an institution is religious or not "by the presence or absence of a certain concept" (188). In the passage quoted (182) from *The Golden Bough*, it is expressly said, however, that "unless the belief [in powers higher than man] leads to a corresponding practice, it is not a religion but merely a theology." The corresponding practice, again, is described as "an attempt to propitiate or please." the higher powers; and obviously such practices imply corresponding emotions—at least in their origin and in those who carry them out with sincere unction. Is it necessary to insist upon it?

There is a sort of *a priori* proof that the Australians are (or must be called) religious: "For the anthropologist, religion is a universal attribute of man, because his historico-scientific purposes require him to have a free hand in the following up of origins right back to the point where the direct evidence of history breaks off" (157). So, if you do not call totemism 'religion,' you are no anthropologist. But is there no beginning of anything in the history of culture; or may not that which Sir James Frazer calls 'religion' have originated at a later stage of culture than that of Australian totemism, not indeed *ex nihilo*, but by a synthesis of elements already existing, namely, a belief in spirits, funerary rites, and (no doubt) the sense of sacredness? At a controversy as to what should be called 'religion,' one soon feels tired; and it might be wise to disuse the word in Anthropology.

Recognising the profound influence of emotion on such observances as those involved in totemism, including the *Intichiuma* ceremonies, how shall the student convey to others the impression they have made upon himself? Shall he chiefly rely upon the describing of what happens, in the hope that the reader's sympathies will supply the rest; or shall he try to bring it home to the civilised bosom by using the language appropriate to what may be considered as parallel social phenomena amongst ourselves? The latter course (which is Mr Marett's) seems to imply that the state of mind amongst Australians during the *Intichiuma* ceremonies is much the same as that of cultivated Europeans during celebration of the Mass. In seeking to secure such ends as are socially approved, we are told, "the postulate of magic, in this sense, and of religion would seem to be the same; namely, that there is a power conferred by holiness of life which, through the mediation of holy men, can be made to abound for the common good" (206). Such a mode of expression (characteristic of the school of Durkheim) it would be very unjust to treat as a misrepresentation. Not false, it is unduly heightened by a generous desire to vindicate the savage against the inconsiderate undervaluation of obtuse observers. But less heightened it would be more effective. Probably there has been an unbroken evolution of the feelings involved in what we call the 'sacred' or 'holy' from the stage of Australian totemism to that of modern Christianity; but there has been an evolution; and those students seem to be justified who hold that such terms at the savage level may most fairly be rendered by our 'dangerous.'

Similarly, when it is suggested that a medicine-man's will 'for power' may be conceived as "a will to exert power over self, whereby a spiritual experience, indirectly involving a disposition towards a life of social service, may be attained for its own sake" (63)—what are we to say? This is an ideal, to which rare individuals may perhaps approximate. And thus we have indicated to us a good reason for not attempting to say much about the emotional side of savage institutions, namely, that the emotions are so variable, not only in the course of ages, but in individuals in the same age; nay, in the same man on different occasions of the same rite. What savages do may be faithfully described; what they think can be approximately translated for us by those who know their language; but what they feel we cannot be told; for two men may perform the same rite with very different feelings. And, indeed, to know what either of them feels may be difficult, or impossible, for the best observer. Many students must have tried to put themselves in the place of the *Naturmensch.* Is the result ever satisfactory? Does there not always remain a surd of impulse and emotion too obscure for words?

There are many other matters in this book which it would be profitable to discuss if space permitted, such as the judicious treatment of war and savagery in chapter II, and of the anthropological methods of Tylor, Gomme and Rivers in chapter IV. It is all earnestly thought and admirably written.

An Introduction to Sociology for Social Workers and General Readers. By Professor J. J. FINDLAY. London: Longmans, Green & Co. 1920. pp. xi + 304. 6*s*. net.

It is scarcely necessary to say that the reader will find in this book much that will stimulate and suggest, and that many will be induced to turn not only to the deepest sources of learning but to the world of experience with a clearer outlook and a firmer determination to get to the basic principles of social life. The book is divided into three sections dealing respectively with principles, types and organization. Of these the student will probably find the last two the more helpful; but the conclusions as to fundamental principles which are to be found in the first section should stimulate him to thought, despite the fact that he will probably wish that Professor Findlay had in some cases given a fuller exposition of the evidence.

It is somewhat startling at the present day, when many are convinced that we are suffering from the existence of too many committees, to be told that "we shall employ not less committees but more," that "every citizen must be at times inspector and manager" (p. 30), and that "the new principle of progress is concerned with numbers, with the size of the unit" (p. 30). One's thoughts turn to the view of Graham Wallas that members of committees are being reduced to the position of obedient shadows or of intriguers for small concessions from the holders of real power.

In the fundamental problem of the relation of the member to the group, the author's treatment illustrates, we believe, the transition from a naïve individualism towards a more adequate recognition of the social nature of man. Thus in the earlier pages of the first section we find such a passage as "uniformity of reaction to identical stimuli is the comprehensive factor which enables us to identify any individual as being a member of a group" and "to declare that the total is a social unit and not a fortuitous concourse" (p. 11). Later we find "This union is not an accretion of uniform particles attracted to one spot by identity of composition" (p. 103) and "they unite...because...they seek amid diversity to achieve a common purpose" (p. 109). No doubt these later quotations represent the view towards which the author is attracted, but the novice may be hindered in his progress towards it by the earlier passages we have quoted from the book, and he may require more guidance than Professor Findlay gives as to the ambiguity lurking in such a phrase as "common purpose."

Individual Differences in Output in the Cotton Industry. (Textile Series, No. 1.) By S. Wyatt. Report No. 7 of the Industrial Fatigue Research Board. London: H.M. Stationery Office. 1920. pp. iii + 13. 6*d*. net.

This highly suggestive preliminary report shows that the various processes in the cotton trade can be classified according to the relative predominance of the human and mechanical factors and according to the consequent range of the variations of individual workers in output. The human factor proves to be of the greatest importance in weaving and winding and "it is in these processes, rather than in spinning, that efficiency may be increased and fatigue reduced by the elimination of waste in method and movement." The writer points out that the magnitude of individual differences in any single occupation may also depend on the amount of skill involved, and that a scientific basis for wage payments and duration of apprenticeship would result from a classification of processes according to the amount of skill involved and other conditions of employment.

The Psychology of Persuasion. By William Macpherson. London: Methuen & Co. 1920. pp. 256. 6*s*. net.

This is a popularly written book, of no great psychological importance, interesting to the psychologist mainly for the illustrations with which it is enlivened, drawn from the fields of oratory, advertisement, salesmanship, cinematography, and art.

Mental Tests. By Dr Philip Boswood Ballard. London: Hodder and Stoughton. 1920. pp. ix + 235. 6*s*. net.

Dr Ballard has written this little book primarily for the teacher. Some of the chapters have already appeared in a slightly different form in the *Times* Educational Supplement and in the *Journal of Experimental Pedagogy*; two others are based on papers read before the Education Section of the British Psychological Society and the Educational Handwork Association. Chapter IV gives a useful translation, made by Mr Cyril Burt in consultation with Dr Simon, of Binet's well-known tests; chapters V and IX describe Mr Burt's graded series of reasoning and spelling tests. The book is very brightly written and will prove of great service to the teacher. Few are better qualified to write on the subject from the educational standpoint than Dr Ballard.

An Introduction to the Study of Hypnotism: Experimental and Therapeutic. By Dr H. E. Wingfield. London: Baillière, Tindall and Cox. Second Edition. 1920. pp. vi + 195. 7*s*. 6*d*. net.

Dr Wingfield is one of the sanest, most expert and experienced of British hypnotists. The first edition of his book was exhausted within a short period after its publication many years ago. This new edition includes an account of the treatment of repressed memories by hypnotism; in other respects the book is substantially unchanged. For those who seek a reliable account of the methods of inducing hypnosis, of the phenomena of hypnotism and of treatment by hypnotism, no better book of its size can be recommended.

Quelques Traits de la Vie de Jésus: au Point de Vue Psychologique et Psychanalytique. Par Dr GEORGES BERGUER. Genève et Paris: Atar. 1920. pp. cviii + 267. Fr. 15.

This is a series of lectures delivered at the University of Geneva in 1917 and 1919. "There is a supreme interest," says the author, "in returning to the primitive documents we possess concerning Christ, and in bringing to bear on this grand Figure all the light of the modern sciences. Research in this field must evidently be accompanied with considerable prudence and with profound humility." The last sentence gives the keynote to the book. From the psychological point of view its results are extremely disappointing. The author's psychoanalytic efforts are feeble, being based on inadequate qualification for the task. His comparisons of Christianity with other mystic religions are superficial. Fear of the *odium theologicum* seems to inhibit the author from start to finish.

Beauty and the Beast: an Essay in Evolutionary Aesthetic. By S. A. MCDOWALL. Cambridge: University Press. 1920. pp. 93. 7*s.* 6*d.* net.

The basis of this book is Croce's theory of the Aesthetic. From Croce, however, the author dissents notably in the former's "rejection of a metaphysic and his denial of a God." In the Introduction he informs us how in attempting to form a philosophy of religion, he had "tried to find in Love, which is the very nature of Godhead, an essential impulse towards creation," how there he found that "something vital had clearly been left out" and finally that "in Beauty I seemed to find what I had missed." The essay is interesting but contains little of psychological importance.

Lectures on Industrial Administration. Edited by B. MUSCIO. London: Sir Isaac Pitman & Sons. 1920. pp. viii + 276. 6*s.* net.

In July 1919, a 'school' for the study of certain problems connected with industrial management was held in Cambridge. A selection of the lectures there delivered, by Professors Sorley, Pear, Cullis and Collis, Messrs Bowie, Wrong, Stelling, Burt, Wyatt, Myers, Fleming and Sargant Florence, is published in the present volume. The school was so largely attended and it attracted so much interest that the publication of these lectures is likely to satisfy a widely felt need.

Mental Fatigue. By Dr G. E. PHILLIPS. No. 40 of the Records of the Education Society, Teachers' College. Sydney: W. A. Gullick. 1920. pp. 103. 7*s.* 6*d.*

This is a thesis prepared, under the direction of Prof. Spearman, at University College, London, for the London University doctorate in Science. It contains an interesting historical survey of recent work on fatigue and a detailed account of the writer's own experiments which deal with the effects of different lengths of rest pauses, with the transfer of fatigue from one kind of mental work to another, and with the form of the work curve during very brief periods of work. The concluding chapter is devoted to a theory of fatigue based on Lehmann's (here consistently spelt 'Lehman's') application of certain chemical laws to muscular output. The author deduces from his experiments that "fatigue is a general phenomenon, and is not specific to the task being performed," that although it is produced 'generally' it is transferred 'specifically,' and that in work periods of 1 or 2 minutes the work curve has a constant shape "independent alike of the task and of the subject who does the task."

Human Efficiency and Levels of Intelligence. By H. H. GODDARD. Princeton: University Press. 1920. pp. vii + 128. 6*s.* 6*d.* net.

The author publishes a course of four lectures delivered in Princeton University during April 1919. His contention is "that the number of people of relatively low intelligence is vastly greater than is generally appreciated and that this mass of low level intelligence is an enormous menace to democracy unless it is recognised and properly treated." He fears the rapid increase of inefficiency and delinquency "through the natural propagation of hereditary feeble-mindedness," he urges the segregation of the feeble-minded from childhood onwards, and recommends the evaluation of the degree of intelligence needed for different occupations, to which only those should be admitted who have the necessary intelligence.

Lehrbuch der Experimentellen Psychologie. Von Prof. J. FRÖBES S.J. Zweiter (Schluss-) Band. Freiberg im Bresgau: Herder & Co. 1920. pp. xx + 704. £1. 6*s.*: geb. £1. 10*s.*

This book maintains all the faults and virtues of its companion volume. It deals with speech, attention, memory, intelligence, emotion, will, personality, sleep, hypnosis, mental disorders, etc. A wonderful scrap book of opinions, culled from the writings of hundreds of investigators, good, bad and indifferent, it is a monumental work of Teutonic diligence, but wholly devoid of *Geist.*

Intelligence and Social Valuation: a practical method for the diagnosis of mental deficiency and other forms of social inefficiency. By Dr R. A. BERRY and S. D. PORTEUS. New Jersey: Vineland Training School. 1920. pp. 100. $1.50 post paid.

The graded series of Porteus maze-tests, it is claimed, "represent an attempt to evaluate socially valuable characteristics not fully tested by the Binet...." The authors propose that "cases should be regarded as 'potential social inefficients' if the average of the Binet-Porteus quotients fall below 75." They investigate the relation of brain capacity to mental development, and discuss the correlation of various physical and mental measurements. The Report includes a series of tables based on a large number of measurements which cover a very wide field of research.

The Mystery of Life as Interpreted by Science. By R. D. TAYLOR. London and Felling-on-Tyne: Walter Scott Publishing Co. 1919. pp. 176. 3*s.* 6*d.*

A National System of Education: some recommendations for establishing it in England during the decade ending ten years hence. Published for the Federal Council of Lancashire and Cheshire Teachers' Associations: Manchester University Press. London: Longmans, Green & Co. 1920. pp. 78. 1*s.* net.

La Philosophie d'E. Boutroux. Par Dr. A.-P. LA FONTAINE. Paris: J. Vrin. 1920. pp. iii + 89. Fr. 3.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

GENERAL MEETINGS.

June 19, 1920. The Emotional Phases of Affective Experience, by James Drever.

July 10, 1920. Complex and Sentiment, by W. H. R. Rivers.

Habit Formation in Guinea Pigs, by E. M. Smith.

Demonstrations of (*a*) Apparatus for testing Psycho-galvanic Reaction, by E. Prideaux; (*b*) Some New Tests of Auditory Sensation, by H. Hartridge; (*c*) A Method of experimenting on the Relation of Affection and Memory, by W. Whately Smith; (*d*) Some Printing Trade Tests, by B. Muscio.

SECTIONAL MEETINGS.

(*a*) Education Section.

June 9, 1920. Equal Additions *versus* Decomposition in Teaching Subtraction: an Experimental Research, by W. H. Winch.

(*b*) Industrial Section.

June 19, 1920. Fluctuations in Mental Efficiency, by B. Muscio.

July 19, 1920. The Problem of Incentives and Output, by Ordway Tead.

(*c*) Medical Section.

June 23, 1920. An Outline of the Idea of Rebirth in Dreams, by Maurice Nicoll.

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DISORDERS OF SYMBOLIC THINKING AND EXPRESSION[1]

BY HENRY HEAD.

1. *Introduction* (pp. 179–181).
2. *These disorders of speech are not due to destruction of 'images'* (pp. 181–184).
3. *The functions which are disturbed in aphasia and kindred disorders* (pp. 185–192).
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    - (*c*) *Nominal defects* (p. 187).
    - (*d*) *Semantic defects* (p. 188).
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1. INTRODUCTION.

THE power to make use of words was acquired at a late stage in the evolutionary history of man. Before he learnt to formulate his thoughts in speech, he was capable of a wide range of accurate discrimination, which could express itself in action only. Progressive development of the higher cerebral centres led to increased discriminative powers; and speech, one of the highest and most recently acquired functions, was from the first concerned with expressing relations.

In animals, cries uttered during states of pleasure, fear or anger, are capable of exciting similar passions in hearers of the same species, and specific sounds may form an essential link in what Pavlov has called 'conditioned reflexes.' 'Clucking' is as effective a call to the chickens as rattling the manger to a horse; both have become definite indications of the presence of food.

Man enormously widened this association between certain sounds and consequent actions, and was finally able to express the relation of some external object to himself in the form of a proposition. He not only called the white substance, which covered the ground in winter, "snow," but formulated the sensory impression it evoked in the phrase "snow is cold." In this way he became capable of manipulating words in thought. Not only could he express the relation of external objects to himself and register his impressions, but he was able to convey them to others.

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[1] Presented at the Oxford Philosophical Conference, 25 September, 1920. For a description of the tests and many of the clinical facts on which the conclusions expressed in this paper are based, see the writer's "Aphasia and Kindred Disorders of Speech," *Brain*, 1920, XLIII, 87–165.

But there always remained elements in thought which were not associated with words. Speech is a discriminative movement capable of fine degrees of adjustment; it is essentially an intellectual mechanism. The more nearly the mental state approaches pure feeling, the less easily can it be expressed in words alone, apart from gesture and the tone of the voice. Even in the gravest cases of aphasia the patient is evidently fully aware of his emotions, and can express them clearly in gesture and action. Under the influence of emotion he may be able to say 'yes' and 'no,' and even phrases such as 'Oh! dear me,' 'I know it, I can't tell,' though he is entirely unable to evoke the words voluntarily or to command. In order that words can subserve intellectual activity, they must be mobile and capable of manipulation at will.

At first sight it might seem difficult to understand how gross destruction of the brain could produce loss of so complicated an activity as speech, without reducing the patient to a state of dementia. But we are familiar with the high-grade loss of sensation due to injuries of the cerebral cortex; the qualitative and affective aspects are not materially diminished, whilst the power of estimating the spacial relations, graduated intensity and similarity or difference of external stimuli may be gravely disturbed. All these changes may occur without materially reducing the patient's intellectual capacity, apart from his power of appreciating sensory relations. Speech is a more complex psychical aptitude; but it can be disturbed without of necessity producing grave intellectual defect, apart from the loss of those functions associated with the activities which underlie the perfect use of language.

The fact that speech can be disordered by injury to the brain is one of the most wonderful means placed in our hands for investigating the relation of mind and body. But, unfortunately, all the elaborate and careful work expended on this fascinating problem has been vitiated by the conception that speech was a well-defined intellectual function, which was strictly localised in some particular site in the brain. Attention was concentrated on correlating the site and extent of the anatomical destruction with the character of the disorder of speech. Such efforts were bound to fail, because the middle or physiological term of the problem was neglected. The utmost skill and care expended on determining the nature and extent of the anatomical lesion will not reveal, except in the roughest manner, the form assumed by the loss of speech during life. Within certain limits the severity of the lesion and the rapidity of its onset are more important than its anatomical extent. A small limited destruction of substance arising acutely may produce a more profound and widespread loss of function than one of wider extent but gradual incidence. A lesion within an area on the surface of the brain, associated with the mechanism of speech, may produce in one case 'total aphasia,' in another 'partial aphasia' ('motor' or 'sensory'), and, in a third instance, be accompanied by no obvious disorder of language. At the root of most of the work on 'aphasia' lies the fundamental error of ignoring the physiological changes intervening between the anatomical lesion and the psychical states with which it is associated.

An even more disastrous fallacy consists in attempting to correlate anatomical changes categorically with metaphysical conceptions of the forms assumed by psychical activity. Destruction of the substance of the brain can only disturb the act of speech, because it interferes with those physiological processes necessary for its perfect execution. Now, there is no reason to suppose that any groups of cerebral functions correspond to the 'memory of words,' 'reading' or 'writing,' or even to 'speech' itself. Any lesion, which gravely disturbs 'speech,' affects activities which are not usually classed with the use of language; and the more carefully the patient is examined, the less certainly does his disorder correspond to any preconceived category.

2. THESE DISORDERS OF SPEECH ARE NOT DUE TO DESTRUCTION OF 'IMAGES.'

As soon as it was evident that speech could be affected by an injury to the brain, observers began to seek for some mechanical explanation of this indisputable fact. Gall had already given the frontal lobes as the seat of memory, which he divided into six different varieties; there was the memory of things, the memory of locality, name-memory, verbal and grammatical memory, and that for numbers. It was an easy step to suppose that such 'memory' was based upon 'images' which were 'stored up' in the cortex. These were destroyed by the organic injury, and speech suffered from a want of specific memories. Bastian, in 1869, started from the *a priori* assumption that we think in words, and that words are revived in the cerebral hemispheres as remembered sounds. He was the first to introduce the conception of 'word-deafness' as the cause of defects of language, a theory subsequently developed more fully by Wernicke. Under the influence of Munk's extirpation experiments, 'mind blindness' crept into the work on aphasia. Finally, a general idea grew up that most of these high grade disorders of speech could be explained by loss of auditory and visual images; the remainder were due to defective motor activity.

The existence of speech in persons devoid of auditory or visual imagery, and the wide diversity in the nature of the normal power to form images, would render any such theory of the mechanism of speech improbable. Images play a part in the use of words and numbers, but the extent to which they are used varies greatly; they are not a necessary and fundamental factor in speech.

Moreover, if the lesion remains strictly unilateral, the phenomena are not those which would accompany 'mind blindness' or 'mind deafness.' For a patient who suffers from 'mind blindness' is unable to recognise the significance of an object, so long as he employs sight alone, although he can do so if allowed to use his other senses. In the same way, 'mind deafness' postulates inability to appreciate sounds, and particularly spoken words, with complete comprehension of the same words presented in print. In uncomplicated cases of aphasia, no such specific difference occurs; the loss of function affects words or their meaning, whether they are heard or seen. On the other hand, with certain bilateral lesions of the brain, images may suffer, either alone or in association with true aphasic manifestations.

The forms of imagery usually found amongst an English-born population is mainly visual, and the majority of my patients possessed strong powers of visual memory. I therefore included the following tests in my systematic examination. The patient drew some object from a model in front of him, and after its removal was told to reproduce it from memory. This is no necessary evidence of visual imagery; but as he drew from memory, he was asked to describe the colour, shape and pattern as far as possible. He then attempted to draw a plan of some room with which he was familiar, such as his ward in the hospital. If this presented any difficulty, I marked on a sheet of paper an oblong to represent the bed, and asked him to indicate on the paper the situation of the surrounding objects. As a further test he was asked to describe the way along some familiar route either within or outside the hospital.

Throughout these tests most of my patients laid stress on the fulness of their inner vision; the difficulty lay in formulating their conceptions. No. 5, when examined in another part of the hospital, drew a plan of the beds in his ward incredibly badly; walls, windows, and even the beds themselves were wrongly placed. But he insisted, "I can see all in my mind but I can't put it down; I've got it all in a picture in front of me," waving his hand before his eyes. He described accurately the colour of the curtains and the red covers of the beds; nor had he the least difficulty in recalling the uniform, both of nurses and sisters. He could indicate the position of the objects round his bed by pointing, but could not transfer their relative situation to a plan. Another patient with severe aphasia was shown a jug, and after its removal drew the outline correctly from memory, and added that it was covered with red and green patterns.

No. 8, who had a considerable domestic reputation as a draftsman before he was wounded, tried to draw a landscape from memory. He explained his complete failure as follows, "I want the thing in front of me. I don't know how to build it up. If I had the view in front of me, I could make you a decent sketch. I can see it in my mind but I can't put it down."

Another intelligent patient, who could not comprehend with certainty the meaning of words or figures, gave me the following example of her difficulties. "People ask me what 'bus are you going by, and I don't know. A number conveys nothing to me. But when I see a 'bus with Barking on it, I see in my mind Barking Park, and then I know that's where I want to go."

One of the ablest of my young officers tried to tell me how he would go from the hospital in which he was lying to the War Office. "Suppose I was going from the Army and Navy, that's what it's called, just round here (Army and Navy Stores), I should then say some place to a hospital about a quarter of a mile away. I remember the hospital is on the left on the way to the War Office, about half-way. I believe it's the Abbey. No it's near the Abbey, on the way to the War Office." Later he added, "I saw the hospital." I put as a question "The Abbey?" and he replied as follows. "It's here and then it's gone again, and I have to feel for it again. The only thing I can remember is the opening, the big opening where everybody goes in. I can get that" (moving his hand in the form of a large arch). Since it was obvious that he had described Westminster Hospital and Westminster Abbey, I asked "Have the Hospital and Abbey anything to do with one another?" He answered "Nothing except my focus, the place of them, the distance, that is all. You see, it's like this; with me it's all in bits. I have to jump like this," marking a thick line between two points with a pencil. "Like a man who jumps from one thing to the next. I can see them but I can't express. Really it is that I haven't enough names."

He repeatedly asserted that he could 'see,' but could not express what he saw. This was exemplified when he was asked to read to himself the following account of a famous prize fight, a few days after its occurrence. "Carpentier, the French champion, met Beckett, heavy-weight champion of England on Thursday, December 4th, at the Holborn Stadium. Beckett was knocked out in 74 seconds by a right hand hook to the jaw." At the end of six minutes he returned the paper and started to tell me what he had read in the following words. "Carpentier met Beckett in the Holborn—say—so—Sadium, I used to know it, and Carpentier being the champion now—who is—oh no—who met Beckett—now the champion in England—knocked—er—knocked—er—Beckett out in 74 seconds—with a right—with a right hook to the jaw." He then added "I wasn't thinking so much of the fight—but what I had been reading—I saw the picture of 74—seconds—saw what I had read."

I then said "Can you see the fight?" and an extraordinary change came over his narration. "Now I think about the fight I see the picture of Carpentier. I see Beckett falling—I see the Ref—Ref—what you call it—Referee—. The last thing I can picture is Carpentier standing like this with both hands like this." He sprang from his bed and assumed an attitude with both hands on his hips looking down towards the ground, in the position of Carpentier in the photographs of the termination of the fight. The change in manner from the puzzled aspect of a man slowly recalling what he had read to the brisk, vivid, smiling person describing what he saw was remarkable.

Auditory images, stored up in certain areas of the cortex, were supposed to be responsible for the 'memory of words,' and normal speech necessitated the revival of these sounds in the mind; aphasia followed as a natural consequence, when these images were destroyed by an organic lesion. But this hypothesis is not sufficient to account for the clinical facts. For a patient may be unable to recall the names of a series of colours, though he can describe them in metaphorical terms; here it is not 'auditory images' of words that are lacking, but power to find the appropriate name. On the other hand, he may be able without difficulty to select a printed card bearing the name of a colour shown to him, although he can scarcely utter or write a single word; it is the capacity to use words and not the power of naming that is affected in this case. The fact that verbal and nominal disorders may occur independently of one another shows that the hypothesis of 'auditory images' is inadequate to explain the phenomena. So long as the lesion is confined to one half of the brain, there is no reason to suppose that loss of 'images,' visual or auditory, is primarily responsible for these defects of speech.

3. THE FUNCTIONS WHICH ARE DISTURBED IN APHASIA AND KINDRED DISORDERS.

From 1868 onwards Jackson insisted that the chief mental activity disturbed in these disorders of speech was the use of words in propositions; those which remain are the same words used 'non-propositionally.' This is profoundly true, and, had we appreciated the wide-reaching significance of this generalisation, we should have been spared many years of wandering in the wilderness.

Although this conception covers the larger number of the facts I have observed, there are some aspects of the loss of function in aphasia which are not strictly comprised under the heading of 'propositionising.' Nor do I believe that it is possible to discover any exact and comprehensive definition which includes all these disordered processes. I shall therefore attempt to enumerate the various activities which may suffer in consequence of a unilateral lesion of the brain, and, for this purpose, it is necessary to consider more particularly the slighter manifestations; severe loss of speech may render all detailed examination impossible.

Words, numbers, pictures and every act which depends upon the use of these symbols in constructive thought may be affected. Any mental process is liable to suffer, which demands for its performance exact comprehension, voluntary recall, and perfect expression of symbolic representations.

I have therefore suggested that the various functions disturbed in aphasia and allied conditions might be spoken of as 'symbolic thinking and expression,' because it is mainly the use of words, numbers and pictures which suffers in these disordered states. But this name must not be taken as in any way defining the group of processes affected; I should have preferred, had it been possible, to have employed some entirely indifferent term. For it is symbols used in a particular manner which suffer in these disorders, and not all symbolic representations.

This group of functions is not uniformly disturbed in every instance; cases of aphasia differ profoundly in their clinical manifestations. The majority, especially in the earlier stages, show evidence of widespread defects in the use of language; the more acute and severe the lesion, the graver and more extensive is the disorder it produces. Many of these changes may disappear, and sometimes the patient recovers entirely; but, in certain cases, one or more aspects of symbolic thinking and expression remain affected over a considerable period, and the aphasic manifestations consequently assume some particular form.

Analysis of these clinical varieties seems to show that the various functions included under the term 'symbolic thinking and expression' can be dissociated in different ways under the influence of organic injury. They do not correspond to different stages of dissolution or recovery. Nor do they reveal directly the elements out of which language is built up. On the contrary, they show the components into which a highly complex set of psychical processes can be separated by destruction of certain portions of the brain. Provided we bear these principles in mind, we are justified in grouping the clinical manifestations under separate headings. Fortunately, all the functions included under symbolic thinking and expression are rarely lost in the same patient; for, if this were the case, he would be reduced to a state in which all detailed examination was impossible.

To each of these groups I have given a name chosen from the most salient defect in the use of words; these names are purely indicative, and are not intended in any way as defining a specific psychical function.

(*a*) *Verbal Defects.* In severe forms of this disorder, the patient's utterance may be reduced to 'Yes' and 'No,' and even these words cannot always be evoked for voluntary use. As speech returns, his vocabulary increases, but his enunciation is slow and halting. Any word he is able to recall can, however, be used for naming an object. It may be so badly pronounced as to be scarcely recognisable, but it is applied correctly. When the patient attempts to repeat what has been said to him, the articulatory sounds are imperfect, but he can usually utter more words than are possible spontaneously. It is characteristic of this form of aphasia that words are evoked with difficulty, and tend to be abnormal in structure.

After the stage of neural shock has passed away, the power of choosing an object to oral or printed commands becomes perfect, and even orders necessitating choice can be carried out correctly, if given in print or in words spoken aloud.

As the spoken vocabulary increases, the power of writing is regained, although throughout it tends to show the same errors as articulatory speech. These patients cannot spell and find difficulty in remembering the order of the letters, even in simple words. They write more easily to dictation, but are unable to carry in the memory a string of words or a long phrase.

The power of reading to themselves with enjoyment is spoilt by difficulty in remembering a series of words accurately; they are frequently compelled to look back to the beginning of a long sentence in order to obtain its full meaning.

The verbal aspect of numerals is affected, but not their significance. Thus, when looking up the page of a book or scoring at cards, the patient may utter the wrong number, but acts as if he had said the right one. Simple arithmetical operations can be carried out correctly, except in severe cases; then it is not the process of addition and subtraction which is forgotten, but the act fails, because of the difficulty in remembering the requisite figures.

These patients can draw, play card games and enjoy jokes set out in print or in pictures. In fact, the disorder from which they suffer affects mainly verbal structure and words as integral parts of a phrase; their nominal value and significance are perfect.

(*b*) *Syntactical Defects.* This is an easy form to distinguish, because the patient tends to talk jargon. Not only is articulation of the words ill-balanced, but the rhythm of the phrase is defective, and there is want of grammatical coherence. The power of naming objects may be retained in spite of the jargon by which he is hampered. Not infrequently, when he cannot utter a word or when the sound emitted is incomprehensible to his auditor, he writes the name correctly.

But although he can write single words, any attempt to convey a formulated statement is liable to end in confusion. Patients suffering from the more severe degrees of this affection cannot write a letter, but, in the slighter cases, writing is easier than articulatory speech. They can all copy correctly and transcribe print into cursive handwriting.

These patients can understand what they read to themselves, provided they are not compelled to reproduce the meaning in words; for then internal speech is also disturbed by jargon.

This disorder is essentially one of balance and rhythm in symbolic expression, and syntax suffers greatly. The patient has plenty of words, but their production is ataxic; this leads him to talk jargon and renders difficult even internal formulation of their meaning.

(*c*) *Nominal Defects.* This is essentially a loss of power to use names and want of comprehension of the nominal value or meaning of words and other symbols. Not only does the patient fail to name objects placed in front of him, but when asked to point to one of them named aloud or in print, the choice, even if correct, is made slowly and with effort.

Such patients read with extreme difficulty, especially if they attempt to spell out the words. Writing is gravely affected, and, although they can copy printed matter correctly in capitals, they may be unable to do so in cursive handwriting. Writing to dictation together with all actions demanding choice are carried out imperfectly to spoken commands; but direct *viva voce* repetition is not affected.

They can usually count and say the alphabet, but suffer from defective appreciation of the meaning of single numbers or letters. This interferes with the power to carry out simple arithmetical operations, and appreciation of the relative value of money is usually more or less affected. Games, such as cards, which demand rapid and correct recognition of names and power to register a score, are impossible, though chess, draughts and dominoes may be played correctly.

Drawing from a model, or from memory after the object is removed from sight, is easily performed; but, when the patient is asked to draw from imagination some such figure as that of an elephant, the result is extremely unsatisfactory. All the distinctive parts are usually omitted.

One of the most instructive forms, assumed by the loss of function, is the difficulty experienced by the patient in drawing a ground plan of some room with which he is familiar. However badly the verbal aphasic may draw, he usually succeeds in indicating the relative position of the windows, doors and principal pieces of furniture. But the patient with nominal defects, fails to produce a correct plan, and tends to slip into an attempt to express the constituent parts in elevation; and yet when asked "where is the table?" or "where is the window?" he can point to their situation correctly.

(*d*) *Semantic Defects.* These consist in a want of recognition of the full significance of words and phrases apart from their verbal meaning. Other functions suffer that have nothing to do with verbalisation, for in this form of the disorder there is loss of meaning in thought. The patient may understand a word or short phrase, and can appreciate the various details of a picture; but the ultimate meaning escapes him.

Thus, although he understands the significance of the words 'Summer' and 'Time,' and knows that 'Summer-time' is "something to do with the Daylight Saving Act," he is unable to say whether the clocks are put forward or back when it begins.

He fails to comprehend the final aim or goal of an action imposed upon him from without. He cannot formulate accurately, either to himself or to others, a general conception of what he has been told, has read to himself, or has seen in a picture, although he can enumerate many of the details of which it is composed; the knowledge he has gained is episodic. Such patients can read and write, but the result tends to be inaccurate and confused.

Counting is possible, and the value of numerals and of coins may be recognised; but arithmetical operations are affected from want of appreciation of the nature of the procedure required.

Such patients fail to understand jokes, which demand complete comprehension of printed words or pictures. They cannot play card games or put together puzzles which confuse them greatly.

These semantic disorders interfere seriously with the activities of daily life and render the patient useless for any but the simplest employment; and yet his memory and intelligence may remain on a comparatively high general level. He does not forget people or places; he can recall spontaneously events both recent and remote, and may be able to give valuable information with regard to his disabilities. But, if he is set the task of retailing accurately some story which he has been told or has read to himself, he is liable to become confused and to omit many factors of importance.

Amongst the many functions comprised under the term 'symbolic thinking and expression' some are affected in one case, some in another, and the clinical manifestations of aphasia depend mainly on the incidence of this loss.

But, apart from this character, the degree to which these functions are disturbed has a profound effect upon the condition of each individual patient. One of them may fail to carry out a test correctly, which another can execute without difficulty; and yet both seem to belong to the same functional group. Moreover, as the aphasia gradually passes away, the patient may respond so differently to the various tasks he is asked to perform, that at first sight the nature of his disability seems to have changed. Thus, not only the form but also the degree to which symbolic thinking and expression are affected has a profound effect on the nature of the clinical manifestations.

The more nearly a symbolic action approximates to a frank proposition, the greater difficulty does it present, and the patient will probably fail to execute it correctly. Conversely, the more closely it corresponds to matching two sensory patterns, the less likely is it to be disturbed. Highly complex symbolic acts suffer more gravely than those of lower propositional value.

For example, if a patient seated opposite the observer attempts to imitate a series of movements, which consist in touching an eye or an ear with one or other hand, he may fail grossly. He can however execute them perfectly when the observer stands behind him, and they are reflected in a mirror. But, when he is asked to write down what he sees, he again fails as badly as before.

For in order to imitate movements sitting face to face, he must formulate the various factors, which make up the command; whilst to copy them when reflected in a mirror is an act of almost pure imitation. As soon, however, as he is asked to write down what he sees in the glass, he is compelled to formulate the task in words and fails to describe gestures he can imitate perfectly.

The higher the propositional value of the mental act, the greater difficulty will it present. Thus, a patient may be able to execute a printed command to hold up his hand, although he is unable to carry out an order to touch with it his eye or his ear. The addition of the second factor has rendered the task too difficult. The larger the number of possible alternatives presented by the order, the more certainly will the desired action be defective. It is possible in this way by suitable tests to grade the patient's disability. First, he is required to carry out a series of movements, such as 'lift your hand,' 'shut your eyes,' etc., which name one part of the body only. Then the right or left hand is specified, and the task is gradually increased in severity until finally it is brought up to the multiple alternatives of the full hand, eye and ear tests. Somewhere on this ascending scale of difficulty, most aphasics will break down.

All propositions express an abstract relation; but even the same generalised statement may vary in difficulty according to the means by which it is expressed. For example, during the compass test, the aphasic patient may be entirely unable to record in speech or in writing whether he has been touched with one or two points. But, if the figures 1 and 2 are written on a sheet of paper, he can indicate correctly whether the contact was single or double. Under the ordinary conditions of this test, he is not only compelled to formulate to himself his sensory impressions, but he must express them in verbal symbols; when it is carried out according to the second method, he has only to determine whether his sensations are of one or of two points, and the means of expression lie on the table before him.

Although the patient is conscious of the position in space of objects around him, he may be unable to express their relations. Thus, when asked to draw a plan of some room with which he is familiar, he may fail completely. But, if a figure is drawn to represent, for instance, his bed in the hospital ward, he may be able to indicate the relative position of the various surrounding objects; this is a lower method of symbolic expression. There are however cases in which even this is impossible; the patient can no longer formulate the relation of the doors, windows or beds to one another, although he can still point to the situation of any one of them. He can remember the place of the window or the door, but he cannot express the relative position they bear to one another.

When the defect consists mainly in want of power to evoke words or names, the patient may be unable to name a set of common objects, although he can choose them correctly to oral or printed command. The verbal symbol conveys its proper meaning, when presented ready formed in sound or print; but to produce it freely at will demands a greater perfection of symbolic thinking and expression.

In the same way when an aphasic calls the scissors "That's the tweezers to cut with," or speaks of a kitten as "a little fur-child," he is expressing a lower grade of symbolic recognition. Patient no. 2 was unable to name a series of colours placed before him and could not choose them correctly to oral or printed commands. But if, instead of the names, he was allowed to call black "What you do for the dead," red "What the Staff wear" with similar descriptive phrases for each of the other colours, he named them all correctly. These metaphorical modes of expression are simpler than direct uni-verbal nomenclature; for the more concrete is lower in the hierarchy of mental processes than the general and abstract.

Behind every propositional expression lies recognition of the meaning of symbols. I must be able to appreciate the full significance of words, numbers and pictures, before I can pose them in propositional form. This loss of meaning is the cause of many aphasic defects. Thus, the short and long hands of the clock have acquired a significance which converts each one of them into a direct symbol, and they are often confused or used wrongly in cases of aphasia. Moreover, we are in the habit of dividing the space between any two numbers on the face of the clock into portions of an hour, giving to each its symbolic value. In many forms of disordered speech, the patient no longer sets the hour hand at a point proportionate to the position of the minute hand. Told to set a quarter to six, he places the short hand at six and the long hand at nine, so that it is impossible to discover without questioning him whether he intended to set 6.45 or 5.45. Not only the hands of the clock but their relative position on the face have acquired a meaning which is disturbed in aphasia.

When adding two numbers together, it is easier to reach the answer by counting, or progressive movement, than by a direct proposition. Thus, many aphasics can add 6 and 3, by saying to themselves 6, 7, 8, 9, but cannot formulate $6 + 3 = 9$. For the same reason some of them could set the clock correctly to a command in railway time, but not if it was expressed in words. Told to set the clock at 4.45, the patient placed the hour hand at 4 and then swept the other one round the face to a position he associated with 45 minutes. But asked to set 'a quarter to five' he placed the one hand at 5, whilst the other hovered uncertainly between 'a quarter past' and 'a quarter to.' On the other hand, when he attempted to tell the time set for him on the clock, it was equally difficult to evoke the names '4.45' and 'a quarter to five.'

Many of the disorders I have described are the result of inability to recognise the nominal or verbal significance of symbols. If, however, the patient fails to appreciate with certainty their ultimate meaning in thought, he is liable to become confused over the intention of an action, whether self-suggested or imposed from without. He no longer perceives clearly the goal at which he must aim. Thus, a young officer was unable to put together his belt when the slides had been displaced, or collect the materials he required before beginning to shave. Another patient could thread the quadrilateral frames for his bee-hives, if the action consisted in bringing the wire across from one side to the other and back again through neighbouring holes; but as soon as he attempted to go from corner to corner he failed entirely.

Perfect symbolic thinking and expression demand that words, numbers, pictures, and all they stand for in thought, should be mobile and susceptible of perfect voluntary manipulation. It is this aspect that is mainly affected in the verbal and syntactical forms of aphasia; they are not due to a dysarthria, but are produced by defective conceptions of the structure and rhythmic balance of the symbol, which interferes not only with articulatory speech, but also with internal verbalisation. One of my verbal aphasics, when saying the alphabet aloud, stumbled over R; he explained that he wanted to say A because he was thinking of 'are.'

Thus, in verbal and syntactical aphasia, it is the idea of the structure of the word which principally suffers both in internal and external speech. With such disorders, uttered words are more affected than the verbal processes of thought, and it is for this reason that I have spoken of symbolic expression. On the other hand, nominal and semantic defects mainly disturb the use of words in the varied processes of thought and their perfect employment as language symbols.

4. CONCLUSIONS.

The points I have tried to bring forward may be summarised as follows:

(i) A unilateral lesion of the brain, affecting the use of language, disturbs a number of psychical processes, which cannot be grouped under such headings as 'speech,' 'reading,' 'writing,' 'motor' or 'sensory' activities, or any other *a priori* categories.

(ii) I have ventured to apply to them the term 'symbolic thinking and expression,' because they consist mainly of the use of symbols in language and in thought; but it is symbols used in a particular manner which are affected, and not all symbolic representations. This term is in no sense a definition, for this group of functions cannot be comprised within any single general conception; they can be determined solely by experimental observation.

(iii) The various manifestations of aphasia and allied disorders of speech cannot be explained by destruction of sensory images. These may remain intact, although they cannot be used voluntarily as part of the symbolic mechanism of language.

(iv) These researches show that the two aspects of meaning inherent in the use of symbols may be separated by suitable lesions of the brain. Want of perfect recognition of verbal significance leads to a defective power of naming; whilst what I have called 'semantic defects' produce a loss of the general meaning of words, phrases, numbers and pictures.

THE OUTLOOK FOR VOCATIONAL PSYCHOLOGY[1]

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I. *The two lines of advance already followed in vocational psychology* (pp. 194–197).
  - (*a*) *Success has been achieved in the placement of routine workers.*
  - (*b*) *Can we hope for success in diagnosing higher qualities of mind?*

II. *The relation of general ability* (*g*) *to the specific intellectual aptitudes* (pp. 197–200).
  - (*a*) *So far this relation has not been clearly understood.*
  - (*b*) *Imitative tests must be used until we can psychograph the vocations accurately.*
  - (*c*) *Some defects of the 'empiric' and 'analytic' tests.*

III. *An attempt at an analysis of human ability* (pp. 200–205).
  - (*a*) *The 'g,' 'w' and 'c' factors in intelligence.*
  - (*b*) *The need in our tests for appeal to the complete intelligence, not merely to the intellectual qualities.*
  - (*c*) *Some suggested methods of testing the higher human qualities.*

I.

THE value of psychological tests for the determination of vocational fitness is now generally recognised to be considerable. A steady succession of investigators has demonstrated their wide possibilities since S. B. Thompson, by engaging as bicycle-ball inspectors only those girls whose discriminative reaction times were speedy, showed that a careful selection of employees for specific tasks might enormously increase industrial efficiency. The success of Walter Dill Scott in choosing salesmen by psychological tests, of Hugo Münsterberg in estimating the abilities of street-car drivers by their reactions to an experimental situation which produced as nearly as possible the mental conditions of their work, of E. L. Thorndike in diagnosing capacity among workers in the clerical and academic professions, of Henry C. Link in differentiating among good, bad and indifferent applicants for routine work in the engineering industry, and of the American psychologists as a whole in selecting on a huge scale the men most suitable for executive positions in the United States army during the recent world-war, provides indisputable evidence of the vitality of vocational psychology in America.

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[1] A paper read at a meeting of the British Association (Psychology Sub-section), Cardiff, 27 August, 1920.

It may, indeed, seem at first sight to some that the field of experiment already covered embraces in its broad sweep little less than the whole province of industry, but there is still much untrodden and difficult ground to explore before we shall be able to say that complete success has been achieved. Moreover, there are certain efficiency experts of unquestionable shrewdness who declare that by quite other means and without the elaborate methods of statistical science they have gained a point of vantage from which they can clearly discern the nature of the more elusive character qualities which we have found it extremely hard to track down and study at close quarters; indeed, they dispute our ability either to dispossess them or to move nearer by laboratory methods to the solution of those problems which have hitherto baffled us. So difficult is it to see the whole prospect in sober perspective, uninfluenced either by ill-founded confidence or unwarranted despair for the future of applied psychology, that an exacter analysis of how things stand is desirable.

In this connexion it may be profitable in the first place to distinguish between two streams of experimental activity which issued originally from different conceptions of the problems to be solved. On the one hand there has been the more recent search for authentic tests for general intelligence with which the name of Alfred Binet will always be predominantly associated, while on the other hand, we have witnessed the attempt initiated by F. W. Taylor, and taken up by our industrial organizers, to select workers for specific occupations not so much in virtue of their general all-round ability as for those particular aptitudes (such as steadiness of hand, accuracy of movement, rapidity of reaction, or quick recognition of sizes and shapes, and so on) which would facilitate their economical and effective adaptation to the demands of an industrial system, based on the functioning of highly specialised activities.

For much of the routine work to be done to-day scientific management, as we know it in the form popularised by Taylor, does not need the complete man endowed with wide powers of intelligence; it calls merely for animated abilities of a minutely differentiated kind. Thus, for example, Taylor wrote with respect to his ideal pig-iron loader, "One of the first requirements of a man who is fit to handle pig-iron as a regular occupation is that he more nearly resembles in his make-up the ox than any other type[1]."

In those industries in which Taylor's dream is becoming realised of an organization which involves a distinct separation between the planning department and the performing department, so that all the brain-work falls to the former, and to the latter all the bodily toil, it has not been found difficult to devise tests for the discovery of workers possessing specific aptitudes utilisable in tasks of a mechanical nature, and for two reasons. In the first place, it is not usual that among those who are content to devote themselves to routine factory work the specific aptitudes which they may possess will be overlaid and partially concealed by any considerable intelligence; while, in the second place, their cultivated interests may be so few and feeble as to leave these specific aptitudes as the sole peaks of eminence in the monotony of their comparatively stunted mental life. More than anything else, the presence of marked intelligence and strong interests complicates the exact estimation of the simpler mechanical aptitudes; moreover, the existence of a strong fund of intellectual energy is the most serious disqualification one could be burdened with in the attempt to achieve a lasting adaptation to much of our present-day factory life. The machine feeder, for example, must not be too intelligent, or he will quickly exhaust the interest of his work, become restless, dissatisfied, and a nuisance to his supervisors, or begin to look out for new employment. Ideally, then, the factory operative, according to the view of the complete Taylorist, will be just a semi-automatic attachment to the machine which he or she feeds, a 'hand,' to use a much-discredited term, rather than a 'brain' or a 'will.'

In support of the view that little intelligence is required for a considerable amount of present-day work the following paragraphs from a dialogue to be found in Link's *Employment Psychology*[2] may be quoted:

> *Mr W.* What became of the morons[3] you found?
>
> *Miss N.* We assigned them to some simple machine-work and they turned out to be excellent operators. They were very quiet and steady. One of them by the way we discovered by her attempt to do the *hard-directions* test. She stumbled along till she got to the direction "Tell where the sun rises, in the east or in the west?" She pondered over this for a while and then remarked, "That's a hard one." "Why, don't you know where the sun rises?" asked the examiner. "No, I used to know but I forgot," was the answer. Yet we found that this girl had the ability to do something and do it well.

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[1] Taylor, *Principles of Scientific Management.*

[2] Pp. 136–137.

[3] *Moron*: a person of defective intelligence.

Mr W. But how about the higher grades of intelligence?

Miss N. There again we find that no matter how high a person's general intelligence may be, it does not follow that he will be especially intelligent in a special kind of work. For instance, another girl who took the test I mentioned came to the same question and put down as her answer "The sun does not rise, but the earth revolving on its axis makes it seem as if it came up in the east." This girl was a high-school graduate and above the average in intelligence, but she did not make good at the clerical work for which she was hired.

It may be added that Link's work admittedly became increasingly difficult as he moved away from the level of the routine worker, and attempted to deal with those of a more intelligent type.

While, then, we may say that considerable success has marked the efforts of Taylor and his successors to make workers of a low-grade type fit into positions in the industrial machine suitable for the functioning of their almost mechanical aptitudes, can we reasonably hope that through the application of psychological tests we shall be able eventually to fit the worker with higher qualities of mind into the sort of position where live interests will not interfere with his efficiency, and a high degree of intelligence will be a real asset?

II.

In investigating this possibility we shall find that what has been so far done in the selection of workers for tasks other than those of a routine nature is less unique than at first sight it seems to be. Thus, Mr Cyril Burt[1] has pointed out that Münsterberg's famous car-driver test did not so much discover the men, and those only, with the peculiar qualities needed for securing a rapid and accurate adjustment to the changing situations of streets, as those with just an undifferentiated general ability which might have been equally well applied elsewhere; moreover, the fact that the college students who also did the test came through, on Münsterberg's own admission, with better results than the most highly-trained drivers, would seem to support this view. If, then, this contention of Mr Burt, and similar contentions, are reasonable, what we have been doing in the selection of workers above the merely routine level, of salesmen and clerks, of car-drivers and telephonists, of army officers and others in executive positions, and so on, amounts to no more than the selection of those among our subjects who have possessed, not special aptitudes, but the greatest amount of general ability. In short, we have weeded out not the *misfits*, but the *unfits*. The American army psychological tests may be quoted as the supreme example of this method of choosing men for highly specialised work by virtue of their possession of an all-round general ability which, it is reasonable to suppose, could be utilised in a variety of circumstances with equal success.

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[1] See *Industrial Administration* (Pitman and Co.), pp. 90–91.

An indispensable and essential preliminary, therefore, to a correct psychography of the vocations—without which vocational selection can only take place in an extremely unscientific fashion—is a satisfactory determination of the relations which hold between general ability and the specific aptitudes called for in the performance of particular tasks. Till this is done, we shall be greatly handicapped. Thus it will be comparatively futile to conclude that a salesman needs, let us say, a good memory and then proceed to test his memory for nonsense syllables without knowing either whether we are testing exactly the same aptitude as functions in his work or whether it is independent of general ability or not.

Until the psychography of the various industrial occupations is successfully accomplished, however, there can be little doubt that it will be advisable to construct vocational tests in such a way as to reproduce as nearly as possible, while allowing for practice effects, the mental conditions of the work for which we may be attempting to diagnose fitness but without stumbling into the pit which Münsterberg apparently could not avoid.

The method most popular, I believe, in America at present differs from this; it is to try out a number of miscellaneous standardised tests, to reject those which show no correlations with known ability for any particular vocation and to retain those which grade actual workers in the order of their trade ranking. Thus, it has been found that the *Completion Test* produces a correlation of + ·63 with engineering ability, and the *Knox Cube Test* one of + ·73 with aptitude for label-pasting[1].

It is difficult, however, to feel quite satisfied with this method. One cannot escape the conviction that either general ability or purely accidental causes were responsible for such a high correlation between such dissimilar activities. If, for example, it should be found that in engineering a turner's ability corresponds closely with his accuracy in checking numbers but that another test which reproduces with greater faithfulness the essential mental conditions inseparable from a turner's work is equally significant, then we must inevitably have greater confidence in the latter than in the former. In the process of turning, the accuracy of judgment demanded is displayed not continuously as in number-checking, but at the end of long periods of concentrated attention, so that a test which calls for the accurate mechanical adjustment of a pointer to the surface of a rotating drum at the moment when a slowly approaching mark comes directly under it will be much more promising than a test involving an acquaintance with words, numbers, and abstract ideas. We may still be testing general ability but there is at least a chance that we shall be testing specific aptitude in addition. (But even in applying such a test as this we should discover that another important factor, *pride in a finished job*, is not being sufficiently evoked. The chief satisfaction a good turner derives from his work is in seeing the spindle, let us say, which, like a sculptor, he has brought out of the rough, lying shining and neat before him.) The point to remember is that the farther we get away from the activities actually employed in the work the more likely it will be that we shall miss whatever specific aptitude may be demanded in the actual task.

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[1] See Hollingworth and Poffenberger, *Applied Psychology.*

This method of constructing 'imitative' tests is advocated because it obviates for the present the necessity for awaiting a clear understanding of the relationship between general ability and the specific intellectual aptitudes, while unconsciously through its use the skilful investigator for whom psychology is an art as well as a science will be able to bring into operation mental factors definitely essential but not yet revealed by conscious scientific analysis. Accordingly, Link's practice of working himself at the tasks for which he was devising tests is the model procedure strongly to be recommended.

One cannot help feeling that the method of Seashore in investigating separately—if this, indeed, is possible—all the qualities necessary for success as a singer, or of Münsterberg in testing telephonists in a similar manner is open to objection and has only succeeded in their hands because its defects were unconsciously made good by the presence of unusual insight and skill.

Suppose, for example, we test two telephonists for sight, hearing, accuracy of co-ordinated movements, immediate memory, and general ability. To proceed, as Münsterberg did, merely to add up the scores in each test as they stand is to assume that all the qualities are equally important. How they function in proportion to one another in the actual work may be quite another matter. If we assume, however, what is not unreasonable, that poor sight may be remedied by glasses, and that intelligence will often fill in auditory blanks, and if we accordingly *weight* the scores for immediate memory and general ability in a suitable manner, we may actually produce in certain cases a *reversal* of the order of ability which the former method, as Münsterberg employed it, gives. Experiment, however, is necessary before this matter can be cleared up satisfactorily.

III.

The vocational psychologist has so far, I believe, been tempted to proceed with his work on the assumption that intellectual abilities are all-important as qualifications for vocational fitness. I believe that we shall only be able to get a deeper insight into our problems through the systematic employment of some such analysis of our human abilities as takes into account not only these but other psychical factors as well. Thus to attempt such an analysis:

Man may be regarded in two ways; as a *creature* capable of adapting himself to the environment in which he finds himself placed, or as a *creator* capable of moulding his environment to suit his will. Under the aspect of *creature* he may be looked upon, in the manner almost traditional now, (1) as a mechanism to be studied through psychophysics, (2) as an organism with his perceptions and impulses coloured by his instinctive preferences and antipathies, or (3) as a rational being capable of deliberation and foresight; while under the aspect of *creator* he will appear as the master of these means to his self expression, capable as a *super*-mechanism of creative artistic work (but, when little more than a *super*-mechanism, of excessive preoccupation with delight in the mastery of his materials); as *super*-organism of ideal devotion and purpose, or as a *super*-rational being of intuitive insight and understanding[1]. In this connexion the conceptions, with which we have recently become familiarised, of a g*, of a w†, and of a c‡ as factors combining to make up total personality, may be utilised as aids to the explanation of such a scheme. We may thus set out the above ideas (see diagram on p. 201).

In the light of this diagram it will be clear that the perfect intelligence test will call into play the power of mastering new problems as well as of dealing effectively with old ones, of exhibiting interests and representing instinctive motivation as well as mechanical efficiency. Thus we may define intelligence as the general capacity of an individual to adjust his thinking both to familiar and to unfamiliar requirements of his environment whether it is that to which he is accustomed or unaccustomed, and to mould it when necessary to suit his needs.

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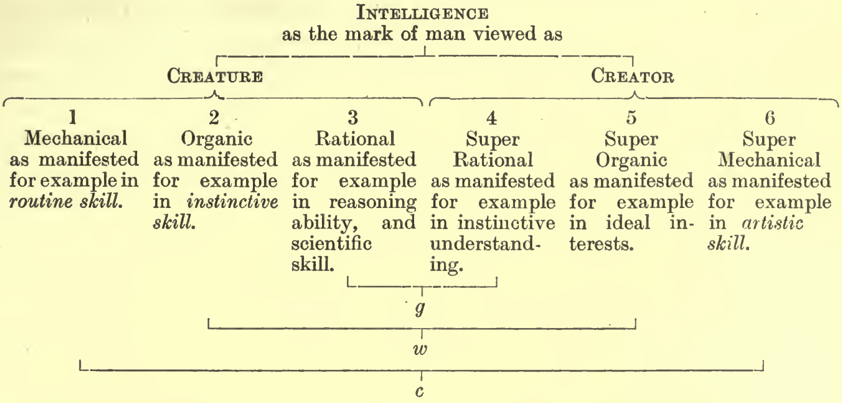
[1] This analysis (slightly modified here) is to be found in Mr Benchara Branford's *New Chapter in the Science of Government*, § 19.

* The 'general factor' of Prof. Spearman. *Br. Journ. Psych.* v, No. 1.

† The 'persistence' factor of Dr Webb. *Br. Journ. Psych. Monog.* No. 3.

‡ The 'cleverness' factor of Mr J. C. M. Garnett. *This Journal,* IX, Nos. 3, 4.


INTELLIGENCE
as the mark of man viewed as
CREATURE
CREATOR
1
Mechanical
as manifested for example in *routine skill.*
2
Organic
as manifested for example in *instinctive skill.*
3
Rational
as manifested for example in reasoning ability, and scientific skill.
4
Super Rational
as manifested for example in instinctive understanding.
5
Super Organic
as manifested for example in ideal interests.
6
Super Mechanical
as manifested for example in *artistic skill.*
g
w
c

From this point of view the American army tests will be seen clearly to be not completely satisfactory for the diagnosis of the higher forms of intelligence; it is the correct but stereotyped rather than the original and creative response which they call out. Moreover, through the time-limit imposed for their performance, they over-emphasize the factor of speedy mental *tempo* in pencil and paper work. Again, viewed in the perspective of our diagram they will be seen to lack any appeal to the *w* qualities, while it will be generally admitted that they place workers who are not engaged in clerical occupations at too great a disadvantage. They are excellent, no doubt, for the discovery of those persons possessing the ability to fill minor executive positions in which accurate comprehension of instructions, and promptness and faithfulness of despatch are demanded, but they would only reveal by accident, if at all, a Newton, or an Edison, of comparatively slower reaction but enormously greater intelligence than the first-class clerk possesses.

From this criticism it follows that from performances in a test involving correct reactions of a predetermined kind we can get no more than shadow pictures of *creative* intelligence, and after all the best intelligence test will be the one which allows for the display of the greatest possible degree of intelligence and not less. Cancellation, number-checking, and association tests fail to come up to this requirement, and consequently we cannot expect that they will avail to sort out for us our Darwins from our well-educated dunces. Speaking generally we may say that the disciples of Taylor have constructed tests for the intelligence shown in column 1 and the American psychologists tests for that shown in column 3.

In vocational work, then, we shall need to make a wider and deeper appeal in our tests to make them successful with workers above the routine level. To quote an example. We may give a simple *form-board* test to fitters and find perhaps that the ability to recognise and place familiar geometrical shapes into recesses shaped to receive them is a good indication of routine fitting ability, but since it does not appeal very strongly to the *interest in making things work*, which after all is the motive force driving the better boys into fitting as an occupation, it cannot be the test most serviceable in the long run. A boy's ability, however, in constructing, so that it will work, a simple arrangement in which certain mechanical principles are utilised, as for example in a bell of novel variety, is a completer indication of fitting ability, since constructive ability which has an instinctive basis and involves more than the *g* activities is here called for. I have found that such a test as this correlates with the trade ability of fitters as closely as + 0·6 while the *form-board* test produces a much lower and usually insignificant correlation. The putting of things together with constructive skill measures, that is to say, a greater common factor of the fitter's intelligence. Link has found, it may be noted, that a mechanical puzzle test such as I have described is usually *too* difficult for those engaged in merely *routine* fitting.

No real progress will be made, I believe, with the application of psychological tests to workers on non-routine tasks involving specific aptitudes and interests unless, then, we learn to appeal not only to the intellectual qualities but also the *c* and *w* qualities. We may discover by intellectual tests good fitters who fail subsequently because of temperamental defects. It would be fatal to predict for one who scored 80 per cent. in tests like the American army tests a greater success in a sphere where initiative and patience are required than another who scored 75 per cent., if the latter should happen to possess a big fund of curiosity and constructive interest.

It must certainly seem that the vocational psychologist will need to take into consideration what has so far been overlooked—the instinctive make-up of the subjects of his experiments, and that in choosing workers for particular tasks he must make sure that their instinctive type is the right one. It would be folly to place the man with strong gregarious tendencies on lighthouse or wireless work, or advise him to go farming in a new colony, just as it would be unwise to encourage a boy devoid of the acquisitive instinct to set up in business. And it will be agreed that a soldier or a reformer without a considerable spice of pugnacity would probably prove a failure, while it is equally beyond dispute that the statesman and the priest, the doctor and the nurse, the teacher and the shepherd, would soon tire of their work if they were not endowed strongly with the tender impulse. The next few years will show whether we shall be able to devise successful tests for the diagnosis of the relative strengths of man's various instinctive qualities.

Meanwhile something may be done by the utilisation of tests already in existence which have not been so far employed to any extent in the investigation of certain instinctive character qualities. For example there is a test in Whipple's *Manual*[1] which has not been exploited to anything like its full possibilities if at all, for vocational purposes. This is the well-known *Testimony Test.* The method of its application is probably familiar to everyone. A subject is shown an interesting picture which he studies carefully and in detail for two or three minutes, and afterwards answers from memory a series of questions concerning it. Many of the questions are deliberately framed to confuse and mislead him. During the cross-examination the subject may indicate the degree of his felt certainty in the reliability of his replies. He may use the numbers 1, 2, 3, and 4 for this purpose. Thus he marks his answers with a 1, when quite uncertain whether they are right or wrong; with a 2, when there is a hesitation to commit himself too far; with a 3, when he is positive that he is right; and with a 4 when he feels he could safely take an oath that they are correct.

The value of the test for the determination of character qualities depends upon the following theoretical presuppositions which experiment largely justifies:

(*a*) The degree of readiness of a subject to commit himself is a rough measure of his general decisiveness in situations where there is a common tendency to hesitate. When justified by results this is an invaluable quality for executive work.

(*b*) A person of little reliability commits himself too freely, *i.e.* in the absence of valid reasons. Such a person is obviously unsuited for a position of responsibility.

(*c*) Equally unfitted, also, for responsible work is the person who is too cautious or timid, who has good grounds for holding decided opinions but rarely or never commits himself to the full, so that the figure 4 is practically absent from his answers list.

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[1] *Manual of Physical and Mental Tests,* II.

In applying this test (in conjunction with a colleague[1] who suggested its use) to the head of a department in a big store we found that whereas she made an excellent score in 'following instructions' she was unwilling to commit herself to any marked extent in the testimony test. This avoidance of decisiveness was also supplemented by a disinclination to tackle questions of a problematical nature when others of a routine nature could be answered instead. We, therefore, formed the opinion that while she might make a successful head of a department in a business where each department depended for its prosperity upon the wider efforts of the management, so that she would be floated along, let us say, on the wave of the firm's general efficiency, she would be unsuited for *pioneering* a new venture outside. No such judgment could be made on the basis of the results of tests involving nothing but intellectual qualities. It was interesting to find that the managing director was willing to accept our estimate as probably correct though he had himself formed quite a different judgment of this lady's capabilities. But he recalled the details of several cases where men had failed in outside independent ventures, and failed more than once after a long record of successful management within and he was willing to believe that we were probably right. The testimony experiment may, therefore, prove invaluable to the vocational psychologist.

In conclusion I may be permitted to mention and describe a test of my own which has proved equally serviceable. It will be interesting and this must be my justification for mentioning it, because it introduces, I believe, a principle into vocational psychology, which has not been consciously employed hitherto, the principle of the *optional question*. I have found that by the employment of the choice principle a test can be constructed which looks like an intelligence test and so takes the 'censor,' to use the Freudian metaphor, off its guard, but which is in reality quite as much a revelation of the character as of the intellectual qualities. The test takes at least half-an-hour to complete and there is no time limit imposed. Ten questions have to be answered, one on each of ten pages. On each page three questions are set out involving for their reply (i) a routine operation such as counting figures, or making a list of common articles, (ii) the accurate comprehension of instructions with the ability to follow them, and (iii) the tackling of a problem of a more difficult kind. It has been found in a large number of cases that in the absence of a time limit, subjects do not, as the non-psychological student might conclude, choose what may be regarded as the simplest questions. Subjects have almost invariably been found to choose the *questions which interest them most.* If the subject is instructed to mark with a cross the question which he would have answered had he not chosen as he did, we can get a shrewd idea, through the third and neglected questions, of the kind of thing he systematically avoids. I have managed to arrange, in addition to the choices mentioned above, choices between questions with an abstract and a concrete reference, between those involving an interest in the ego and in general ethical principles and between those involving an appeal to various specific aptitudes. It is a test which may, therefore, be constructed so as to give us a good general picture of a subject's mind in its broader outlines. The psychology of the optional question[1] has not been seriously studied. So that even if a more extended use of my particular type of test should prove unfruitful, the conscious and systematic application of the optional question principle should open up a way into the province which we are all eager to enter, where the secrets of personality lie concealed.

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[1] Mr E. R. Mason-Thompson of the Pelman Psychological Laboratory.

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[1] I am indebted to Mr Cyril Burt for this phrase used in connexion with the test first described.

PSYCHICAL SELECTION: EXPRESSION AND IMPRESSION

By C. LLOYD MORGAN.

1. *Introductory* (pp. 206, 207).
2. *Relation of Expression to Impression* (pp. 207–209).
3. *Imitation. Behaviour with and without Intention* (pp. 209, 210).
4. *Similar Expression with different Impression* (pp. 210, 211).
5. *Similar Expression in different situations* (pp. 211, 212).
6. *Behaviour in 'Courtship.' Is it primarily intentional?* (p. 213).
7. *Two sub-classes of secondary sexual Characters. An example from Insect life* (pp. 213–215).
8. *A 'taste for the beautiful'* (pp. 215, 216).
9. *The stress on 'recognition'* (pp. 216–218).
10. *The Expressive Behaviour of the Male* (pp. 218–221).
11. *The coyness of the Female* (pp. 221, 222).
12. *Conclusion* (pp. 222–224).

1.

Whatever may be said for and against the hypothesis of natural selection, this is not the place to defend it. Properly safeguarded I for one accept it whole-heartedly. Here I am concerned with psychical selection. In some sense of the phrase it has unquestionably been a factor in evolutionary progress. I propose then to discuss what seems to be its chief relational feature. This will involve a reconsideration of some of the data afforded by sexual selection. But for our present purpose, and in this *Journal*, the emphasis falls on psychological factors. And since psychology does not deal only with mental processes in man, since, too, salient examples of that which is to be considered are afforded in the life of birds, most of the illustrations will be taken from their behaviour and what seems to be implied thereby.

Some reference will be made to the emotional qualification of consciousness or, as I prefer to say, of enjoyment. Fully realising the great value and importance of Mr Shand's treatment of the emotions as conational complexes definable by their ends, I here regard them as affects, describable only as what they feel like, though the concrete instances of mental process in which their presence may be detected are no doubt cognitive and conative as well as affective. I am not here concerned, however, to distinguish their several qualities, save incidentally in a quite perfunctory way. This does not fall within my picture. But a more general question is pertinent. What is the good of emotion—not of this or that quality of affect—but just of emotion as such? What utility can be assigned to it? For our present purpose, without staying to defend the position, I define emotion as super-normal enjoyment, positive or negative. And the good of it—its utility from the evolutionary point of view—is to modify the behaviour of the organism with whose functional activities such super-normal enjoyment is correlated. Behaviour is generally enhanced and rendered more strenuous; sometimes its level of vigour is depressed; and occasionally, in abnormal cases, its effective integration is interfered with; but in these last cases its biological end is defeated. It need hardly be said that there is in this broad view nothing to preclude differentiation of behaviour in accordance with the specific quality of the emotional affect in correlation, let us say, with the 'hormonic' balance in the organism.

A further introductory sentence must be added. I use the word 'instinctive' as adjectival to behaviour (or to a disposition to behave) in so far as such behaviour has not been learnt in the course of individual life. It is, however, in this usage, very often only an instinctive *factor*, which, though inseparable, is distinguishable, under analysis, from the acquired factor due to individual learning and profiting by experience. I think that in reference to behaviour, and its correlated enjoyment, this usage may be retained without prejudice to the different usages of Prof. McDougall, of Dr Drever, and of others whose universe of discourse is in one sense narrower, and in another sense wider, than that of the following discussion; narrower in so far as it is mainly concerned with instinct in man; wider in so far as it is applicable to more than behaviour with its specifically correlated enjoyment.

2.

In the life of birds the behaviour is apparently similar under different circumstances—for example when intruders of the same or of allied species enter the breeding territory, when so-called courtship is in progress, and when the young are disturbed. In each situation the bird appears to be in a state of excitement which we interpret as emotional; but the emotional qualification of enjoyment seems to us to be anger in the first case; to be that which accompanies strongly felt sex-hunger in the second case; and to be something of the nature of distress in the third case. What we have to lay stress upon is the similarity in the type and intensity of the response at different emotional periods. Here the question of biological value naturally arises. Is the similar behaviour contributory either to race-maintenance, or to self-preservation, or to both, under all the different circumstances in which it is observed? Those who assert that only that structure or that behaviour which has utility can escape elimination in the struggle for existence, seem bound to contend that the behaviour must be useful at some period of life; but there seems no reason why we should reject the hypothesis that the same type of behaviour may be useful in different ways in different situations. As Mr Eliot Howard says in his *British Warblers*: "If it can be shown that there is a strong probability of some specific type of behaviour serving a double or a treble purpose, so much the more reason is there for its survival. The reactions that we meet with in bird life may thus be said to have developed firstly as a means for arousing the requisite amount of pairing hunger in the female, secondly as a warning to intruders; and thirdly as a protection for the helpless offspring. Clearly everything centres round the strength of the probability in each case[1]."

In the third case to which Mr Howard here refers, the protection of the helpless offspring is afforded indirectly. The behaviour of the parents is such as to draw to themselves the attention of intruders, and thus to divert attention from the young, which are thus preserved from injury. Now we may speak of the behaviour in all three situations as an *expression* of a disposition which is emotionally toned. And in all three situations the suggested utility of the expression—that utility of which we seek evidence—arises from the *impression* which is produced thereby—in the first by arousing the requisite pairing hunger; in the second as a warning to intruders; and in the third by diverting attention from the helpless young. I propose, therefore, to use the word 'expression' for that which is correlative to 'impression,' on the understanding that such impression is normally provocative of behaviour in the organism which receives it. On these terms expression is a sub-species of behaviour differentiated from other modes of behaviour in that its utility lies in the impression it produces on some other organism; while the impression is a sub-species of presentation differing from other presentations in that what is presented is expressive behaviour on the part of some other organism. If this usage of the words be accepted, the points for emphasis are (1) that what is apparently the same expression is expressive of different emotional states, and, further, (2) that the impression produced is different in different situations. On the thorough-going doctrine of utility there must be some biological value in each situation. But not all behaviour is expressive in the sense suggested. It seems that, in bird life, there are many activities which are not expressive on these terms. Birds are very active in the early hours of the day, especially during the breeding season. But how much of their procedure is expressive, in the sense that the utility, or even part of the utility, of what they do lies in the impression it produces on another bird, or animal, and, through that impression, influences *its* behaviour? Not all certainly; still, some of it is thus expressive and does produce an impression; and that is what we are here concerned with.

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[1] Part IX, 8, 9.

3.

Passing reference to imitation is here in place. Setting on one side what Prof. Mark Baldwin speaks of as 'self-imitation,' it takes at least two to constitute an imitative situation. In such a situation there is behaviour on the part of *A*; and this behaviour is expressive in our sense in so far as there is an answering impression received by *B*; and the behaviour of *B* is imitative in so far as it resembles the expressive behaviour of *A*. Imitation is therefore a specialised case of the relation of expression to impression. Since, however, there are those who contend that imitation, *sensu stricto*, implies intention to imitate, it should be understood that I here use the word as inclusive even of what the biologist may term 'mimetic' behaviour where intention is absent. Are there then cases of imitation in a broad sense which are to be regarded as instinctive? From my point of view there can be no doubt as to the answer. There are forms of imitation in this broad sense which have not to be learnt but just come by nature.

Now perhaps it will be said that if we rule out intention we thereby place such so-called imitation altogether outside the universe of psychical regard. But is this so if the impression is itself psychical in its nature? I assume that as an enjoyed presentation it is so. And I urge that, if so, the expression which gets its value as such in relation to the psychical impression is thereby drawn within the field of psychological regard. That is why I lay stress on expression as, for our purpose, to be taken in its relation to impression. They are correlative terms in one field of relatedness. None the less the distinction between behaving with intention and behaving without intention is important. It must be steadily borne in mind. And this is the more necessary because so much of our own behaviour is in relation to foreseen ends, and because the consequences of our own acts are so often quite definitely and deliberately intended. But even with us surely not always. And in the case of animals purely instinctive acts (if such there be) and much of their behaviour, in so far as instinctive and unlearnt by the individual performer, afford, as I think, no evidence of an end in view such as intention involves. Professor Stout would say that a very vague and indefinite end in view is always present. Let it go at that. Practically (I do not say theoretically) there is little difference between what I mean by absence of intention and what he claims as present in so nebulous a form.

We must realise then that, in some measure in human life, and probably in far larger measure in animal life there may be expression which produces impression in the complete absence (for purposes of practical interpretation) of any intention to produce that impression; and that this is especially the case in so far as instinctive factors of behaviour are concerned. If it be granted therefore that apparently similar behaviour at one time warns off an intruder into the territory, at another time stimulates the pairing hunger of the hen bird, and at yet another time attracts the attention of an animal that too closely approaches the young, there need be no intention to warn, or to win the mate, or to draw off from the offspring, still less, in the last case, to delude by simulating helplessness. On my view there is no such intention so far as the behaviour is instinctive, though it may be present so far as acquired experience is co-operant. So too the impression received need be no more than provocative of responsive behaviour which the recipient feels like carrying out.

4.

Let us, then, take expression and impression as correlative; and let us assume that for behaviour to be expressive there need be no intention to make an impression on, say, some other bird, and that behaviour may instinctively be the outcome of impression without any intention so to behave. We may now ask how we can account for the fact that, so far as observation goes, the same or very similar expression in behaviour seems on different occasions to produce a different impression, or at any rate one which leads to very different behaviour on the part of the recipient. I do not think this presents much difficulty. In the first place the observations which Mr Howard describes are made on adult birds, and, in them, what we speak of as the impression carries meaning which has been acquired in the course of previous experience. Indeed it is only with some difficulty that we can realise that there may be impressions, and other presentations in naïve awareness, which have not put on the vesture of acquired meaning. And so far as such meaning is present, there is no difficulty whatever in understanding how closely similar impressions may be differentiated on different occasions. Let us however consider what is actually presented, say to vision, on the several occasions. Is it so similar as at first sight it may seem? By fixing our attention on the expressive attitudes and movements *we regard them in isolation from their context.* But as they are presented to the recipient of the impression they are nowise thus isolated. They are set in the context of the situation as a whole. More than we readily realise the animal, like the little child, is responsive rather to the total presented situation than to the details—even the salient details—which we, for the purposes of our thought, distinguish and isolate. We concentrate attention on the similarity of behaviour because that is what we seek to interpret; and yet we see clearly enough that in each of the three cases to which Mr Howard refers, the circumstances differ notwithstanding the similarity of expressive behaviour in their midst. The situations are clearly differentiated for *our* observation. Why should not the bird, after his naïver fashion, feel this difference and act accordingly? Why should not the context play its part in determining *his* behaviour? We must remember, too, that living through, and behaving in the midst of, a given situation begets an attitude of mind, a progressive differentiation of enjoyment, in relation to just that situation. The outlook of the bird is brought into harmony with that upon which he looks out. Not only therefore is there difference in the world context in which the expression finds its place, there is difference also in the mind context in which the impression finds its place. During the period of so-called courtship the male bird, and probably the female too, are, very markedly, as it seems, under the influence of sex-hunger, and we may well suppose that specialised interest colours the whole situation and gives at that time special meaning to expressive behaviour, which, seen at other times, would carry a different meaning.

5.

But how are we to account for the similarity of expression in different situations? Why should there be much the same behaviour when another male enters the breeding territory, when mating is in progress, and when the young are disturbed? In the first place it should be remembered that the modes of behaviour now in question are chiefly in evidence during the period when dispositions subsidiary to race-maintenance find expression in modes of action common to this period under the physiological influence of the internal secretions of the group of related glands in the organic sex-complex. If this common expression have diverse utility in different situations, like factors therein would be emphasized. The whole period is one of unusual excitement—*i.e.* emotion as super-normal—and it may be that the readiest outlet of the motor effects of this excitement is along certain predetermined channels. One is here reminded of Herbert Spencer's doctrine that an overflow of 'nervous force' takes first the most 'habitual' routes. And one knows that in the case he discussed in advocating this view[1] laughter may be the outcome of very different kinds of emotional excitement, often, as we commonly say, of mere high-strung nervous tension. It may be, therefore, that the similar behaviour in different situations is an index of a general type of excitement under a ruling disposition, and that we must, and practically do, infer the specific quality of the emotional affect rather from the context of the behaviour than from the behaviour itself. There may, too, be subtle differences which our powers of observation are not sufficiently delicate to enable us readily to distinguish—differences analogous to those between the genial laugh of good fellowship, the laugh of exultation over a fallen enemy, and the laugh of irritation when, say, a golfing opponent has his usual persistent and undeserved good luck. These differences we learn to appreciate because we too are human folk and grasp the nuances of the situation. May there not be differences which birds sense better than we do, just because they *are* birds? Such modes of expression as we are considering, in their relation to impressions received, whether they are really alike on different occasions, or really slightly diverse and only apparently the same, betoken, as has been said, periods of excitement. That excitement should find an outlet in vigorous behaviour has biological value and hence utility in a broad sense. It is a relief from tension and strain which might otherwise be harmful to the organism. If there be also specialised utility in affording an impression which stimulates another bird to behaviour appropriate to the situation, there do not seem to be insuperable difficulties which preclude an adequate interpretation of the facts.

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[1] *Essays,* I, 194.

6.

Let us now consider in further detail the bird's behaviour during the period of so-called courtship. It may be well first to say why I speak of *so-called* courtship. I take it that the word 'courtship' may naturally be held to imply intention on the part of the male so to act—so to sing, for example, or so to display his plumage—as to make himself attractive to the hen-bird he 'seeks to win' as his mate. Now of course such intention may be present. There may be in the mind of the male something of this sort: "By such and such expression an impression will be produced; and the production of this impression will contribute to the end I have in view." It may be vaguer than this—so vague that one can hardly put it in words; he may just realise in some dim but sufficient way that what he does has its effect and intends to do it for the sake of this effect. On the other hand it may be urged that what is *necessary* for an interpretation of the observed facts is less even than this. All that is required, on this view, is that, in accordance with his disposition at the time, the male feels like behaving in certain ways; that this behaviour does produce a stimulating impression; and that, on the receipt of this impression, the hen-bird, in accordance with her disposition at the time, feels like mating with him. We will provisionally assume that only this minimum is present so far as the instinctive factors in behaviour are concerned.

It may be said that no act in adult life *is* purely instinctive. When the bird has reached the age of 'courtship' there is always in addition to, and intimately combined with, the instinctive and unlearnt factor in behaviour, some legacy from the lessons which have already been learnt. That is so; and it is just this which renders interpretation peculiarly difficult. My point is that, so far as we can get at the facts by observation, there is in the male bird's procedure much that is seemingly unlearnt—much that seems just to come in presence of the 'courtship' situation when first it is presented. And I urge that, *so far as this unlearnt part is concerned*, intention so to act as to attain results of which there has been no prior experience, is presumably absent.

7.

We must now come to still closer quarters with some of the phenomena connected with the life of birds in the period of 'courtship.' In the Second Part of his great work on *The Descent of Man*, Darwin very fully discusses the evidence for his hypothesis of sexual selection as a factor in evolution. How does he differentiate it from natural selection or, as he sometimes puts it, from other forms of natural selection? It depends, he says, "on the advantage which certain individuals have over others of the same sex and species solely in respect of reproduction[1]." This advantage has specialised biological value to the end of mating, and hence there have been developed secondary sexual characters which in their present form are in part due to this selection.

Now it is clear that the secondary characters developed, as Darwin claims, through sexual selection, fall within two different sub-classes; (1) those which are not primarily dependent on the impression they make on the mate; (2) those which *are* thus dependent since they allure or excite an individual of the other sex. Those in the first category are of advantage in the struggle to secure a mate; but they are not necessarily of advantage through any appeal they make to that mate. It is not through her preference under psychical selection, that the best fighter wins. She may *also* prefer the winner; but she must often yield to the victor whether she prefers him to others or not. She must often yield to him who can capture and hold her. She is just a prize to the strong, and has, so to speak, no voice in the matter. But under the second category the advantage lies with the possessor of those organs and structures, used to the best effect, which so allure or excite the female as not merely to force but to induce her to mate.

Since from the point of view here taken the whole matter turns on the relation of expression to impression it may be well to consider briefly a simple but in some respects typical example in insect life. "The female of the Emperor moth (*Saturnia carpini*) is so eagerly sought by the males that when a virgin female is taken into a favourable locality, the collector is soon surrounded by troops of males which have been guided by a marvellously delicate sense of smell residing in their branching antennae. So delicate is the sense that the female is recognized perhaps miles away, and recognized as a virgin[2]." Now here the scent emitted by the female is a form of expression which produces a presentative impression on the males through receptors in the specially developed antennae. And there seems little doubt that the biological value of expression and impression here centres in, if it be not exclusively related to, reproduction. If this be so, the possession of specialised antennae may fairly be regarded as a secondary sexual character in the male. It will, no doubt, be said that there is here no evidence of preference on the part of the female; she does not select the male with the most branching antennae. Still the case does fall under Darwin's principle as stated by him; and it does fall under the second category, for it involves alluring and exciting an individual of the other sex. Now let us suppose that, of two females, one produced the expressive scent in greater abundance than the other. That one would have a greater and more far-reaching effect. Some distant male, perhaps with better developed antennae than others, would receive an impression from the one female and not from the other, or perhaps a stronger impression from the one than from the other. May we say that if he flies to the one and not the other he chooses her? And can we say that in any valid sense she chooses him? Some will say: most certainly not. Choice is choice; and this is not choice. It is simply behaving under the stimulating influence of what is assumed to be the stronger impression. Yes! And that, I venture to suggest, is just what Darwin meant (even in the much more complex cases of bird-life) by speaking of the females as 'unconsciously preferring' certain males which most strongly excite them. "It is not probable," he says, "that she consciously deliberates; but she is most excited or attracted by the most beautiful, or melodious or gallant males[1]." One must remember that at our human level of mental development, choice is for the most part purposeful. As we commonly use the word there is nearly always implied a foreseen end for the sake of which the choice is made. At the much lower stage of mental development we are trying to read ourselves into—with difficulty since we must leave so much of *ourselves* out of account—at this stage what we are forced by the exigencies of language, fashioned to deal with human affairs, to speak of as choice or preference, may be much more naïve—so naïve indeed as to be reduced to the level of the relation between adequate stimulation and adaptive response, if, but only if, we deliberately turn our gaze away from the correlated psychical aspect.

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[1] p. 322.

[2] Poulton, *Colours of Animals*, p. 288.

8.

Another verbal question—which like the others is really more than merely verbal—calls for passing comment; and then we can proceed on our way. In many passages Darwin speaks of a 'taste for the beautiful,' and uses the word 'aesthetic.' Thus he says "On the whole birds appear to be the most aesthetic of all animals, excepting of course man, and they have nearly the same taste for the beautiful as we have[2]." In what sense are we to understand 'taste'? The pilfering blackbird seems to have nearly the same taste for ripe fruit that we have. Is one misunderstanding Darwin if one supposes that he used the phrase 'taste for the beautiful' in an analogous sense? I take it that Darwin did not wish to raise subtle questions, but to indicate fairly obvious inferences from what may be observed as interpreted in the light of his hypothesis. Look up the word 'aesthetic' in some current dictionary. Chamber's *Twentieth Century Dictionary* lies at hand. There I find, *sub. verb.* "the feeling of beauty in objects; the principles of taste and of art; the philosophy of the fine arts." Which of these, thus or otherwise formulated, did Darwin mean? Surely, I venture to suggest, the first. What does the bird know of the principles of taste and of art? What of the philosophy of the fine arts? But I see no reason why we should not credit the Peahen, when the Peacock erects and spreads his splendid tail-coverts, with a qualification of enjoyment analogous to that which we too experience on sight of him, *so long as we leave it at that* and do not begin to discourse on aesthetic values, beauty for beauty's sake, and the rest. I may no doubt be reading out of Darwin part of what he really meant. Or it may be said that I am reading into him my own views. But he does seem to safeguard his position in many passages in just the way I venture to suggest. Take for example this one. "I refer only," he says, "to the pleasure given by certain colours, forms and sounds, which may fairly be called a sense of the beautiful. With cultivated men such sensations are, however, intimately associated with complex ideas and trains of thought. When we behold a male bird elaborately displaying his graceful plumes, or splendid colours before the female, whilst other birds, not thus decorated, make no such display, it is impossible to doubt that she admires the beauty of her male partner[1]." For the present it is enough to lay stress on some differentiating character of the presentation which leads to response. It is this which seems to be really essential. There is differentiation in expression; this produces differentiated impression; and this again leads to a responsive act. When this expression evokes response, while *that* does not, we infer from the response that the female 'chooses' *this* male in preference to *that* one. But the observed facts may be interpreted on the supposition that *this* as expression affords an adequate impression while *that* does not; and that true choice with an end in view is absent.

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[1] p. 643.

[2] p. 550.

9.

Let us revert for a moment to the Emperor moth. Prof. Poulton says, in the passage I quoted above, that so delicate is the sense of smell in the male that the female is recognized perhaps miles away and recognized as a virgin. This use of the word 'recognized' is quite common among naturalists. But this recognition is not that the nature of which is discussed by psychologists. The latter is generally held to imply prior cognition in the course of individual life. All that is implied in what is sometimes called instinctive recognition is such impression as shall lead to responsive behaviour.

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[1] p. 140.

It is well known that Wallace was unable to accept sexual selection. And it is well known how much stress he laid on 'recognition marks.' "Among birds," he says, "these recognition marks are especially numerous and suggestive. Species which inhabit open districts are usually protectively coloured; but they generally possess some distinctive markings for the purpose of being easily recognized by their kind, both when at rest and during flight[1]." "Among insects the principle of distinctive coloration for recognition has probably been at work in the production of the wonderful variety of colour and marking we find everywhere, more especially among the butterflies and moths; and here its chief function may have been to secure the pairing together of individuals of the same species[2]." He speaks of "the various sounds and odours which are peculiar to the male, and which serve as a call to the female or as an indication of his presence. These are evidently a valuable addition to the means of recognition of the two sexes;...and thus the clearness, loudness, and individuality of the song becomes a useful character, and therefore the subject of natural selection[3]." Now recognition marks, call notes, and song, are, in our terminology, modes of expression; and Wallace's contention turns upon the claim, not only that they give rise to impression, but that, under natural selection, their very existence as such depends upon this impression—otherwise how can they be regarded as recognition marks *evolved to this very end*? But they are useful to each species "in relation to the most important function of their lives[4]"—that of reproduction. And it is just in view of their relation to this function that Darwin places them in the category of sexual selection. It is in virtue of *this* that recognition marks to the end of pairing are differentiated from warning colouration to the end of survival—also the outcome of the relation between expression and impression. But Wallace went even further on his way with Darwin than I have yet indicated. "Anyone," he says, "who reads these most interesting chapters [of Darwin's] will admit that the fact of display is demonstrated; and it may also be admitted, as highly probable, that the female is pleased or excited by the display." That seems to give Darwin pretty nearly all that he claims.

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[1] *Darwinism*, p. 222. [2] p. 226. [3] pp. 283, 284. [4] p. 285.

I have sought to emphasize the measure of harmony that may be found in Darwin and Wallace in the light of the relation which obtains between expression and impression. The point of interest to the psychologist is that for both certain features of structure or behaviour appeal to the mind and that in the absence of this appeal these features would not be what they are and as they are. Their evolution depends on a psychical factor—on some impression which they produce. Regarding then expression as correlative to impression, it seems that both the biological and the psychological value of expression is dependent on the psychical factor I deal with under the name 'impression.'

The further biological problems do not here concern us. Wallace was not satisfied that differential appeal of expression through the impression it produces suffices to enable us adequately to interpret the origin of secondary sexual characters. That may well be so. But Darwin dealt rather with the selection than the origin of these characters in the sense in which Wallace discussed the matter. These problems are not in our present picture. This however may be added. Wallace was unable to believe "that all the females of a species, or a great majority of them, over a wide area of country, and for many successive generations, prefer exactly the same modification of the colour or ornament[1]." But he seems to have found no difficulty in believing that all the members of a species *recognize* the same modification of colour or of ornament. But recognition marks imply differential impression and differential response no less than secondary sexual characters.

10.

Now so far as the response is instinctive there is, on our interpretation, no intentional or deliberate choice on the part of the female. But what about the behaviour of the male? Few would be prepared to contend that beautiful plumage is an intentional expression on the part of its possessor. Indeed it may be said that only by an extended use of the word can it thus be spoken of as an expression. Still it must be remembered that the word is here used as correlative to an impression produced, and in this sense the extension of usage is justifiable. If once we disabuse our thought of the idea that expression must necessarily be intentional, there is no reason why the brilliant plumage of birds, the mimetic or warning colouration of insects, the nectar, scent, and conspicuous hues of flowers, should not be regarded as expressive, in so far as they lead to impressions received by sentient organisms, and in so far as the expression has in some manner been evolved in relation to the impression produced and the behaviour which results therefrom. What we may speak of as *structural expression*, then, is not commonly regarded as, in any degree, intended by its possessor. When, however, we pass to 'display' in behaviour the presence of intention is generally assumed. It is indeed this assumed presence of intention which has led to the popular emphasis on the *courtship* of animals.

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[1] p. 285.

One who differs from Darwin in his interpretation of animal life, does so at his peril. In this matter of display Darwin spoke with no uncertain note. "When we behold two males fighting for the possession of the female; or several male birds displaying their gorgeous plumage, and performing the strangest antics before an assembled body of females, we cannot doubt that, though led by instinct, they know what they are about, and consciously exert their mental and bodily powers[1]." The males "sedulously display their charms before the females[2]." They "endeavour to charm or excite their mates by love-notes, songs, and antics[3]." They "display their fine plumage and other ornaments with so much care before the females[4]." "Sufficient facts have now been given to show with what care male birds display their various charms, and this they do with the utmost skill. While preening their feathers they have frequent opportunities for admiring themselves, and of studying how best to exhibit their beauty. But as all the males of the same species display themselves in exactly the same manner, it appears that actions, at first perhaps intentional have become instinctive[5]." "Hence we may conclude that it is the object of the male to induce the female to pair with him, and for this purpose he tries to excite or charm her in various ways; and this is the opinion of all those who have carefully studied the habits of living birds[6]."

It is noteworthy that Darwin was generally careful to qualify his allusions to preference on the part of the females by using such a phrase as "prefer, or are unconsciously excited by the more beautiful males[7]." But with regard to the behaviour of the male he says roundly, on the same page, that "they display their colours with elaborate care and to the best effect." Still in one of the passages I have quoted he seems ready to admit that in so far as instinctive, the behaviour is no longer intentional. And we have on the other side the passage in which it is suggested that the male birds have opportunities, while preening their feathers, for admiring themselves, and for studying how best to exhibit their beauty. If this suggestion be accepted, the behaviour which is the outcome of this study is clearly so far learnt; data for the most effective manner of display are intelligently acquired. I cannot accept the suggestion even from Darwin. If we grant that the male engaged in the instinctive business of preening, is also admiring his fine feathers, what he sees in detail is very different from what the female will see when she views him as a whole from some distance. I regard it as extremely improbable that the male bird realises that what thus, *ex hypothesi*, pleases him in detail, will produce, under quite different circumstances, so favourable an impression on his desired mate as to induce her to accept him. I am unable to water down what is supposed to pass through his mind to much less, in essentials, than this; and this I think is quite beyond his powers of inference.

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[1] p. 325.
[2] p. 341.
[3] p. 561.
[4] p. 629; cf. pp. 600, 672.
[5] p. 612.
[6] p. 616.
[7] p. 757.

I submit that the elaborate display, executed in substantially the same manner by all the males of the same species, may fairly be regarded, complex as it undoubtedly is, as quite unintentional. Behaviour is no doubt in delicate harmony with ornamentation. Darwin compares the plumage and the display of the Peacock and the Peacock Pheasant[1]. The arrangement of the ocelli is such that effective sensory presentation involves 'display,' in the one case when the male stands in front of the female, erects and expands the tail-coverts, and at the same time 'shows off' his rich blue throat and breast; and in the other case when he stands obliquely to the female lowering the expanded wing on the near side and raising that on the far side. In this attitude, the tail-feathers being also expanded obliquely, "the ocelli over the whole body are exposed before the admiring female in one grand bespangled expanse." By such expression is an adequate impression produced. If, with Wallace, we regard the origin and the special character of the ornamentation as independent of psychical selection, are we to regard also its specific manner of display as independent of such selection? If, as those who deny even 'unconscious preference' on the part of the female must urge, the so-called display produces no impression that leads to response, how are we to account for the differentiated manner of display in this species and in that? If on the other hand it be intelligently learnt in the course of individual life, to subserve the foreseen end, is there a satisfactory observational basis on which is founded the inference that such a process of learning does take place? To revert to Darwin's suggestion that male birds are observed to preen their feathers. Is it likely that the Peacock or the Peacock Pheasant, in preening his feathers and admiring the ocelli thereon, reaches the conclusion that, as seen from some distance by the female, *this* mode of behaviour will display most effectively the specific ornamentation in the one case, and *that* in the other? Moreover it does not seem that the males of different species *have to learn* how to display their plumage to best advantage; this appears to come by nature. In other words the form of the behaviour as a whole appears, on the evidence, to be instinctive.

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[1] p. 605.

11.

Suppose, then, we regard it as a tenable view that the unlearnt basis of display is primarily instinctive, or in modern phrase a function of the unconscious; and suppose we regard the expressive display as giving rise to impressions on the part of the female; what then? How does the business work? "The courtship of animals," said Darwin, "is by no means so simple and short an affair as might be thought[1]"; and again, "the courtship is, in many instances, a prolonged affair[2]." Furthermore "as the illustrious Hunter long ago observed," the female "generally requires to be courted; she is coy, and may often be seen endeavouring for a long time to escape from the male[3]." Here we have, in the germ, the interpretation so ably developed by Gross in his *Play of Animals*. We must, however, be content to deal, and that briefly, with the root idea, which is that the coldness or coyness of the female (which surely involves a psychical factor) is only overcome by the adequate effect of strong expression on the part of the male; each being reciprocally developed in relation to the other. This view now meets with wide acceptance. But we must bear in mind that, for quite a long time, the male is always ready and eager to pair. Such is his disposition while the hormones course in his blood. Not so the female; her readiness, perhaps her eagerness, to pair, comes in periodic gushes, and is possibly correlated with the liberation of internal secretions when the ova are dehisced from the ovary. If this be so—if sexual excitement in the female be periodic—this must be taken into consideration. If, for example, for every ten times the always-eager male approaches his mate, only once is she physiologically attuned, and psychologically inclined, to receive his 'addresses,' we should call her cold or coy on the predominant evidence of the nine occasions though she may be extremely ardent on the tenth. I am disposed to believe that such

[1] p. 330. [2] p. 561. [3] p. 342.

evidence as we have is in favour of this view. But it may here be asked: If this be so, wherein lies the biological value of the persistent ardour of the male? In reply it may be said that, on the basis of general considerations, it seems advantageous that *one* of the partners should be ready at all times during the pairing season. For if each were ready only on some tenth occasion, pairing would depend on the chance coincidence of this tenth occasion in both sexes. But, it may still be said, if the so-called preference of the female mainly depends on her physiological state at the time, will she not then pair with *any* eager male that chances to be at hand? When the pairing hunger is insistent, will it not be satisfied by union with *this* mate as readily as by *that*? Perhaps in many cases this is so. The evidence alone can decide. In seasonally monogamous species, only her already 'chosen' mate is at hand—though if he be killed a successor is soon in evidence. But, to follow up the hunger analogy, does it follow that because hunger is a condition of taking food, the hungry animal will eat *anything*? Surely notwithstanding the presence of hunger, there may be, and commonly is, some 'preference' for *this* over *that*. May we not say that although a certain internal state (disposition) is a factor in every instinctive response, there is also required an adequate incentive to that response? Is it therefore unreasonable to suppose that even when the wave of pairing sex-hunger comes over the female, the 'display' of *this* male may, in some cases at least, afford an adequate expression, while the 'advances' of *that* male do not? If this supposition be justified by the evidence, then, in such cases, there is sexual selection. We must however remember that, if there be a process of sexual selection in intimate relation to other forms of natural selection, the outcome of its continuance for many generations would be to keep the expressional display of the male up to the level, or within a definite range, of adequacy. We do not meet with many failures under other forms of natural selection, just because such failures are so steadily eliminated. Ought we to expect to meet with many cases of lack of adequacy to afford the adequate impression for eliciting the pairing response? Their very absence may be a sign of the stringency of a selective process.

12.

I quite realise that some objection may be raised to my use of the word 'expression.' Why limit it to that which is correlative to impression? Well, why not, for the purpose in hand, if it serves to elucidate a point of view? That point of view is that what I have called impression is psychical in its nature, a way *in* which the bird or other organism is conscious, in the broad sense of this word; and that if expression is (under definition) correlative to impression, then there is a valid sense in which it may be said that the genesis of expression depends on—or, if it be preferred, has been evolved for the sake of—the impression it produces. If this be so, the importance of psychical selection receives due emphasis.

Part of my aim has been to show the intimate connexion of biological and psychological values though the latter are, in the cases discussed, at an early stage of their development. I wish to protest against an undue sundering of natural and psychical selection. Why should not both be regarded as fundamentally natural, our object being to correlate diverse aspects of that which is found in the given nature of one reality? This does not of course preclude the drawing of distinctions. In psychical selection there is preferential appeal of the best expression; under other forms of natural selection there is differential weeding out, the incidence of which falls initially on the worst individuals—those which are bionomically deficient. Psychical selection works from above downwards; natural elimination works from below upwards. But the net result in either case is the preservation of efficiency.

Apart from this it is, I think, only under a misapprehension that sexual selection is regarded as an alternative to natural selection. It is in truth a differentiated form of natural selection, dependent, according to Darwin on exclusive relation to reproduction. Darwin does, indeed, contrast sexual and natural selection, qualifying the latter, in one passage[1] by speaking of "ordinary or natural selection." But one is not, I think, departing from the spirit of his teaching, if one speaks of sexual selection as a differentiated form of natural selection. Let us assume, then, that sexual selection is analytically distinguishable from other forms of natural selection in and through its exclusive relation to reproduction; and that under the heading of sexual selection two sections may be distinguished; (*a*) that which includes those modes of elimination that do *not* involve what I have called the relation of expression to impression; (*b*) that which includes those modes of selection which *do* involve, in some form, this relation. And let us go a step further, and distinguish, under (*b*), two sub-sections; (*α*) that which includes those modes of expression and impression that are only indirectly related to the pairing act, which we may speak of, in monogamous species, as securing a mate; (β) that which includes those that are *directly* related to the pairing act. We then have the following scheme under which we may conveniently classify the observed phenomena.

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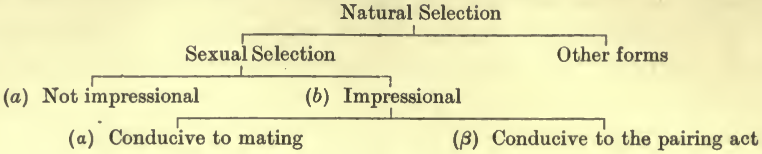
[1] p. 349.



(*Manuscript received* 31 *October* 1920.)

THE NATURE OF VERSE.

By E. W. SCRIPTURE.

I. *The controversy originating this inquiry. 'Quantity verse' and 'stress verse'* (pp. 225, 226).
II. *Apparatus employed and analysis of the record obtained. 'Enunciation verse' and 'melody verse'* (pp. 226–231).
III. *Conclusions as regards the controversy. Their general validity* (pp. 231–233).
IV. *Verse as a product of human activity* (p. 233).
V. *Fallacy of the 'metric' theory of versification* (pp. 233–235).

I.

In a dispute which arose between two professors of Greek at Harvard and Yale Universities concerning the nature of Greek verse, they ultimately had recourse to their own experiences in speaking English verse. The former professor maintained that anybody could hear that English verse consisted of long and short syllables, whereas the latter protested that anybody with ears knew at once that it consisted of loud and weak ones. The difference between their views may be illustrated in a somewhat exaggerated form as follows. The Harvard professor would say that the line

Somebody said that it couldn't be done

really sounded somewhat like this:

So-o-ombody sa-a-aid that it co-o-ouldn't be do-o-one.

Whereas the Yale professor would say it was like this:

Somebody said that it couldn't be done.

In the first case four of the syllables are made longer, in the second they are made louder. The first is called 'quantity verse,' or verse of long and short syllables. The second is called 'stress verse,' or verse of strong and weak syllables.

Neither the two professors nor their audiences at various philological meetings came to any agreement. Finally the Yale professor appealed to the Psychological Laboratory to devise some apparatus that would settle the controversy. This was the starting point of certain investigations on speech which have continued ever since. The methods first used were very elaborate and expensive, but it is now possible to register verse in such a way that the question of its nature can be answered with certainty and accuracy without great labour.

II.

The apparatus now used is shown in Pl. I, Fig. 1. The person speaks into a comfortable mouthpiece. The vibrations and puffs of air from the mouth pass down the wide tube to the recorder at the end. This recorder is of the form devised by the Abbé Rousselot of the Collège de France. Instead of the rubber membrane I use one of oiled cloth that responds quickly to every impulse of air and has no vibration of its own. This membrane has a latent time so small as to be negligible and is 'dead beat' like a galvanometer with damping. The movements of the membrane are enlarged by a light lever and registered on a surface of smoked paper around a revolving drum. After the sheet of paper has been filled, it is removed, passed through varnish and dried. It can then be studied at leisure. Before or after a record a tuning fork is applied to the drum, drawing a wavy line in which each wave indicates $\frac{1}{100}$ of a second: measurements of time to the thousandth of a second can thus be made. This particular drum has its speed fixed so that it is necessary only to test it from time to time.

An inscription of "Somebody said that it couldn't be done" is reproduced in Pl. I, Fig. 2. The first thing to be noticed is that it is a continuous movement. This spoken verse is a smooth gesture without breaks. It does not resemble in any way the printed verse consisting of a series of independent types separated by spaces. In the typed line there are six large spaces between what are called separate words, and a number of smaller spaces between what are called separate letters. In this particular inscription there are no spaces of any kind. This does not mean that spoken verse may not contain pauses. Pauses do occur with the greatest frequency as part of the expression; every pause of even a hundredth of a second is clearly indicated in an inscription. On this particular occasion there was no pause.

Similar sounds give similar inscriptions. Those that have an emission of air, such as *s*, raise the line of the record. Those that have a complete stoppage of the breath, such as the usual *b*, register as a line on the lowest level. Those that have a sharp explosion, such as the usual *b*, produce a sharp upward jerk of the line. Experience with inscriptions makes it easy to pick out the parts belonging to the different sounds.

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PLATE I

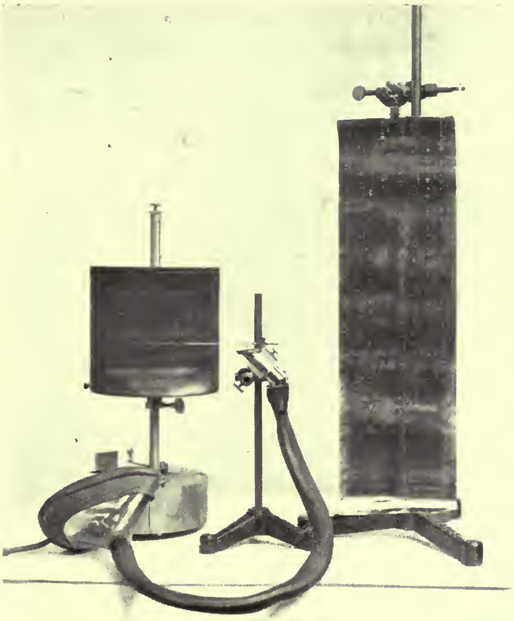

Fig. 1. Apparatus for making inscriptions of speech.

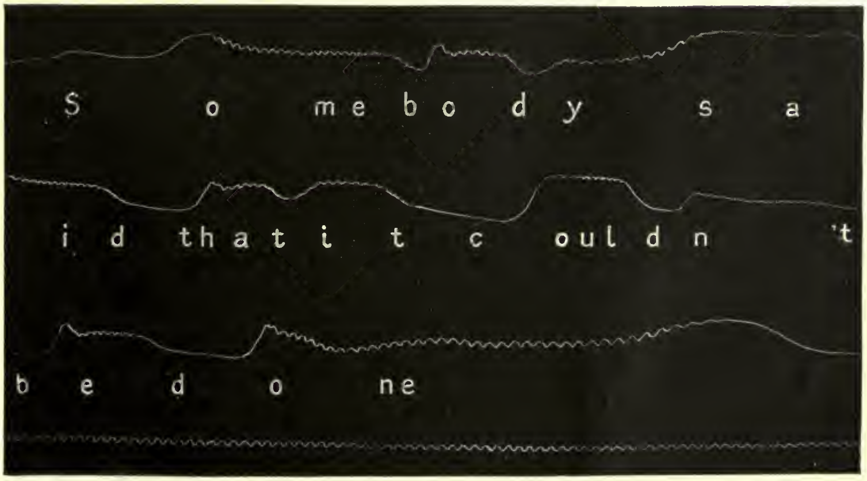

Fig. 2. Inscription of "Somebody said that it couldn't be done."

The inscription in Fig. 2 begins with a rising line for the sound *s* which consists of a current of air forced through a small opening between the tongue and the palate. A somewhat similar raised line is seen for the *s* of *said*.

The small waves are registrations of the vibrations of the voice. They are larger for the vowels, somewhat fainter for the *m* and the first *n* and still fainter for *b* and *d*. They are very large for the final *n*. To avoid misunderstanding due to spelling, it must be remembered that the *e* of *some* is a superfluous letter for which there is no sound. The same is true of the *e* of *done*; the sentence ends with the sound of *n*. The *th* of *that* is not a double sound but a single one for which modern English possesses no letter because it has thrown away the old Anglo-Saxon one.

A fall of the line indicates that the current of air is being cut off. In the first *b* the line falls slowly; this shows that the lips close gradually in making this sound. When they open, the air escapes as a puff or explosion and the line is sharply blown up. This *b* is not a typical carefully made *b*; the line should have fallen quickly and remained on the base level till the upward jerk began. The first *d* is even more carelessly made; the tongue cuts off the breath gradually and lets it out again with practically no explosion. The last *b* and the three other cases of *d* are somewhat better made, but the only really careful one is the *d* of *done*. The *d* of *said* is followed by a rush of air for *th*; it does not explode separately because the tongue simply changes its position in passing from the one sound to the other. The *t* at the end of *that* is very carelessly made. Moreover it has small waves that show the larynx to be vibrating, although in a properly made *t* this should not occur. The reason is clear. This *t* is between two vowels; the larynx does not take the trouble to stop, just as a motor omnibus slows up in the hope that the passenger will get on while it is in motion and thus save the omnibus the trouble and expense of stopping.

It is indeed quite evident that some of the sounds in this inscription are carefully made while others are spoken carelessly. The usual *s* registers as a smooth raised line. In the first *s* in Fig. 2 the line remains low for a while, showing that the tongue was so tightly pressed against the top of the mouth that the opening was a very narrow one. The upward jerk at the end, almost like an explosion, shows that the tongue was suddenly released. The second *s* shows some similarity to the first one. These two examples of *s* are made with excessive carefulness. Let us go over the consonants in succession. The first *s* is careful. The next *b* and *d* are careless. The *s* of *said* is careful. The following *th*, *t* and *t* are careless. The *c* is careful; so is the following *d*. The next *t* and *b* are run carelessly together. The last *d* is careful.

There are hence four regions in which the consonants are carefully made, alternating with three regions in which they are slurred over. Careful enunciation means more energy put into the organs of speech. This line therefore has four regions of increased energy of enunciation.

Energy of enunciation has nothing to do with loudness. Weak speech may be very carefully enunciated; loud speech may be quite careless.

We have thus at the outset discovered an entirely new principle of rhythm; we are quite justified in saying that this verse is an 'enunciation verse.'

It will be noticed that the line of the inscription rises higher in the regions around *said*, *could-* and *-ne*. There are thus at least three regions of greater intensity or stress separated by regions of weakness. This verse is quite evidently a stress or intensity verse. The Yale professor was therefore quite right.

Some of the sounds in Fig. 2 look longer than others. To get the lengths accurately the record sheet is placed under a microscope with a measuring scale. The speed at which the drum revolves is known. The measurements by the microscope can therefore be turned into time. For example, if the measurement on the record sheet shows that a sound has a length of 22 mm., and if the drum has been revolving at a rate of 1 mm. = 0·0057 sec., then the duration of the sound is

$$0{\cdot}0057 \text{ sec.} \times 22 = 0{\cdot}1254 \text{ sec.}$$

Although one sound passes without break into the next one, an attempt has been made to assign limits between them. The limits are somewhat arbitrary, yet they enable us to get measurements of the approximate lengths of time occupied by the sounds. The durations, or lengths, of the single sounds are given in the duration chart in Fig. 3. This chart shows a long *n* at the end but no other very long sound. The *o* of *Some-* might be expected to be long and the *y* of *-body* to be short, but the inscription shows the *o* to be shorter than the *y*. Similarly the vowel *ou* of *could-* is shorter than the *n* that follows.

The conclusion must not be drawn that the verse does not consist of long and short sounds. It is quite true that it does not consist of syllables each containing a long vowel alternating with syllables each containing a short vowel. The duration chart shows, however, that there are four regions where the sounds are longer alternating with regions where they are shorter. The first region of long sounds includes *So-*; the second includes *-y sai-*; the third includes *couldn'-*; and the fourth the word *done.* The verse is evidently a quantity verse composed of regions of long sounds alternating with regions of short sounds. The Harvard professor also was quite right.

Each of the small waves in the inscription records one vibration of the voice tone from the larynx. A long wave is the record of a low tone, a short wave of a high one. The horizontal length of each wave is measured under the microscope and the pitch of the voice is calculated for each instant.

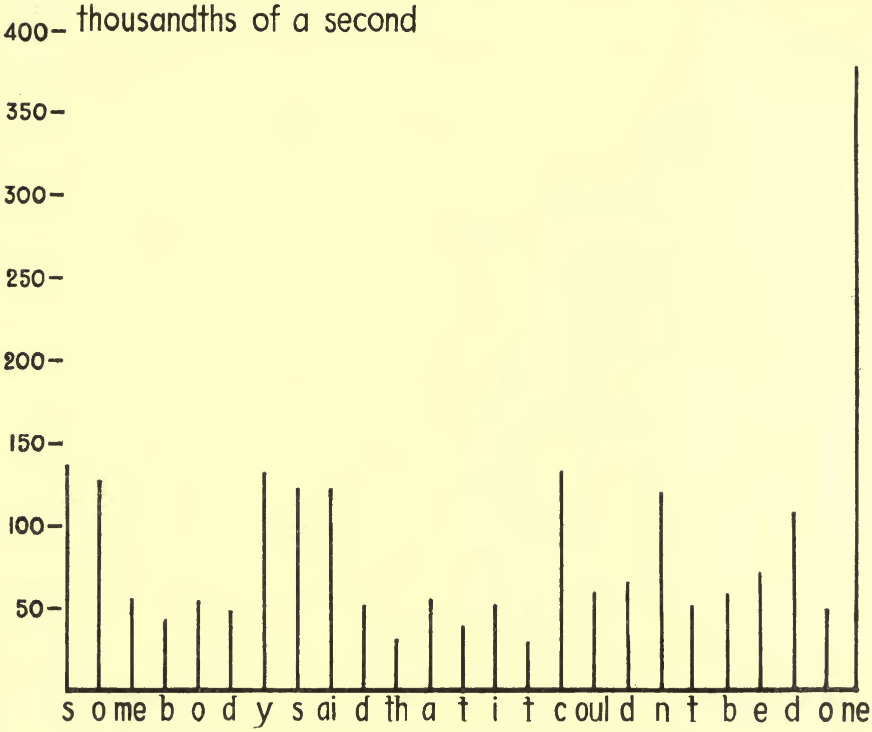

Fig. 3. Duration chart for the inscription in Fig. 2.

The inscription in Fig. 2 was taken at a speed of 1 mm. = 0·0057 sec. The first wave of the vowel *o* measures 1·4 mm. Its duration is therefore 0·0057 sec. × 1·4 = 0·00798 sec.

The pitch of a tone corresponds to the number of vibrations in one second. If a vibration lasts 0·01 sec. then the pitch will be 1 ÷ 0·01 = 100. For a vibration with the duration 0·00798 sec. the pitch will be

$$1 \div 0{\cdot}00798 = 125.$$

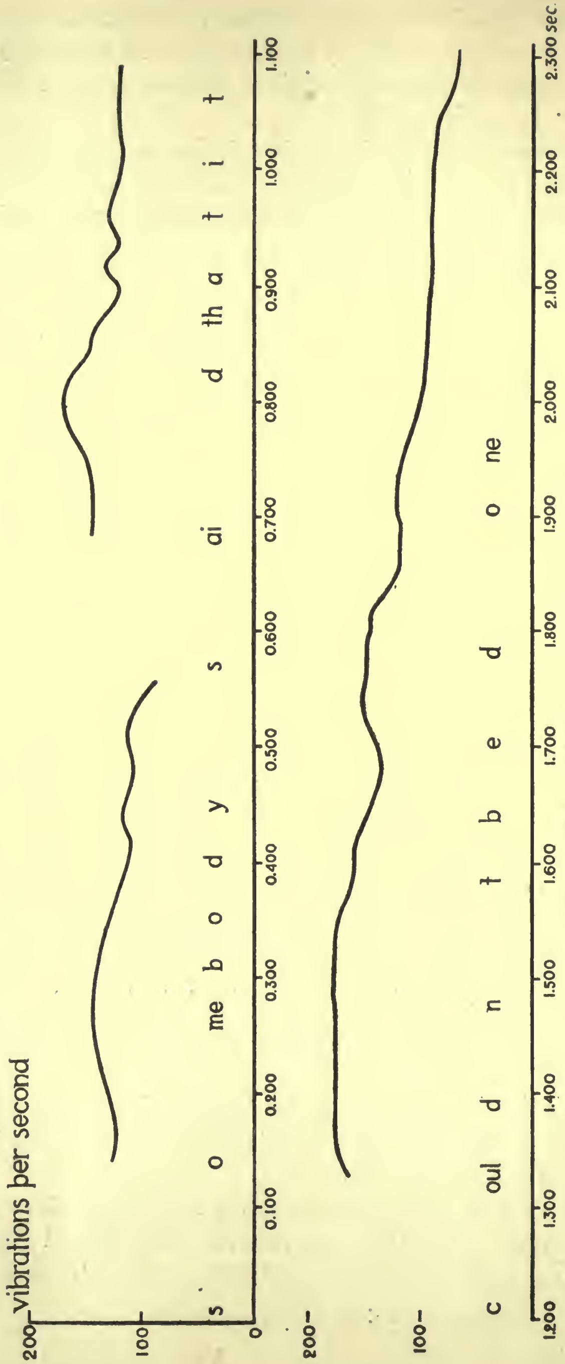

Fig. 4. Melody Plot for the inscription in Fig. 2.

When the pitch of the tone corresponding to each vibration has been calculated, the inscription is supposed to be laid along a horizontal line. Above each wave a dot is placed corresponding to the pitch. A line is drawn through the dots. The result for Fig. 2 is given in Fig. 4. This line indicates how the voice rises and falls during the sentence. It may be called the 'melody plot.'

The voice rises during *-ome*; it starts high and rises still higher in *-ai-*; it starts high and remains very high in *-ouldn'-*. Between these high regions there are regions of low pitch. A regular rhythmic effect is given by the alternations of high and low pitch. The verse is therefore a 'melody verse.'

III.

Each of the professors was quite right as far as he went. English verse is a quantity (long and short) verse such as Latin verse is supposed to have been. It is also a stress (loud and weak) verse such as Anglo-Saxon is supposed to have been. Neither of them thought that it might be a melody verse, as some persons have asserted that Greek verse was. An enunciation verse was something yet undreamed of.

Wherein they were both wrong was in the method they employed. Each appealed to the ear. But anybody with a prejudice can be made to hear anything; all that is needed is to create the proper expectation. Consequently the Yale professor was more nearly right; for ultimately he appealed to experimental methods.

The professors might be either right or wrong from another aspect. They assumed that all human beings are much alike, that the rhythmic instinct of the Greeks did not differ essentially from ours, and that therefore what is true of English verse might be true of Greek verse. Several problems are involved in this assumption. In the first place, are the principles found for the line of verse in Fig. 2 valid for English verse in general? In the next place, are they valid for the verse of other languages?

As regards the problem whether the above principles are generally valid for English verse, the following data are at hand at the present time. In the first place a study, just finished, of "To be, or not to be," gives exactly the same results as those above described. Studies—yet unfinished—of "When that Aprile with his showeres soote" and "Beowulf was braeme, blaed wide sprang" (spoken by a person of the present day) lead to the same results. A study of the sentences "Fred is sleepy," "*Fred* is sleepy," "Fred *is* sleepy" and "Fred is *sleepy*" shows that the emphatic words get their effect from one, two, three or all the elements of better enunciation, greater intensity, longer duration or higher pitch. The same conclusions have been drawn from studies of the inscriptions of the interrogative sentences: "Is Fred sleepy?" "*Is* Fred sleepy?" etc. and from the exclamatory sentences: "What a sleepy Fred!" "*What* a sleepy Fred!" etc. Not only that; the more emphatic parts of a simple sentence such as "Fred is sleepy" use one or more or all the four elements to distinguish them from the unemphatic parts. Numerous other English sentences have been studied in the same way with the same results. In fact, what has been stated above as the law of English verse is really the law of English speech in both prose and verse, namely—the stronger parts of a phrase or a sentence obtain their effect by the use of one or more of the four elements of better enunciation, greater loudness, longer duration or higher pitch.

How then does English verse differ from prose? Studies of inscriptions of "Thus I pass by and die" as prose and as verse show that in prose the waves of emphasis do not recur regularly whereas in verse they correspond to a regular rhythm such as the movements in walking or dancing. The discussion of the nature of prose must be reserved for some other occasion. It may be pointed out, however, that some so-called prose is really verse even if it is not divided up into lines. I have read through long passages in Shakespeare's plays, where the print indicates prose, without being able to pick out a sentence that was not as perfectly rhythmical as his verse. On the other hand, there are not a few poets wholly lacking in rhythmical sense whose 'poems' are really nothing more than unrhythmical prose cut up into pieces and printed in lines and stanzas so as to delude the public into believing them to be verse.

Do these principles of verse apply to all English people? There can be no doubt about the answer. All English people walk, bicycle, dance, and perform other rhythmic movements in about the same way. Some do the acts skilfully and some awkwardly, yet the principles are the same.

Do these principles apply to other languages? At present I have only the following data. A study of inscriptions of "Ein Fichtenbaum steht einsam im Norden auf kahler Höh'" spoken by thirteen persons from different parts of Germany showed that all of them used the same melody although they came from regions where the melody of ordinary speech differed greatly. Of course, this was the result of their education in speaking the language of their culture, namely, modern high German. No matter how differently they spoke at home, they all spoke nearly alike in the matter of literature. The law of variations in melody as an element of verse was the same for all. No observations were made on the other elements. We can anticipate, however, that the same principles apply to all verse for the reason that all human beings act on roughly the same lines. In one language the intensity element may be the most prominent, in another the duration element may play a more important rôle, etc.

For Latin verse the same principle must apply. We cannot doubt that the syllables were in general long and short as we were taught in school. But now we cannot doubt that the so-called long syllables were often also louder, more precisely enunciated and higher in pitch. It is quite impossible to believe that a Roman could, even if he tried, say the words "Arma virumque cano" all in exactly the same pitch, with exactly the same intensity and with exactly the same carefulness. An organ might produce tones in that way, but such a demand on a Roman would be quite inhuman.

IV.

Verse is a product of human activity and must conform to the laws of the psychology of action and conduct.

In the first place, spoken verse is a course of continuous action, not a series of disconnected jerks. Except where jerks are purposely introduced spoken verse is a smooth flow of sound.

In the second place, spoken verse is produced with a rhythm or beat effect. To the continuity this adds the effect of a regularly recurring impulse. Here again there is nothing jerky or interrupted. The beat effect is not like that of sudden taps separated by silences; no human activity is ever of this nature. The foot strikes the floor at regular moments in dancing, but these taps are only phases in a continuous movement. The manner in which the beat effect is produced in verse is explained above.

In the third place, the verse of a stanza or a poem is a continuous whole. There are beat effects of superior order that characterize the lines, and still superior ones that characterize the stanzas. The superior beat effects are produced by pauses, alliterations, rhymes, etc.

V.

We must rid ourselves of certain erroneous notions derived from printed verse. Verse existed long before type was invented.

With the spread of writing and printing there has arisen a new method of communicating verse, although a very imperfect one. To indicate the superior beat effects, the verse is printed in stanzas; the reader naturally makes a pause between them. A somewhat less superior beat effect is produced by printing the verse in lines; it is expected that a slight pause shall be made at the end of each line. The beat effect of blank verse is much injured if it is read without this slight pause. When rhyme is present, the line is usually made to end with the rhyming sound.

This is absolutely all there is to be said about *spoken* verse and its representation in print. Unfortunately students of verse have taken the *printed* verse as a basis and have built upon it a theory of versification called 'metric.' The characteristics of printed type have led them into a realm of fantastic error that has no counterpart in reality.

Printed verse consists of a series of letters. It is quite natural and easy to divide such a line of type into 'feet.' But, as we have seen, spoken verse cannot be so divided; there are no feet in spoken verse. The verse-typist would divide the speech of Fig. 2 as follows:

Somebody / said that it / couldn't be / done.

If any one attempted to say the words in that way, the result would not be verse at all. The absurdity is still more manifest when we note that the type-lover groups the words *that it* with *said* whereas they belong to *couldn't* and that he connects *be* with *couldn't* whereas it belongs at least equally to *done.*

The typist goes still further and divides the feet into smaller parts called syllables. He would mark them off thus:

Some-bod-y / said-that-it / could-n't-be / done.

Not the faintest trace of any such division can be found in Fig. 2. Moreover any such division into parts is quite contrary to the continuity of speech. If an attempt were made to speak the verse in Fig. 2 with division into syllables, it would cease to be verse or even true speech.

It is so easy to dissect printed type that we cannot wonder that the typist should divide words into their smallest elements. But we must object to his assumption that in so doing he has divided spoken language into its elements. When he speaks of single sounds, we must correct him into saying single letters or groups of letters. But his single letters do not represent single sounds; in continuous speech there are no single sounds.

With his notions of syllables and feet the typist proceeds to analyse printed verse. He erects whole systems of trochee, dactyl, iambus, anapæst, etc. A long syllable followed by a short one in a foot he calls a trochee. A long followed by two shorts is a dactyl. A short followed by a long is an iambus; two shorts followed by a long is an anapæst.

Much printed verse can be cut up on such principles. The printed verse corresponding to Fig. 2 he would cut up thus:

Sōmebŏdў / sa͞id thăt ĭt / co͞uldn̆'t bĕ / dōne.

He would describe it as a dactylic verse of four feet with the last foot 'checked,' that is, incomplete.

But what can the typist do with the many poems constructed like Kipling's *Cities*? On the basis of long and short he would print

Thĕ Cītĭes ăre fūll ŏf prīde
Chăllĕngĭng e͞ach tŏ e͞ach.

How are these lines to be divided into feet? Is a line composed of iambics and anapæsts thus:

Thĕ Cīt/ĭes ăre fūll / ŏf prīde,

or is it composed of trochees and dactyls thus:

Chăllĕngĭng / e͞ach tŏ / e͞ach?

Neither system suits both lines. The typist may try to escape from the difficulty by saying that it is trochaic with an accessory syllable placed in front of the first line for purposes of effect or perhaps attributable to the poet's laziness or carelessness about his 'metre.' In fact, the typist has to build up a most complicated machinery of extra syllables, decapitations, resolutions, etc., in order to force English verse into any of his systems. It can never be made to fit smoothly or permanently. One recent writer claims great credit for showing that Shakespeare's blank verse is not iambic as is commonly supposed, but really trochaic with various accessories.

It ought to have been evident long ago that such a manner of treating verse must be quite wrong. The poet knows little or nothing about 'metric,' and the less he knows the better verse he writes. The public who reads or hears his verses likewise knows nothing about 'metric.' The poet feels a rhythmic swing to the verse as he speaks or writes it, and the public feels it as the poet intends. The study of speech inscriptions has shown in what this swing consists. Neither for the poet nor for the public, nor for the scientific analysis of the verse, does anything exist beyond what this study has shown. Metre has been defined by one of its advocates as the quantitative framework on which verse is constructed. More truthfully, metre can be defined as a purely artificial system of treating printed verse with which spoken verse has nothing to do. The whole system is reminiscent of the mediæval dialectics which must be dispelled before there can be any approach to the truth.

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EXPERIMENTS ON MEMORY AND AFFECTIVE TONE.

By W. WHATELY SMITH.

(*From the Cambridge Psychological Laboratory.*)

A. *Objects of the experiments* (pp. 236–237).
B. *Technique and procedure* (pp. 237–242).
  (i) *Methods of measuring affective value.*
  (ii) *Method of measuring memory value.*
C. *Results* (pp. 242–247).
D. *Conclusions* (pp. 247–250).

A. OBJECTS OF THE EXPERIMENTS.

THE experiments described below were undertaken with the object of investigating the influence of affective tone on the process of memory by as quantitative and unambiguous a method as possible.

It is not unusual to find in psychological textbooks statements to the effect that some such influence is exerted; but these statements are not, as a rule, supported by anything more scientifically precise than an appeal to common experience. It is suggested that we tend to remember 'pleasant' things and to forget 'unpleasant' ones, and it is pointed out that we are much more apt to forget tedious and disagreeable appointments, for example, than those which promise interest and pleasure. This may be true enough, but it is equally a matter of common experience that there are many intensely distressing experiences which we find it only too difficult to forget and which insistently obtrude themselves into our memories. Common experience is, in fact, as unsatisfactory and conflicting in this connexion as in most other cases. It is also too vague to be of much scientific value.

Evidence of a much more scientific nature has been brought forward by Freud and others of his school (cp. Freud's *Psychopathology of Everyday Life*). He has found that in many cases where certain special items of experience, such as proper names, numbers, etc., which one would expect to be remembered easily, are in fact forgotten or distorted, it is possible to trace an associative connexion between them and some distressing or disagreeable experience or 'idea' of a nature calculated to provoke mental conflict—they are, in fact, associated with 'complexes.'

From this he concludes that such cases of forgetfulness are special instances of the general process of 'repression.'

The reasons advanced in support of this view have, however, been criticized on various grounds; as, for instance, that they are in no way quantitative, that no *measurement* either of 'affective value' or 'memory value' is made in respect of the forgotten material; that the associations noted may themselves be to some considerable extent determined by the observer's interest in the subject and his views on the cause of forgetfulness; and that the material dealt with is of a somewhat highly specialised type.

I therefore felt that it would be worth while to investigate the question by a purely objective method from which these sources of error were eliminated.

B. TECHNIQUE AND PROCEDURE.

(i) I proceeded to attempt to measure, quantitatively, the amount of affective tone excited in different subjects by certain words, and also the ease with which these words were retained in memory, thereby obtaining data amenable to mathematical treatment.

The procedure was as follows:—

I applied to each of 50 subjects (36 men, 14 women) a word-association test consisting of 100 words.

The same list was used throughout except that after three subjects had been examined nine words were altered, in order slightly to increase the range of interests covered by the list. Otherwise the words used were substantially the same as those given by Dr Eder in the introduction to his translation of Jung's *Studies in Word Association.* The final list used is given in Table I (p. 238).

In addition to these, five 'practice' words (Hand, Chain, Face, Run, Egg) were given to each subject before starting on the main list. These were intended to enable the experimenter to make sure that the subject properly understood what was required of him, and to reduce, so far as possible, the effects on the early words of the main series of any initial embarrassment or agitation which the subject might feel.

Three different methods were used for detecting and estimating the affective tone evoked by the words:

I. The Psycho-galvanic reflex. (All subjects.)

II. The Reaction-time measured in fifths of seconds. (49 subjects.)

III. Jung's Reproduction test. (22 subjects.)

TABLE I.

1. Head
2. Green
3. Water
4. Sing
5. Dead
6. Long
7. Ship
8. Make
9. Woman
10. Friend
11. Cook
12. Ask
13. Cold
14. Stalk
15. Dance
16. Village
17. Pond
18. Sick
19. Proud
20. Bring
21. Ink
22. Angry
23. Needle
24. Swim
25. Go
26. Blue
27. Lamp
28. Carry
29. Bread
30. Rich
31. Tree
32. Jump
33. Pity
34. Yellow
35. Street
36. Bury
37. Salt
38. Dress
39. Habit
40. Pray
41. Money
42. Silly
43. Book
44. Despise
45. Finger
46. War
47. Bird
48. Walk
49. Paper
50. Wicked
51. Frog
52. Try
53. Hunger
54. White
55. Child
56. Speak
57. Pencil
58. Sad
59. Plum
60. Marry
61. Home
62. Nasty
63. Glass
64. Fight
65. Wine
66. Big
67. Carrot
68. Give
69. Doctor
70. Travel
71. Flower
72. Beat
73. Box
74. Old
75. Family
76. Wait
77. Cow
78. Name
79. Luck
80. Horse
81. Table
82. Work
83. Brother
84. Afraid
85. Love
86. Chair
87. Worry
88. Kiss
89. Motor
90. Clean
91. Bag
92. Choice
93. Bed
94. State
95. Happy
96. Shut
97. Wound
98. Evil
99. Divorce
100. Insult

Nothing need be said about the two last, but it may be of interest to give a few details about the first, which proved by far the most delicate and valuable test of affective tone.

The arrangement of apparatus was as follows:

A Wheatstone's bridge was used in conjunction with a D'Arsonval galvanometer (resistance 92 ohms) and the resistances and connexions were as shown diagrammatically in Fig. 1. The electrodes, E_1, E_2 consisted of zinc-plate discs about 2 inches in diameter, covered with wash-leather and soaked in concentrated solution of common salt. They were always applied to the dorsal and palmar surfaces of the subject's left hand, and the hand, electrodes, wool pads, etc., were strapped firmly to a wooden splint in order to ensure a firm and satisfactory contact. The galvanometer was used in conjunction with a lamp and a celluloid scale graduated in millimeters.

A 10-ohm shunt was connected across the terminals of the galvanometer and a two-volt cell applied to the bridge.

The general objects and nature of the experiment were briefly explained to each subject before starting operations; he was then seated in a comfortable easy chair in such a position that he could see neither the experimenter nor the apparatus, the electrodes were applied and the patient's resistance balanced on the bridge. The necessary particulars of name, age, etc., were then taken, the five practice words given and the bridge was rebalanced. The experiment proper then began and, unless the subject's resistance increased or decreased so much that the spot of light moved off the scale altogether, the bridge was not readjusted during the experiment. (In those cases when readjustment proved necessary suitable corrections have been made in the subsequent computations.)

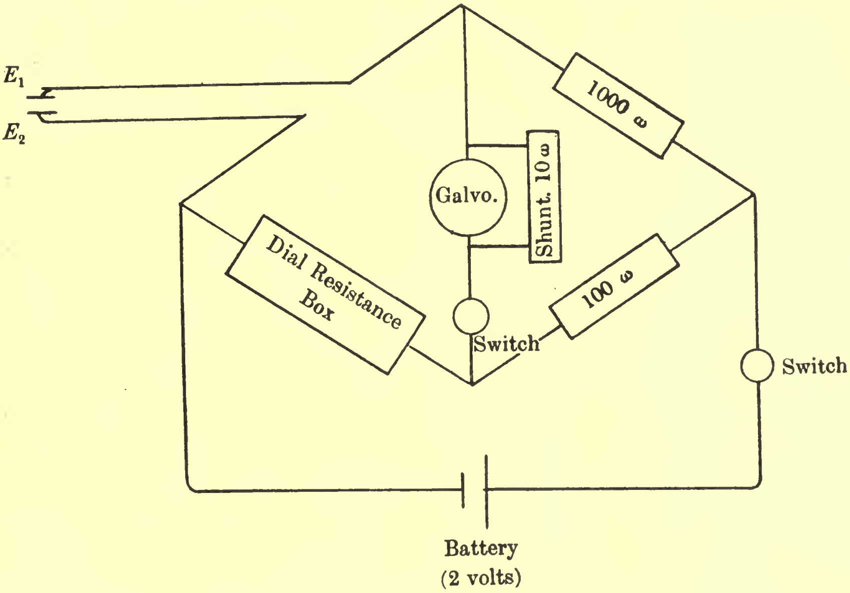

Fig. 1.

The data actually recorded for each reaction were:

(*a*) No. of the word in the list.

(*b*) Subject's response.

(*c*) Reaction-time: in $\frac{1}{5}$ seconds.

(*d*) Initial position of the spot of light on the scale at the beginning of its excursion.

(*e*) The position on the scale reached by the spot of light at the end of its excursion. (Galvanometric deflections were read to the nearest mm.)

(*f*) The difference between (*e*) and (*d*). (This was inserted later.)

(*g*) Whether the reproduction of the association was right or wrong.

A typical line of the record would thus read:

No.	Response	R.T.	From	To	Difference	Reproduction
9	Man	8	63	75	12	(Right)

In general the stimulus words were called out at approximately equal intervals, so far as was compatible with allowing the galvanometer to come substantially to rest between each excursion.

In the comparatively rare cases in which bodily movements or other causes prevented the movements of the galvanometer being read with reasonable precision, no entry was made under the 4th, 5th and 6th heads shown in the above record. These effects of bodily movements were easily recognisable, for the true reaction is always preceded by a latent period, whereas bodily movements which affect the mechanical conditions of contact produce their effect immediately. A very short experience enables the experimenter to distinguish infallibly between the two kinds of deflexion.

It was found that different subjects varied considerably in the reliability of the records it was possible to obtain from them. Some gave clear, regular, well-defined deflexions; others were much more erratic. In order to reduce, so far as possible, any accidental errors which might arise from this source, each set of observations was assigned a 'weight' according to the experimenter's estimate of their reliability. Observations of first-class reliability were weighted as 6, second-class as 5 and third-class as 4. Two exceptionally erratic subjects were weighted as 2 only. The weights were incorporated in the subsequent calculations in the usual way. This weighting was not applied to reaction times.

In order to make the deflexions given by different subjects as comparable as possible, I calculated the mean resistance of each subject by adding together the initial readings of the galvanometer for each reaction and dividing the sum by the number of reactions (normally 100). This gave the mean initial position of the galvanometer for each subject, from which, knowing the subject's resistance when the bridge was balanced, it was easy to ascertain—by direct calibration of the apparatus—the mean resistance of the subject during the experiment.

I then multiplied the deflexions of each subject by his mean resistance (in thousands of ohms) thus obtained. This eliminates variations due to subjects possessing different intrinsic resistances. (I hope sometime to give a justification of this statement in a special note devoted to the *technique* of the psycho-galvanic reflex.) Deflexions were also, of course, multiplied, for purposes of computation, by the appropriate 'weight' as already indicated.

Inasmuch as the resistance of most subjects decreased during the experiment, this procedure tends slightly to penalise the earlier words of the series as compared with the later words. But this does not affect the conclusions drawn from the experiments so far as memory is concerned and is, moreover, to some extent counteracted by the *torque* of the galvanometer suspension which tends to decrease the deflexion when the excursion begins at a high initial reading.

All the subjects were thoroughly normal and were drawn from the educated classes. Of the men 35 were between 19 and 30 years of age, while one was about 35: 15 of these were junior Naval officers, the remainder were University students. Of the women 12 were between 19 and 30 and 2 between 45 and 60.

(ii) In order to measure the 'memory value' of the words, I caused the subjects to learn by heart a selection of the stimulus words whose concomitant affective tone I had measured in the manner described above. These words were reproduced by the subjects at gradually increasing intervals and from the success of the reproductions a measure of their 'memory value' was obtained.

The procedure was as follows:

Two days after the experiment I sent to each subject a list of thirty words arranged in five rows of six words each[1], *e.g.*:

Long	Friend	Ask	Dance	Sick	Ink
Swim	Carry	Tree	Pity	Dress	Money
Finger	Paper	Frog	Hunger	Child	Plum
Marry	Wine	Carrot	Flower	Family	Cow
Name	Luck	Chair	Kiss	Choice	Insult

They were carefully instructed to commit these words to memory as mechanically as possible (*i.e.* not to invent any *memoria technica* for them), to destroy the list when they had learned it well enough to say it through once without a mistake, and to think as little about the words as possible when once they had been learned.

Reply postcards were then sent to each subject on the 5th, 9th, 14th, 21st and 31st days after the experiment with the request that the subject would write down as many of the words learned as he could then remember and would underline those over which he hesitated or which seemed to recur with difficulty. All subjects were warned not to 'rack their brains' or to make great efforts about the reproductions but rather to write down quickly and immediately as many words as came to them in the space of two or three minutes.

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[1] Association by meaning between successive words was sometimes inevitable, but its effects tell against, rather than in favour of, my conclusions.

In computing the results, words which were perfectly reproduced were given two marks for each reproduction, words underlined were given one mark and words not reproduced 0 marks.

Any word might, therefore, make a total score of any integer from 0 to 10. The total score actually made was taken as the memory value of the word concerned.

C. RESULTS.

In case it should be suspected that the magnitude of the galvanometer deflexion is not a quantitative indication of the amount of affective tone evoked by the corresponding stimulus word, I give here the mean deflexions, arranged in order of magnitude, which were obtained in response to the hundred stimulus words of my list (Table I) for the whole of the fifty subjects examined.

TABLE II.

Word	Defln	Word	Defln	Word	Defln	Word	Defln	Word	Defln
1. Kiss	72·8	21. Worry	33·0	41. Book	26·1	61. Cold	23·0	81. Bird	19·6
2. Love	59·5	22. Insult	32·5	42. Travel	25·9	62. Long	22·7	82. Bread	19·6
3. Marry	58·5	23. Friend	32·2	43. Sick	25·8	63. Go	22·6	83. Old	19·3
4. Divorce	50·8	24. Head	31·7	44. Bag	25·8	64. Cook	22·3	84. Cow	19·0
5. Name	48·7	25. Angry	31·5	45. Water	25·6	65. Yellow	22·2	85. Bring	19·0
6. Woman	40·3	26. Wine	30·9	46. Home	25·4	66. Chair	21·7	86. Clean	18·8
7. Wound	38·0	27. Luck	30·8	47. Big	25·3	67. Finger	21·5	87. Ink	18·7
8. Dance	37·4	28. Green	30·4	48. Bed	25·2	68. Sad	21·4	88. Sheet	18·6
9. Afraid	36·8	29. Ask	30·0	49. Silly	25·2	69. Tree	21·2	89. Table	18·5
10. Proud	36·7	30. Make	29·9	50. Brother	25·2	70. Needle	21·1	90. Work	18·3
11. Habit	36·6	31. Pity	29·7	51. Street	24·9	71. Blue	20·6	91. Carrot	18·2
12. Money	35·6	32. Choice	29·7	52. Beat	24·6	72. Ship	20·5	92. Bury*	18·0
13. Fight	35·0	33. Dress	28·5	53. Carry	24·5	73. Motor	20·4	93. Hunger	17·9
14. Child	35·0	34. Wicked	28·4	54. Wait	24·4	74. Frog	20·2	94. White	17·8
15. State	34·8	35. Dead	27·6	55. Speak	24·3	75. Walk	20·1	95. Glass	17·6
16. Despise	34·7	36. Sing	27·6	56. Box	23·9	76. Try	20·0	96. Give	16·7
17. War	34·1	37. Horse	27·1	57. Nasty	23·6	77. Plum	20·0	97. Flower	16·1
18. Family	33·6	38. Evil	27·0	58. Jump	23·5	78. Village	19·9	98. Pond	15·5
19. Happy	33·4	39. Doctor	26·9	59. Paper	23·2	79. Rich	19·9	99. Pencil	15·4
20. Pray	33·1	40. Stalk	26·2	60. Lamp	23·1	80. Salt	19·8	100. Swim	14·2

* Note: this word was frequently understood as 'Berry.'

The deflexions given are weighted means and may be taken to represent the reactions of a typical 'standard' subject of resistance 1000 ohms.

It will, I think, be generally admitted that the earlier words in this list are intrinsically more likely to arouse affective tone in normal persons than those which appear later.

Of the first six words, five are closely connected with sex-life and therefore likely to be of the utmost affective significance. The other —'name'—may derive some of its importance from its associations with the 'ego-complex' (some subjects reacted with their own name), but I am inclined to think that its effect is mainly due to the operation of sex-factors, for the word is obviously likely to arouse thoughts of a wife, husband, lover or other person of sexual interest to the subject.

The dominance of this group of six words is very clearly demonstrated if the deflexions are shown graphically in order of magnitude. There is a steady and almost uniform increase of the ordinate from 'swim' to 'wound' inclusive when it suddenly shoots up in a most marked manner.

The somewhat low value for 'woman' as compared with the other members of the sexual group is apparently due to the fact that it possesses a much higher emotional significance for men (mean deflexion = 44·6) than for women (mean deflexion = 33·5), which is what one would expect.

Satisfactory reproductions of words learned were obtained from 41 subjects (31 men, 10 women).

The words learned by these subjects were classified according to their memory value (0, 1, 2, etc., up to 10) and the weighted mean of the galvanometric deflexions was calculated for each class. The probable error of these weighted means was also computed. The results are shown in Table III.

TABLE III.

Words scoring	No. of words	Weighted mean G.D.	Probable error of mean
0	326	25·62	1·03
1	23	46·26	7·93
2	77	21·73	2·13
3	12	40·00	10·31
4	22	22·89	5·25
5	12	27·35	5·48
6	31	25·44	3·63
7	38	21·61	2·48
8	93	33·47	2·51
9	102	38·10	3·64
10	457	36·93	1·34

(*Note.* The chance that the excess of the mean G.D. for words scoring 10 over that for words scoring 0 is accidental is about ·00001. The chance that the excess of the mean G.D. for words scoring 0 over that for words scoring 2 is accidental is about ·115.)

These figures are not very informative as they stand, owing to their irregularity and the great differences in reliability between the different means.

Their significance can be better brought out by drawing a smooth curve of the form $y = ax^2 + bx + c$,

through the points $x = 0,\ y = 25{\cdot}62,$

$x = 1,\ y = 46{\cdot}26,$

$x = 2,\ y = 21{\cdot}73,$ etc.,

by the usual method of Least Squares, *weighting the values of 'y' inversely as their probable errors.*

This curve is $y = {\cdot}324x^2 + 1{\cdot}16x + 23{\cdot}49$. For convenience of calculation the middle of the memory value range is taken as origin: this also applies to the equations given later.

This yields the following values for the mean G.D.'s:

TABLE IV.

Words scoring	Mean G.D.
0	25·8
1	24·0
2	22·9
3	22·5
4	22·6
5	23·5
6	25·0
7	27·1
8	29·9
9	33·3
10	37·4

This result is shown graphically in Fig. 2. It is clear that the words best remembered have on the average a very much higher mean galvanometer deflexion (*i.e.* arouse on the average considerably more affective tone) than those which are soonest forgotten; and that the latter have on the average an appreciably higher affective value than words which are moderately well remembered[1].

It must be admitted that the large probable errors of some of the means are calculated to arouse suspicions as to the reliability of the curve. Elimination of the least reliable values would not, however, alter its general indications. Moreover, the three curves obtained are concordant and, in addition, I have recently investigated a number of other points arising out of this experiment and using the same material; the results have been uniformly harmonious to an extent which would be impossible if the general form of this curve were incorrect.

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[1] In the latter, a still higher affective value would have been observed but for the effects of dilution (see p. 248).

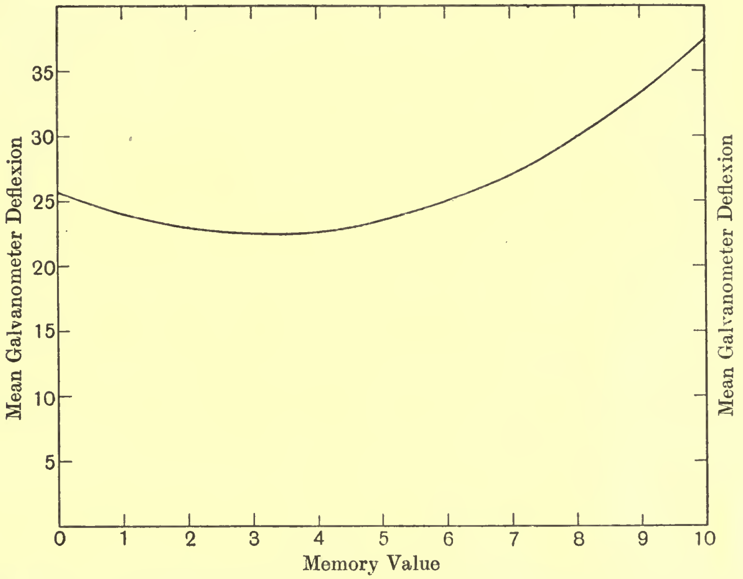

Fig. 2. Curve showing relation between Memory and Emotion as measured by the Galvanometer.

Similar treatment of the reaction times (40 subjects) gives the values shown in Table V.

TABLE V.

Words scoring	Mean R.T.	Probable error	Final values for representative curve
0	11·84	·25	11·99
1	11·13	·59	11·68
2	12·13	·49	11·44
3	11·58	·92	11·25
4	13·45	1·09	11·13
5	12·18	·84	11·06
6	11·16	·52	11·05
7	10·13	·34	11·11
8	13·31	·86	11·22
9	11·39	·48	11·41
10	11·67	·24	11·63

The equation to the representative curve is

$$y = \cdot030x^2 - \cdot036x + 11\cdot06.$$

This curve is shown in Fig. 3.

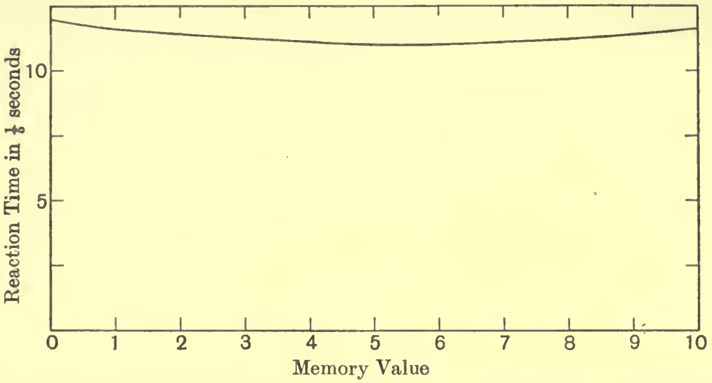

Fig. 3. Curve showing the relation between Memory and Emotion as measured by the Reaction Time.

Here again we find that the words best and worst remembered have a somewhat higher affective value than those moderately well remembered.

If we compute the percentage of failures in the reproduction test occurring among words scoring 0, 1, 2, etc. (20 subjects), and apply a similar method for obtaining a representative curve we get the figures given in Table VI.

TABLE VI.

Words scoring	Failures in reproduction (per cent.)	No. of words	Final values for representative curve (per cent.)
0	35·4	82	34·5
1	30·0	10	33·5
2	31·6	38	32·5
3	0·0	10	31·5
4	45·4	11	30·6
5	66·6	3	29·7
6	36·4	22	28·9
7	20·0	15	28·0
8	34·2	44	27·2
9	10·4	48	26·4
10	27·4	234	25·7

The equation to the representative curve is

$$y = \cdot014x^2 - \cdot88x + 29\cdot73$$

and the curve is shown in Fig. 4.

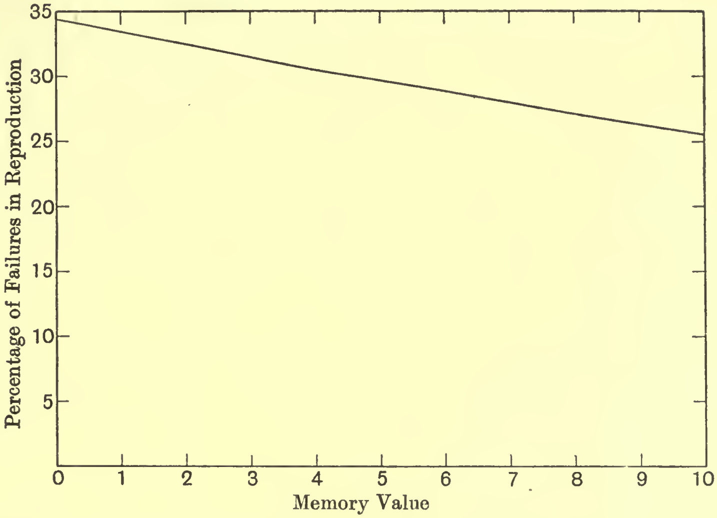

Fig. 4. Curve showing the relation between Memory and Emotion as indicated by faults in the Reproduction Test.

In this case it will be noticed that the curve is practically a straight line. There is a considerably greater percentage of failures in reproduction among words easily forgotten than among those well remembered. Practically speaking, the ease with which the word is remembered is inversely proportional to the tendency towards failure in the reproduction test.

D. CONCLUSIONS.

These results are entirely concordant with each other and clearly show two things:

First, that memory for words is influenced by affective tone; secondly, that, so far as the affective tone detected by the psycho-galvanic reflex is concerned, its influence may be exerted in two diametrically opposite directions; the fact that a given word evokes well-marked affective tone may lead to its being better remembered than a less intensely toned word, or may lead to its being forgotten more quickly. Affective tone as shown by the galvanometer deflexion should, therefore, be regarded as of two kinds, one of which facilitates, while the other impedes, the remembering of words which it accompanies.

On the other hand, the kind of affective tone which is shown by Jung's reproduction test is uni-directional in its effects and tends to impede the remembering of the words concerned.

The results obtained from reaction times are less marked than those given by the galvanometer deflexion or by the reproduction test, and at first sight appear to conform more closely to the former than the latter. That is to say, the affective tone shown by prolongation of reaction time appears almost equally likely to facilitate or to impede memory. But, for reasons which I shall give later (p. 249), I regard this appearance as misleading and consider that prolongation of reaction time is almost exclusively an indication of the kind of affective tone which tends to impede memory.

I do not propose here to consider the relation of the two kinds of affective tone mentioned above to the varieties commonly described as 'pleasant' and 'unpleasant.' This is a question which would take us very far and I think it wiser to adhere strictly to the necessary inferences from the experimental results.

I shall, therefore, speak of that variety of affective tone which facilitates remembering as 'positive' tone and of the opposite variety as 'negative' tone. To do this will imply no more than the bare fact already established that there is one kind of tone which aids the accession to consciousness of the ideas which it accompanies and another which impedes it. In very many cases, of course, pleasant and unpleasant will correspond with positive and negative respectively, but there are many exceptions to this and I feel that the former antithesis is of little value for the understanding of mental processes, because the effects of pleasantness and unpleasantness are not invariable.

The fact that the mean galvanometer deflexion for words scoring 0 is less than that for words scoring 10 (Table IV) is, in my opinion, due not to negatively toned words being less *intensely* toned than positively toned words and therefore giving a smaller deflexion, but to the fact that the words corresponding to this end of the curve contain a much larger proportion of 'indifferent' words (*i.e.* words of light tone, giving a small deflexion) than those corresponding to the other end. The words possessed of strong negative tone which are immediately forgotten (and so make a low memory score) are more copiously 'diluted,' so to speak, with indifferent words, which are gradually forgotten in the course of the experiment, than are those possessed of strong positive tone which are well remembered and so make a high memory score.

The effect of this will be to decrease the mean galvanometer deflexions (computed for a mixture of negatively toned and indifferent words), at the left-hand end of the curve corresponding to easily forgotten words. If we were to eliminate these 'indifferent' words, the curve would be far more nearly symmetrical than it actually is. Accordingly, I think it probable that the galvanometer is approximately equally responsive to positive and negative affective tone.

Similar considerations apply to the reaction time curve. The index to affective tone is, in this case, the prolongation of the reaction time and the effect of dilution with indifferent words is to depress the left-hand end of the curve. If this dilution were eliminated the curve would slope downward from left to right more steeply than it actually does. That is to say, prolongation of reaction time is, in general, a sign of negative tone.

The curve for the reproduction test shows, even as it stands, that failure in reproduction occurs mainly in the case of words easily forgotten —*i.e.* 'negatively toned' words—and here again this effect would be considerably increased if it were not for dilution by indifferent words.

This agrees with what is already known about the reproduction test, and the same comment applies, though less forcibly, to the preceding paragraph.

One conclusion of some importance which can be drawn from the form of the curve for the galvanometer deflexions is that positive affective tone and negative affective tone are equally definite and, so to speak, 'real' things. That is to say, positive tone does not consist merely in the absence of negative tone; nor does negative tone consist merely in the absence of positive tone. If either of these two latter alternatives were correct, the curve would not show a minimum value as it does; in the first case it would slope downward throughout from left to right, in the second from right to left.

This influence of affective tone on memory is relevant to the rate of forgetting. It is well known that the rate is more rapid for the period immediately following the learning of the material concerned than it is for later periods. I consider that this effect is largely due to qualitative and quantitative differences in affective tone between different parts of the material.

A number of words, for example, all of which were accompanied by equal and similar affective tone, would be forgotten according to some unknown rule determined by other than affective causes—possibly at a uniform rate,—whereas differences of affective tone would result in the negatively toned words being forgotten sooner and the positively toned ones later than would be indicated by the rule. This would result in the number of words forgotten per unit of time (*i.e.* the rate of forgetting) being greater in the earlier stages of the process and lower in the later stages than would be the case if affective factors were not operative. In other words, if we could discount the effect of affective tone, the curve connecting the quantity of material remembered with the time elapsed since learning it, would approximate more closely to a straight line than it actually does. It seems probable that considerable light may be thrown on the nature and relative importance of the factors involved in memory by further investigation of this point.

(*Manuscript received* 27 *August* 1920.)

REPORT ON THE SELECTION OF CHILDREN FOR HIGHER EDUCATION AT HAMBURG[1].

By GODFREY H. THOMSON.

The State of Hamburg decided to divide the elementary school above the fourth class; the bulk of the pupils at the age of ten, numbering about 20,000, continuing for another four years in the ordinary elementary course, the more capable, about 5 per cent. of the 20,000 being selected, with the aid of psychological tests, to proceed to a special five years' course. The latter was called the *F-Zug* or *Fremdsprach-Zug*, two foreign languages being learnt; the more elementary course was called the *D-Zug*, German (*deutsch*) being the only language.

990 children had to be selected. In the first place 1355 were chosen by the teachers, six boys and three girls from each school or twice these numbers in the case of certain double schools. The final selection was made on the combined basis of three factors. (*a*) The ordinary school report for each pupil; (*b*) a special psychological report, made by the class teacher who answered certain questions about each candidate on a special Observation Sheet; (*c*) a psychological group test, lasting for four hours,—a kind of examination in which, however, the tests were designed to prove not knowledge but ability.

The Psychological Laboratory of Hamburg prepared a full account of all the tests which had ever been standardised for such purposes as these; and their recommendations included such a large number of tests that the special 'coaching' of the children in all of them was quite impossible. The Laboratory is constantly adding to these.

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[1] *Hamburger Arbeiten zur Begabungsforschung.* Nr. 1, *Die Auslese befähigter Volkschüler in Hamburg,* von Rudolf Peter und William Stern. S. x + 157. 1919. Mk. 12. Nr. 2, *Untersuchungen über die Intelligenz von Kindern und Jugendlichen.* Herausgegeben von William Stern. S. iii + 167. 1919. Mk. 15.60. Nr. 3, *Methodensammlung zur Intelligenzprüfung von Kindern und Jugendlichen,* von William Stern und Otto Wiegmann. S. v + 256 1920. Mk. 24. Beihefte zur Zeitschrift für angewandte Psychologie. vi Folge. Leipzig: J. A. Barth.

The eight tests used at the examination, held at Easter 1918, were as follows:

(1) Definition Test: the four words 'booty,' 'uncle,' 'courage,' and 'envy' had to be defined. (2) Fable Test: the moral contained in two fables had to be given. (3) Criticism Test: the absurdities contained in a short story had to be pointed out. (4) Observation Test: long sentences were given once, and had to be written down from memory. This test was introduced to ascertain whether only odd words, mechanically remembered, or whether the substance of the whole, possibly in other words, could be reproduced. (5) Conjunction Completion Test: in a piece of prose the conjunctions were left out and these had to be filled in. (6) Three-word Test: out of three given words to make a sensible sentence. (7) Arranging Ideas: a number of words were given which had to be arranged in a sensible order. (8) Essay Test: two pictures were shown which gave two successive phases of an event and this event had to be recognised and described in a short essay.

The tests were given simultaneously at several centres. This involved the careful training of supervisors at each centre. The supervisors were members of the Psychological Laboratory Seminar Class, and exact instructions for their conduct of the tests had been printed and explained to them at several meetings beforehand. Indeed many of these supervisors had been engaged in compiling the tests which it was intended to give. The tests were finally conducted in 11 schools and 61 classrooms on two days. Each group was therefore of about 20 to 25 pupils, and had one supervisor and assistant.

The marking of the tests was very carefully planned, but cannot be described here. The system was one in which marks were given for errors rather than for excellences, and the children with the fewest marks were the winners, as far as these tests alone went. But the tests were not the only criterion, as was mentioned above.

The committee that finally decided which children were to be chosen consisted of five persons, namely a *Schulrat* (probably an experienced member of the Education Committee), a school-inspector, a male teacher, a woman teacher, and the director of the Psychological Laboratory. This committee had before it in the case of each child:

1. *The judgment of the teacher* whether the child was specially recommended or not.

2. *The ordinary school report* which in Hamburg takes the form of a number allotted to each child.

3. *The details of the year's work* of the child, giving particulars of

his marks in each subject of instruction for each of the past four years, particulars of attendance, etc.

4. *The result of the Intelligence Tests*, given as a number.

5. *The Observation Sheet*, on which the teacher had answered a number of questions relating to the child's various psychological qualities, such as memory, imagination, etc., and also inquiries about the leadership of the child in games, about his hobbies, and so on. This Observation Sheet itself would need several pages for a full description of its various features.

All the children whose marks in 1, 2, and 4 were uniformly good were taken without question. Similarly there was a set of children about whose rejection there was no doubt. The difficulties of the committee entered in deciding the border-line cases. Here the Observation Sheet became of remarkable assistance, If for instance the teacher noted thereon that the child worked as a rule very slowly but very thoroughly, this threw light on any failure to finish the task in the tests. In other cases it appeared from the Sheet that the school marks of the child were the result of industrious memorising rather than of real intelligence.

CRITICAL NOTICE.

By CARVETH READ.

The Foundations of Character: being a study of the Tendencies of the Emotions and Sentiments, by ALEXANDER F. SHAND, M.A. London: Macmillan and Co. 2nd edition. 1920. pp. xxxvi + 578. 20*s.* net.

When this important book first appeared in 1914, only the briefest notices of current literature were ever given in this *Journal*, and unfortunately it was never noticed at all. We are very glad to see the issue of a new edition; chiefly, indeed, for the evidence thus supplied of a wide interest in the subject and of public appreciation of merit, but incidentally because it presents an opportunity to make up for former neglect.

It may be assumed that the reader is acquainted with Mr Shand's general conception of the organization of character—how impulses and instinctive actions are variously utilised in the systems of the emotions, and emotions coordinated in the service of the sentiments. An outline of this is given in the first part of the present volume; but to understand the full meaning of it in the constitution of character we must wait for the second volume, in which the laws of the sentiments are to be fully unfolded. It is not too much to say that an interested public is looking forward to the appearance of that work with flattering impatience.

The Foundations of Character is one of the most original works that British psychology has produced. It is full of matter of fact, experience and the witness of the greatest observers of human nature. The results of critical reflexion upon this material are summed up in a series of laws of the behaviour of the constitutive forces of human character, especially (in this volume) of the emotions. Shallow critics have complained that some of these laws are already commonly known; and that is true, but no valid animadversion; for in giving a systematic account all of them must be included. There is probably no reader to whom the greater part of them will not be new and enlightening. And the way in which they are arrived at is thoroughly scientific, namely, by beginning with approximate generalisations, and correcting them step by step in view of the exceptions, until a proposition that may be considered a law of the tendency of some character force has been provisionally stated. The author claims no more; he pretends to no finality. With a subject so complex and entangled there seems to be no other method; and one admires the patience, penetration, circumspection and resource with which it is carried out.

Still, although "the desire of perfection" (as someone said in expostulating with Napoleon) "is the greatest evil that afflicts mankind," yet it is so strong in a critic that he is sure to find some ground of complaint. The general conception of the problem in this book is biological, and if this had been strictly adhered to some positions might perhaps have been made clearer. For example, at p. 21 Mr Shand states the law of organization: (1) *Mental activity tends...to produce and sustain system and organization*; for, he says, the mind reflects the body. Should we not, therefore, add—*in relation to the environment*? Except in relation to the environment there is no such thing as character. Man, like every other animal (and Mr Shand never forgets his zoology) is adapted to live under certain conditions—social conditions being for him the most important; and although he has greater range of choice how to live—which expands with civilisation—yet there are always certain things he must do, or aim at, in order to live at all. Above all, he must plan for the future in order to live well; and, therefore, purposes, sentiments, desires, rule his conduct. In most cases a man does chiefly one thing; and, if he has been able to choose this freely, we may suppose that it represents better than any other would the strongest tendencies of his nature; whilst other things that he does at leisure, or as subsidiary tasks, represent his secondary tendencies—or with him who had little choice of principal task, the primary tendencies. In following out such purposes, he meets with various helps and hindrances, and various emotions become manifest, generally as reinforcing (or inhibiting) his actions in dealing with the situations arising from such helps or hindrances. A situation is constituted on the one hand by circumstances, and on the other hand by the man himself in such a disposition as he happens to be in at the time; and this situation excites his emotions. Situations, in their general setting, are very few (though in detail infinitely varied), and they frequently recur; and, therefore, the primitive emotions corresponding with them are also very few, as Mr Shand's analysis shows. Because the situations in their general setting are few, it is possible for man (or the higher brutes) to be so organized innately as to respond to them appropriately when they occur.

The emotions, then, are amongst the innate functions of a man's organization (with impulses and instincts) in relation to the environment; and perhaps it is only in this sense that they are *inter*organized; whereas Mr Shand appears to hold that they are innately organized in relation one to another (pp. 37–8); and this view leads to certain questionable modes of expression. He personifies the emotions, because each involves cognition, conation and feeling—the three essential elements of the mind as self; and this is still more plainly true of a sentiment (p. 64). The device is certainly convenient in exposition, and not merely for its literary effect. But taken along with the doctrine of an innate organization of the emotions, it results in some statements as to their operation one upon another which obscure the real causation. For example, Law (2), at p. 38, reads: *every primary impulse, whether it is independent or belongs to a primary emotion, is innately connected with the systems of fear, anger, joy and sorrow, in such a way that, when opposed, it tends to arouse anger, when satisfied joy; when frustrated sorrow, and when it anticipates frustration fear; these systems being similarly connected together.* Would it not be more accurate to say that a man is so organized that when any primary impulse is opposed, or satisfied, or frustrated, or threatened, the *situation* tends to arouse anger, or joy, or sorrow, or fear? The activity of the impulse is indeed necessary to complete the situations, but does not itself arouse the various emotions.

In several other laws in which one emotion is said to elicit another, it would be clearer to say that the change of situation elicits the other emotion. Emotions directly influence one another chiefly by mutual exclusion, when their physiological expressions are incompatible. Consider the following Law (91), p. 403: *Repugnance tends to exclude joy; and when its impulse is opposed, to evoke anger; and when it is frustrated, sorrow.* The first clause is strictly true; but the second and third clauses might begin—*and when its impulse is opposed, the situation tends to evoke anger*, etc. Similarly in the functioning of sentiments, if danger to a beloved object excites anger or fear, it is not love, but the situation of which love is a necessary factor that has these consequences. A sentiment (in this sense) establishes no interorganization amongst the emotions.

So much for the desire of perfection, that can only discover such formal flaws, and finds nothing to complain of in the essential method or in the wisdom of the book before us[1].

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[1] For want of a better place, a footnote may be utilised to draw attention to B. I, c. XI, *On the Relative Ethics of the Sentiments*: a remarkable chapter, which has not perhaps obtained the consideration it deserves.

The system of desires is discussed in B. III with great originality and penetration. Desires being excited by ideas of ends not immediately attainable, in our efforts to fulfil them success may seem more or less likely; and hence arise emotions peculiar to the state of desire, the 'prospective emotions' of Hope, Confidence, Anxiety, Disappointment, Despondency and Despair (p. 462). In the third chapter (p. 469) it is shown that these prospective emotions resemble (or how far they resemble) Joy, Fear and Sorrow. Such emotions arise (as Mr Shand expressly says) upon changes in the situation (or in our estimate of it), as more or less prosperous in view of the end. The system of a desire, therefore, is very much like the system of a sentiment. "Desire is a chief constituent of all human love" (p. 477); "there are some desires of such consequence that our whole nature supports them"—as when life is in danger; "the principal desire of every sentiment is supported by the whole strength of its system" (p. 500). These sayings seem to be true; and in the context it is not clearly shown how sentiments differ from the contained desires. But this lacuna has been filled in the third Appendix to the present edition; where we learn that a sentiment is richer and more enduring than its corresponding desire; for every desire comes to an end with its fulfilment, whilst there remains to the sentiment joy in the contemplation of the end. And the sentiment of love includes "the contemplative emotions—joy, admiration, wonder and sorrow apart from its desire" (p. 565); and hatred, of course, the opposite states of displeasure.

In this third Appendix Mr Shand also disposes of an objection raised by Prof. Boyce Gibson to Law (140): *Desire and Joy tend mutually to exclude each other* (p. 510). In the desire of Knowledge, it was urged, there may be a continual joy of progressive attainment. But "we must distinguish between a desire as it is felt in consciousness and the entire system of desire." Felt desires are incompatible with joy; but "when a desire in the second sense is active, as shown by the progressive accomplishment of its means, though not felt, there is no antagonism between desire and joy" (p. 559); hence when moved by a desire of knowledge, we enjoy the process of learning.

A reader may perhaps feel a doubt intruding whether, after all, the desires do not constitute the necessary skeleton of character; which might almost be conceived to live and work by itself without the flesh and blood of emotion and sentiment. It might be argued that the emotions arise merely to reinforce, or to give conspicuous expression to, tendencies and desires; and that sentiments result from the richness of the higher mental life. And certainly the emotions do their work very clumsily, being all liable to excesses in which they frustrate instead of helping our activities; but they make a fine show. 'Desire,' indeed, is an inadequate appellative for the chief controlling influence of a mature character: 'purpose' better expresses the tutelary power that gives unity to a man's life, above fear and anger, above love and hate. But if we conceive it possible to subtract desire and purpose from character, do not the systems of emotion and sentiment crumble into dust? This may be worth the considering.

The new matter in this edition will be found chiefly in the Appendices. The third, on Desire and Joy and Desire and Love, has already been described. The first, *Of Impulse, Emotion, and Instinct*, endeavouring to settle the connotation of these terms, is instigated by differences with Prof. McDougall and Dr Drever. The second, *Of two Opposite Tendencies of Joy and Sorrow*, enriches the philosophy of the subject with several new laws. In reading it we feel again on what a solid ground of experience these *Foundations* are laid. Only upon the ground of experience, by long, patient, profound meditation could work so original and so trustworthy have been accomplished.

PUBLICATIONS RECENTLY RECEIVED

Mind and Work. By Dr CHARLES S. MYERS. University of London Press. 1920. pp. xi + 204. 6*s*. net.

This book is an expansion of the industrial section of the author's *Present-day Applications of Psychology.* Written in untechnical language, it gives an extremely lucid exposition of the problems facing the psychologist who attempts to apply his science to industry. In successive chapters, general accounts are presented of Movement Study, Fatigue Study, Selection Study, Restriction of Output, Systems of Payment, and Industrial Unrest. Discussions of methods and principles are admirably interspersed with statements of results already achieved,—including some motion study results obtained in this country (pp. 6–11, 15).

Among the most interesting discussions are those on systems of payment and industrial unrest. "Under present conditions, a flat day rate for all is psychologically unsound, unless there be grades of day rate" (p. 150). As an alternative to payment by results, Dr Myers suggests that there should be in each occupation grades of workers, these grades being based upon individual differences in skill and capacity, and he gives an instance of such a system in operation (pp. 149–150).

Present industrial unrest is considered pathological. Partly due to the war, mental overstrain has produced on a large scale abnormal psychological conditions, such as excessive irritability, sensitiveness, and lack of self-confidence. Employer and employee have adopted defence mechanisms in blaming each other, and a general atmosphere of suspicion and unrest has thus become universal.

The book ends with a reference to the National Institute of Industrial Psychology, which has recently been established in London. The research this Institute is intended to promote should make important contributions to the problems outlined by Dr Myers.

A useful bibliography is appended to each chapter.

Psychology applied to Music Teaching. By Mrs J. SPENCER CURWEN. London: Curwen and Sons. 1920. Pp. xii + 304. 15*s*. net.

This book is dedicated to Professor John Adams and is based on Herbartianism. The sections devoted to anatomy and physiology are derived apparently from Carpenter and Huxley,—but with modifications! "At the base of the skull," we are told (p. 29), "in front of the medulla and including a part of it, there is a collection of ganglionic masses which are called the Sensory Ganglia, or, taking them together, the Sensorium." The "nerve-process leaves a path in the actual matter of the brain. Every time you repeat the passage the path becomes deeper and more definite.... After a while it does not go to the upper brain at all, but is switched off at another point, outside consciousness, at one of the lower centres" (pp. 33, 34). "When the object is no longer there you have no percept, but you have a mental image of it"

(p. 93). "We do the greater part of our thinking by means of images, more or less generalized" (p. 120).

Modern psychology may smile or sneer at such statements. But it must welcome in Mrs Curwen's book a sincere attempt to apply its general principles to the teaching of music. To persons so engaged, who have had no training in psychology, the book will be unquestionably valuable and suggestive. It is a pity, however, that Mrs Curwen has not laid more emphasis on educational experiment. So much has yet to be done in applying to different methods of teaching music the principles of scientific research.

Reports of the Industrial Fatigue Research Board. No. 8, *Some Observations on Bobbin Winding.* By S. WYATT and H. C. WESTON. Pp. 40. 1*s.* 6*d.* net. No. 9, *A Study of Output in Silk Weaving during the winter months.* By P. M. ELTON. Pp. 69. 2*s.* 6*d.* net. No. 10, *Preliminary Notes on the Boot and Shoe Industry.* By J. LOVEDAY and S. H. MUNRO. Pp. 32. 1*s.* 6*d.* London: H.M. Stationery Office. 1920.

In each of these Reports the psychologist will find material of interest to him. Messrs Wyatt and Weston draw attention to considerable individual differences in efficiency and their dependence on differences in ability, method and temperament; they urge careful selection of the workers, and individual, rather than group, training, in order to secure maximum efficiency. Mr Elton finds that "under artificial illumination production falls, even if electric light of sufficient intensity is provided.... Every unnecessary hour under artificial light means a direct loss of production and makes the task of the worker more difficult than it need be." He considers that "unbroken 4½-hour and 4¼-hour spells of employment are too long." Messrs Loveday and Munro find that the daily output "curve of the skilled workman is more regular and begins to fall later in the week than the curve of the operative whose skill is less, or who is ill-suited to his work." Mr Loveday describes an interesting experiment in the introduction of rest periods. Double presses were worked "with a team of three girls, each operative working 40 minutes in each hour and resting 20 minutes instead of with two girls working continuously throughout the day." The operatives were paid by day-rate *plus* a bonus on output. "The employees increased the output per machine by roughly 50 per cent." None of the girls desired to return to the former hours.

Psychoanalysis: its history, theory and practice. By ANDRÉ TRIDOU. London: Kegan Paul, Trench, Trubner and Co. 1919. Pp. 272. 10*s.* 6*d.* net.

"What I have attempted to do," explains the author, "in the present volume is to sum up in a concise form the views of the greatest American and foreign analysts [the book is printed in the United States] which at present are scattered in hundreds of books, pamphlets and magazine articles. I have, whenever possible, presented their thought in their own words, through either direct quotation or condensation." The twenty-one chapters of the book cover a wide range of subjects, *e.g.* feminism and radicalism, the artistic temperament, crime and punishment, re-education and prophylaxis, the new ethics. A bibliography, containing over 200 references, and a glossary of psychoanalytic terms are appended.

Instinct and the Unconscious: a contribution to a biological theory of the psychoneuroses. By Dr W. H. R. Rivers. Cambridge: University Press. 1920. Pp. viii + 252. 16*s*. net.

A critical notice of this book will appear in a subsequent number of the *Journal*.

Lehrbuch der Logik: auf positivistischer Grundlage mit Berücksichtigung der Geschichte der Logik. Von Dr Th. Ziehen. Bonn: Marcus und Weber. 1920. S. viii + 866.

Activism. By H. L. Eno. Princeton: University Press, and Oxford: University Press. 1920. Pp. 208. $1.50.

From the Unconscious to the Conscious. By Gustave Geley. Translated by S. de Brath. London: W. Collins and Co. 1920. Pp. xxviii + 328. 17*s*. 6*d*. net.

Phenomena of Materialisation: a contribution to the investigation of mediumistic teleplastics. By Baron von Schrenck-Notzing. Translated by Dr E. E. Fournier d'Albe. London: Kegan Paul, Trench, Trubner and Co. 1920. Pp. xii + 340. 35*s*. net.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY

GENERAL MEETINGS.

October 2, 1920.	The Bearing on Psychology of Experiments on the Sensitivity of Plants, by Sir Jagadiz Chunder Bose.
December 11, 1920.	Sensation and the Sensory Image, by H. Wildon Carr.
	The Psychological Basis of Liberty as a Political Idea, by Graham Wallas.

SECTIONAL MEETINGS.

(*a*) Education Section.

October 12, 1920.	Unconscious Determinants of Educational Attitudes, by S. S. Brierley.
November 9, 1920.	Problems occupying the present attention of Education in the U.S.A., by M. T. Whitley.
December 7, 1920.	Psycho-analysis and Parenthood, by J. C. Flügel.

(*c*) Medical Section.

October 20, 1920.	Some Biological Aspects of Sexual Repression, by J. C. Flügel.
November 24, 1920.	Study of a Severe Case of Obsessions, by James Young.

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THE INFANTILE PSYCHE, WITH SPECIAL REFERENCE TO VISUAL PROJECTION.

BY DAVID FORSYTH.

I. *Visual hallucinations in childhood* (pp. 263–265).
II. *The revival and exterior projection of visual memories* (pp. 265–266).
III. *The supernatural in childhood* (pp. 266–268).
IV. *Psychic reality and objective reality: pleasure and reality principles* (pp. 268–270).
V. *Magic and the belief in spirits and demons* (pp. 271–273).
VI. *Projection of feeling: rationalisation and magic* (pp. 273–276).
VII. *Conclusion* (p. 276).

I.

IT has long been known that the process of 'thinking' conforms generally to one of two types. In some individuals it is dependent on a series of verbal ideas, in others on a series of visual pictures. Of the two, the visual type is the more primitive and is alone found in infants. In keeping with this is a phenomenon which never fails to surprise those who are beginning to acquire the *technique* of psycho-analysis, namely, that patients, when coming to narrate the experiences of early childhood, recall their infantile memories as visual pictures, detailed and vivid as happenings of yesterday. In much the same way, forgotten events of long ago are often reproduced in dreams pictorially. Visual hallucinations form yet another class of psychical phenomena closely related to the foregoing.

Of late years much attention has been given to the whole subject by psycho-analysts and attempts have been made by Freud and his followers to formulate the laws governing the production of dreams and hallucinations, the revival of visual memories, their association and their repression and substitution by other visual memories. In practically all these investigations the approach has been by the analysis of adult psychical structures whereby they are reduced to their infantile elements.

An alternative and, in some measure, complementary procedure is to study the phenomena as seen in early childhood. This latter method, however, presents obvious practical difficulties and, so far as I know, has not been adopted on a psycho-analytical basis. The account that follows is intended as a presentation of this other side of the question.

My interest was first definitely drawn to the matter by an incident, apparently trivial, which when reflected on, disclosed a wide field of attractive problems. It was this. One of my children, a boy of four years, who had just come from playing with his Noah's ark, was passing a half-lit landing *en route* for bed, and stopped to give his good-night kiss. He got so far as to put his arms round my neck when he suddenly drew back with a cry of surprise; and explained that as he was about to give his kiss he closed his eyes, whereupon he saw vividly before him the very animals which he thought he had left safely in the ark downstairs. It was evident from his further remarks that he in no way doubted the reality of this unexpected vision.

On a subsequent occasion the same child, when asked experimentally to shut his eyes and say if he saw anything, replied instantly, "Farmers raking hay," and readily identified the scene (and the farmer and farm hands) as an incident in his summer holiday more than two years before. Asked to repeat the experiment he now said he saw a picture from one of his books which he thereupon proceeded to bring from his bookshelf and quickly turned up a coloured illustration of hay-makers.

Both these memories came without any conscious effort and the child regarded them as nothing out of the ordinary. Nevertheless, the question arises as to why these particular images—of hay-making—presented themselves on that particular occasion, instead of any of the thousand-and-one other visual impressions which his mind had stored. The clue to this is to be found in the current circumstances. At the moment the boy was about to leave for morning school on one of the first bright days of early spring after the gloom of a long winter spent in London. His visual images indicated plainly enough where his feelings would have transported him. If this explanation is correct (and I do not doubt it), it is enough to show that visual memories are connected with emotions (inner excitations) and are revived in harmony with the prevalent feeling-tone at the moment.

Similar experiments with other children never failed to disclose these images, and in the case of one child—an intelligent boy of 10 years—a succession of no fewer than 201 separate pictures was described by him in a period of 40 minutes when the experiment was brought to a close as he complained of mental fatigue. In sum, these observations reveal a phenomenon which has been but little heeded but which would seem to be of universal occurrence. Moreover, as an every-day experience of individual children, it cannot fail to play an important part in their mental life. For these reasons a study of its various bearings promised to throw a light on psychic development at this period of life. Before the matter is carried further, however, some cognate theoretical considerations must be briefly stated.

II.

The special sense organs (eye, ear, etc.) are characterized by a high degree of sensitiveness, each to one special form of external stimulus (light, sound, etc.); and though for a short while after birth they are of small account, they come to exert far-reaching effects. The visual organ, which alone interests us here, transmits a centripetal wave of excitement which is registered in the mind as a memorative impression of the excitation. This visual memory becomes associated with inner (somatic) excitations, and can be subsequently activated from either of the two directions in which it has established excitatory connexions: (i) a repetition of the external stimulus will revive the original memory, which will bring with it some of the inner excitation previously associated with it, or (ii) the same inner excitation recurring, the memory will be activated even in the absence of an external stimulus. In the former event the nature of the affect which is roused by the later stimulus is not a matter of chance, but is already determined by the affective response to the original stimulus.

The alternative occurrence—activation of a memory by inner excitement—is responsible for and explains several psychical phenomena which have customarily been grouped together under the apposite name of 'imagination' (image-forming). In circumstances when this excitement has accumulated without relief and the tension has led to an overflow of energy to the psychical sphere, a visual memory (or memories) previously linked with this excitement at the time of an earlier experience, is revived; it is seen, so to speak, 'in the mind's eye,' or as a visual hallucination; and inasmuch as it is activated by inner excitation, its essential function is to serve as a substitutive or imaginative relief of this feeling, in lieu of real gratification; its purpose to realise an unfulfilled desire.

A further development of the same process is seen under the continued stress of ungratified excitement, when a series of hallucinatory images composes a day-dream or fantasy. Yet again, a similar awakening of memories by ungratified excitement is to be recognised in what is the most striking phenomenon of sleep, namely, the dream. This, like the hallucination and the fantasy, is an imagined gratification of feeling, and serves the purpose of wish fulfilment.

It is necessary, however, to recognise a peculiarity of visual (and auditory) memories as compared with impressions from erogenous zones. In the case of the latter, the stimulus (food, faeces, etc.) is necessarily in physical contact with the sensory surface; and when the memorative impression of the object providing the stimulus is vivified, the feeling associated with it is projected to the local site where the outer stimulus was originally applied (peripheral projection). But in the case of visual and auditory impressions, the object of which a mental trace has been formed lies at a distance, and the impression is projected, not to the eye or ear, but to the distant site whence the stimulus was derived. We recognise, therefore, a stage of 'exterior projection' of images.

III.

To return to the visual pictures seen by children. It is difficult for many adults who have left their childhood far behind to realise how important a part these hallucinations do, in fact, play in the daily life of children. Indeed, given the suitable condition of darkness, dim light, or even broad daylight provided the eyes are closed, children can hardly escape the experience of seeing visions. Not that they speak about them readily, and this for several reasons. Partly because they are so familiar with them that they take them for granted and so not worth talking about; partly because they are not encouraged to discuss them by their parents who often enough regard all such manifestations as unhealthy and consider that to attach any importance to them will foster a morbid mind—a tendency better met by ridicule than by sympathy; and partly because they like to keep their pleasant visions to themselves, and to forget the unpleasant ones as soon as possible. Nevertheless, they will speak freely enough to those who have their confidence, and no one who is responsible for the up-bringing of a child can afford, in the child's interests, to dispense with the help which these hallucinations offer—the agreeable ones give an insight into its joys and aspirations, the fearful ones into its apprehensions and terrors, and together they provide a running commentary on its psychical development.

In general, darkness is the necessary condition for 'seeing' these images (though the fact that merely closing the eyes serves the same purpose is proof that images are always flickering in the mind without being consciously recognised) and it is after going to bed, waking during the night, in dark rooms, stairways and passages that children mostly become aware of them. The forms they take are legion. One child, peering into a dark room, saw an angel dancing and beating time with its wings; at night-time an ugly old woman was seen grinning at the foot of the bed. In different circumstances different images are seen but every child is familiar with some which recur at short or long intervals, though even these are liable to cease appearing, their places being taken by others.

One of the most significant points in connexion with them all is that children project them externally. They see them, not in the mind's eye, but outside themselves, in the vicinity; and younger children at any rate never doubt that they are seeing real objects. Even fairly old children, though they may seek to explain them away as imaginary, can by no means altogether dispossess themselves of the feeling that they are real. In this they are like many adults who, under stress of feeling, fancy they see a ghost or the figure of a dead relative or hear an expected knock at the door[1]; reason may say that these are imaginary but the effect is as though they were real. The explanation of this is not far to seek. It is not, as most suppose, that the image provokes the emotion; on the contrary, the image is a memory which owes its revival to internal excitation, and the hallucination is not the cause but the result of the feeling. But the feeling is genuine and the assumption is made that the image is equally authentic.

Just as children, for several years, never suspect the reality of their night dreams, so they accept their hallucinations at the same valuation. It is possible, therefore, to form some notion of the kind of world in which every infant finds itself—a world in which the creations of the imagination are not in any way discriminated from the objective occurrences in the outer world. When, further, it is remembered with regard to hallucinations that emotional excitement may revive several memories simultaneously which may share little or nothing in common except that all of them have at some previous time established associations with this particular emotional tone, it will be realised how the most dissimilar images may be brought together in a single hallucination, and a situation represented which, in respect both to objects and actions, is impossible, fantastic, extravagant. Nevertheless, this is accepted without question as real. A mind which necessarily begins its development with an incompetency so complete, will, even years later, not only see nothing incongruous in fairy tales, myths and legends, but find itself accurately and sympathetically reflected in fabulous creations of all kinds.

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[1] And, of course, insane people may revert habitually to the childish belief in the reality of their projected images.

To the young child, therefore, the supernatural and the natural are one and the same, time and place impose no limits, physical laws do not operate. And it may safely be added that the younger the child the more completely do the two spheres of the real and the imaginary coalesce. The world to an infant is created and peopled in harmony with its changing feelings, to which, indeed, it merely serves to give expression.

This conclusion cannot be examined without its bearing on several matters of psychological interest becoming apparent. In three directions especially can it be usefully applied,—(i) psychic and objective reality: pleasure and reality principles, (ii) magic and the belief in spirits and demons, (iii) projection of feeling: rationalisation and magic.

IV.

Psychic reality and objective reality: pleasure and reality principles. The infantile psyche is totally unable to appreciate the essential difference between the objects which are seen in the outer world, and those which are the product of its own mechanism. Only in the course of years can this differentiation be made with certainty, and an intermediate stage must be passed through in which the two must often be confounded[1]. (Parenthetically, it would be instructive to know how often the look of open-eyed wonder on the features of an infant even of a few months is the result of a noticed contradiction as between the two spheres.) The question suggests itself as to which—the objective or the psychic—, when first the difference between them is becoming recognised, would present itself as true, which as fictitious. Familiarity must be the decisive factor. The earliest excitations are psychical and the approach to the objective can only be from the familiar to the strange. Moreover, hallucinated images are accepted as real and the emotions responsible for their revival are indisputably real. To the infant, therefore, the psychic world is the real world and the objective world is to be accounted for only in terms of psychic activities.

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[1] Most often, the psychic is mistaken for the objective, but the reverse is not uncommon. Thus, a child of 7 years, taken for the first time to the Victoria and Albert Museum and wandering through the (to him) vast galleries with their beautiful furniture, strange statuary, clothed wax-models and the echoing silence everywhere, exclaimed several times that he thought he was dreaming and would soon wake up and find it was all a dream.

But this, of course, is the contraposition of that taken in later years. Reality is then defined as 'actual being or existence' or as 'the quality of having an actual existence,' and the term is customarily employed in reference to the objective or physical world as perceived by the senses, and in contradistinction to 'imagination' by which I mean 'the power to reproduce an object of sense previously perceived.' According to this use of the terms, an hallucination, including both the visual image and the emotion that activates it, is imaginary and not real.

But the emotion is to be identified as nervous energy, itself a form of physical energy. Equally, all sensory stimuli coming from the outer world are forms of physical energy. No matter, therefore, whether it is a question of sensory stimulation or of emotional excitation, the manifestation is one of physical energy. Both are real in the sense of actual being, and the primary distinction between reality and imagination cannot be maintained. The relation between them might be usefully indicated by the terms objective reality and psychic reality, objective truth and psychic truth. And as, moreover, the phenomena of psychic reality are familiar experiences at an age when little or nothing can have been learned of the phenomena of objective reality, it must be concluded, if only on chronological grounds, that reality to an infant can only mean psychic reality and that the other kind has not yet become differentiated. The attitude is a reversed one: what is imaginary to us is real to the infant, and what is real to us is perceived by the infant only as part of psychic reality.

This brings us to a consideration of the 'reality principle' and the 'pleasure principle' which, formulated by Freud[1], have gained general acceptance as the two opposing principles shaping psychical development. Freud has recognised that the deeper, earlier and more primitive tendency is to seek pleasure (this includes avoiding pain), *i.e.* the relief of feeling, wish fulfilment; this is the only principle recognisable in infantile life. Later, it becomes opposed by the reality principle (which he regards as probably derived from it) which leads the individual towards adaptation to external environment, whether this process is immediately pleasant or not, *i.e.* towards 'adjustment to reality.' Subsequent to the infantile period both processes are in simultaneous operation, and the success with which the reality principle prevails over the other is largely the measure of the normal or the neurotic disposition of the adult; while only those of exceptional intellectual capacity are able to order their lives in accordance with the reality principle, since this implies severe renunciation of pleasure.

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[1] "Formulierungen über zwei Principien des psychischen Geschehens," *Jahrbuch der Psychoanalyse*, III. 1.

These principles can be usefully considered in relation to the projection of visual images in early life. Children accept their feelings and the hallucinations produced by them as real, and their behaviour is influenced accordingly, though neither feelings nor hallucinations have any objective existence. At the same time they lack experience of the external world —an experience which comes only by long use of their peripheral sense organs. The difficulty, therefore, with which they are confronted is that of reconciling the conflicting impressions received on the one hand from the external world through their peripheral senses, on the other from the inner psychical world; and of the two the latter bears the stamp of reality[1]. In other words, the pleasure principle represents the quest of psychic truth, the reality principle that of objective truth. The one concerns only the relief of feeling, the other the recognition of peripheral sensory impressions.

It would appear, therefore, that the reality principle is not genetically derived from the pleasure principle, but arises independently. Nevertheless, a factor common to the two must not be overlooked. Peripheral sensations imply excitation of the peripheral organs, whichever these may be, and this stimulation involves the relief of tension and is a source of pleasure. Sensory impressions, therefore, are themselves pleasurable in the first instance and the reality principle possesses at any rate one root which is hedonic.

It may be permitted here to indicate quite briefly two main lines of development, whether phylogenetic or ontogenetic, leading from the two kinds of reality. Where the pleasure principle dominates and psychic truth is accepted as the standard, interest passes to elemental psychical processes and thence to the supernatural and the spiritual; this is exemplified in the evolution of religion and of personal religious faith. Alternatively, with objective reality as the aim, chief importance is given to the facts of the physical and material world and thence to natural laws; along this line come science and an interest in science[2].

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[1] It is, of course, not only children who fail to accept the evidence of their senses when this conflicts with their feelings. Thus, an acute physician of my acquaintance whose only child developed a sore-throat which was obviously diphtheritic, was unable to accept the testimony he saw in her throat until four other physicians had been called in one after another and given the only possible diagnosis.

[2] The conflict between religion and science represents the difference between psychic and objective reality. Each gives credence only to the laws which operate in its own sphere. Each has truth indisputably on its side; each remains ignorant of the other kind of truth.

V.

Magic and the belief in spirits and demons. Another set of implications arises out of the fact that the child's world is created by its feelings and serves to give expression to them. As we have seen, visual projection is a phenomenon which results in a child seeing in its external environment objects which do not exist outside its mind and owe their psychical existence to its emotions at the time. With every change of feeling goes a change of projected visual images, and whatever the kind of feeling, the associated memories are projected externally. Daily experience of this can at first only support the belief against which for the time being no substantial evidence can be forthcoming—that it is its feelings which determine the conditions of the outer world, and therefore that its feelings are the central factor on which the outer world depends. In other words, the outer world changes in response to its own wants and desires, and this without any effort or action on its part. To wish is alone enough to bring about the change.

To illustrate this we may take the case of a very young infant—too young to have as yet acquired experience of the objective world and familiar only with its own psychical processes. Such an infant crying loudly with hunger will, under the stress of its desire, visualise and project an image of its mother's breast, perhaps of the mother herself. Soon the mother appears, suckles it and relieves its hunger. The infant is not to know that it was its cries that summoned the mother, nor can it as yet distinguish between the image and the objective mother[1]. The incident can contain nothing more for it than that, in response to its want, the mother appears and satisfies it. Experiences such as this furnish the ground for the belief that merely to wish effects changes in the outer world, *i.e.* that the world is subservient to its wishes.

Regarded from another point of view, this implies a belief in one's power of influencing the (unknown) forces of nature according to one's desires; and those who are familiar with Frazer's work on the rites and

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Of the two, psychic truth is the more primitive and expresses the deepest needs of the individual; objective truth is the more highly acquired and best serves in subduing the forces of nature in the interests of the individual. Neither can be disregarded and the strife between them cannot cease until both are recognised and directed to a common end.

[1] Against this it cannot even be urged that an apparition vanishes in a way which is impossible to a corporeal form. A babe who watches its mother disappear through an open doorway, or behind a screen and emerge on the other side, cannot possibly see in this anything but what it is familiar with in connexion with the coming and going of a mental image. Apparition or corporeal form, each dissolves as suddenly, completely and inexplicably as the other.

customs of savages and primitive races will recognise this belief as the foundation of the world-wide practice of magic[1]. Almost any of the abundant examples which Frazer has collected bears this out and it will be enough to quote a single instance—one which is given under the heading of 'Magical Control of the Sun.' "In ancient Egypt the king, as the representative of the Sun, walked solemnly round the walls of a temple in order to ensure that the Sun should perform his daily journey round the sky without the interruption of an eclipse or other mishap." The purpose of this ceremony is plainly to secure the fructifying influence of the sun in the Egyptian fields; behind this is the motive of hunger. The ceremony further discloses the belief that the sun can be controlled in harmony with the motive. Moreover, it need not be doubted that each day of unbroken sunshine was attributed by the Egyptians to the efficacy of the ritual performance.

It is not difficult to recognise the essential similarity, psychologically, between this regal circumambulation on behalf of the potentially hungry Egyptians and the visualisation of its mother by a hungry babe. In the latter case, feelings of hunger provoke the idea of food-breast-mother: in the former, similar feelings conjure up the idea of food-earth-sun. This calls forth in the one the imaginary mother, in the other the imaginary sun as represented by the king. In both the object of desire appears as subject to the control of human feeling. In both is implicit the assumption that the real mother and the real sun respectively present themselves in response to this need, *i.e.* that the objective world is controlled in the interests and at the behest of desire. But the case of the Egyptians is admittedly magic: the case of the infant, therefore, is of the same order.

It is only to be supposed that all children must pass through a belief in magic before they attain, by the help of their peripheral senses and guided by the reality principle, an understanding of the laws of nature, their immutability, permanence and independence of human wishes. Alike in the evolution of the human race and in the development of the individual child, magic is a direct consequence of the psychical function of the revival and projection of memories[2]. A further correspondence between phylogeny and ontogeny is to be noted. Evolutionally, the belief in magic gives place to religious belief; and religion in its earlier stages includes a belief in souls and in spirits and demons (these being later replaced by gods, and, later still, by a single god). Children, too, regularly pass through a stage, usually extending over several years, in which they believe in fairies, goblins, ogres, ghosts, etc. and show all the facility of primitive man in personifying inanimate objects[1]. The belief in souls arose, according to most writers, out of the experience of dreams in which absent or even dead persons appeared, *i.e.* out of the phenomenon of visual hallucinations. Similarly, spirits, demons and gods, which, like fairies, etc., can be nothing more than projected visual impressions, owe their place to the same psychical mechanism. In this connexion, recalling how children hallucinate most vividly in the dark, we may note as a suggestive fact that spirits and demons have most often been seen by night or in the dark recesses of trees, woods, caves, etc.—*i.e.* under conditions of light most favouring visual hallucinations[2].

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[1] See Sir J. G. Frazer's *The Magic Art and the Evolution of Kings*, 3rd ed. 1913, I. 312, etc. See also Freud's *Totem and Taboo Resemblances between the Psychic Lives of Savages and Neurotics*, Moffat, Yard and Co., New York, 1918, 138 *et seq.*

[2] In a remarkable paper, Ferenczi, approaching the subject from another direction, recognises in infantile psychology a "period of magic-hallucinatory omnipotence"—a stage corresponding with that dealt with above (see *Internat. Zeitschrift für Aertzliche Psychoanalyse*, I. no. 2).

VI.

Projection of feeling: rationalisation and magic. It is well known that savages hold their spirits and demons to be friendly or unfriendly (*i.e.* good or evil) which means that they are animated by love or hate[3]. The close psychical parallels that are everywhere found to exist between savages and children would suggest that the infantile mind similarly confers feelings on its own creations. The question therefore which presents itself is, Does the child mind endow hallucinated objects with feelings? If so, What determines the kind of feeling on each occasion?

Direct evidence bearing on this in the earliest years is, of course, hardly to be obtained, but so soon as children are able to describe their experiences of this kind no doubt can remain that, like savages, they believe the hallucinated objects to be animated by feelings towards themselves. In general these visions are either pleasant or frightening. To take one of the latter as an example—say, an ugly old woman seen in the dark near the bed—the child's explanation is sure to be that the sight of the woman suggests the thought that she is bent on harming him (*i.e.* is animated by hostile feelings) and that on this account he is frightened. But this, of course, is a complete inversion of the sequence of events which is, (1) fear begins to stir in the child, (2) this activates memories already associated with fear, (3) these impressions are projected, (4) the hallucination is now regarded as the cause of the fear, (5) the whole is capped by a false explanation, namely, that the figure is animated by hostile intentions.

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[1] Even the parts of their own bodies are regularly personified. Thus, a child of four years old was warned against swallowing gristle with his meat. Thereafter he came to regard his uvula as a little red man on guard in his throat to see that no gristle was allowed to pass.

[2] Pan himself dwelt in forests. Sir J. G. Frazer (*The Scapegoat*, 3rd ed. 1914) has collected accounts from all parts of the world of the occurrence of demons, and reading his pages the eye catches repeatedly such phrases as: at night, after dark, especially at night, in the mysterious darkness after sunset, at nightfall; while the habitats of the demons as given by different tribes and races include: thickets, hollow trees, great shady trees, bush, forest, woods especially, groves, jungle, forest glades, rocky places, ravines, depths of rivers, lakes or sea, caves, holes and dens of the earth, wells, steep mountain-sides (flat, featureless surfaces suitable to receive projected images), shadows of ruins, etc.

[3] In point of time, evil spirits are usually an earlier creation than benevolent ones. This is to be explained by the operation of the psychical mechanism of 'repression.'

In the case of a pleasant hallucination—*e.g.* the image of the mother projected by a hungry infant—it is permissible to infer a similar mechanism, once the child has gained some experience of the outer world and feels the need of accounting for its feelings. If it could speak, it would explain that the sight of the figure suggests the thought that the mother is coming to suckle it (*i.e.* is friendly) and that on this account its desire (= love) is stirred. Yet here, as before, the sequence is (1) desire for the mother, (2) revival of memories of previous occasions when the desire has been experienced, (3) projection of these memories, (4) the hallucination is accepted as the cause of the desire, (5) the assumption is made that the mother image is actuated by friendly feelings.

These five steps may be traced in all hallucinations, whether pleasant or the reverse, and the first part of our question is answered affirmatively: hallucinated objects are endowed with feelings by a child. In other words, projection of images is accompanied by 'projection of feeling.' This new psychical mechanism of projection of feeling is destined to play so large a part throughout later life, normal as well as pathological, that a few observations on its first occurrence is desirable.

That a projected image is held to be inspired by feeling is due, as we have seen, to the child's need of accounting for its own feeling. It is a false explanation which seems to explain and is accepted as explaining feeling (and behaviour) which in fact arise from another cause, of whatever kind, which is not recognised. It is justifying behaviour which is itself irrational by an explanation which passes as rational. It is, therefore, the earliest example of what Ernest Jones has called 'rationalisation'[1]. As manifested in adults—and it is a process as widespread as humanity itself and can be recognised in the reasons proffered by almost all people for their opinions and behaviour in matters great and small, from religion and politics down to the trifling affairs of everyday life—it represents the attempt to explain behaviour, which is based on unrecognised sources of feeling, on grounds which are intellectual and have reference to the outer world. It endeavours to credit to the reality principle what is in fact an indulgence of the pleasure principle.

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[1] "Rationalisation in Everyday Life," by Dr Ernest Jones, *Jl. of Abnormal Psychology*, III. no. 2.

Applying this to childhood, we see that the projection of feeling arises from a conflict between the inner needs and the outer reality, and is an attempt to bring the two into harmony. It can come into operation only after some experience of the peripheral senses has been obtained, and at a time when the child is not only already intimately familiar with perceptions originating within but accepts them as psychic truth—the only reality with which it is familiar. It aims, therefore, at subordinating the outer world to these inner perceptions. Its relation to magic is thus apparent.

It may further be said that rationalisation itself represents a modern persistence of the practice of magic. Its function is to pick out, reject, invent, twist or otherwise modify the evidence of the senses until it squares with the inner feelings. It, too, aims at subordinating the outer world to inner perceptions. In illustration of this we may take the case of a man who, on account of niggardliness, is unwilling to give a donation which has been asked of him; without acknowledging his motive he invents and offers in excuse half-a-dozen reasons which he believes to be the real ground of his refusal. Compare this rationalisation with *e.g.* the means taken by various primitive people to guard their cattle from evil spirits. In one country the peasants set thorny branches or sharp pitchforks against the cow sheds; in another they chalk crosses on the doors of their cattle stalls; elsewhere they stick twigs of birch in the dung heaps[1]. In each of these the wish to keep their property inviolate impels them to look for and use material objects which by suggesting protection give a feeling of security; therein lies their magic and they serve the same function as the rationalisations of the civilised man.

With regard to the kind of feeling that is projected on any occasion. We have seen in the case of the hungry infant that its desire for the mother becomes projected on to her as her desire for the child; that is, love is projected as love. But in the case of the ugly old woman fear is projected as hate. In each example rationalisation is reponsible for the projection, and the kind of feeling that is projected is that which would account for the internal feeling[1].

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[1] See "The Public Expulsion of Evils," pp. 160–2, in Sir J. G. Frazer's *Scapegoat* 3rd ed. 1914.

VII.

It was remarked earlier how very little attention has been paid to these visual phenomena; even those most directly concerned with questions of child psychology have passed them by practically unheeded. Yet the part they play in shaping and giving direction to the earliest stages of psychical development must rank high in importance. Not only are they constantly experienced by all children and therefore must fill a large part in their mental life, but the fact of their external projection must contribute more than anything else to enlarging and expanding the mental horizon to include what is external to the child. Moreover, the essential differences between a visual hallucination and a visual impression of an actual external object must confront a child with problems, confusions and contradictions, the understanding of which will do very much in helping forward its intelligence. Nevertheless, for a long time its attitude towards the outer world is based only on its experience of psychical reality, and is so unlike what later it will come to be that it is difficult for an adult to even picture it. It is a period when the imagination is unchecked by any contact with experience of objective reality, when the impossible happens as a matter of course, and the possible bears no distinctive feature by which it can be recognised. The world is not divided into real and imaginary; everything is magical, and there is no wish but what it comes true.

One last point. Are not the facts set out above enough to account in themselves for the fear which many children have of the dark? The relation between darkness and visual projection is intimate. It is specially in the dark that children, like savages, see apparitions, and these will be pleasant or terrifying. Timid children, *i.e.* those in whom runs a pretty steady undercurrent of fear, will be most liable to see terrifying visions, and therefore will most wish for night lights and companionship. Other children not specially subject to fear will have no such ground for shunning the dark. It seems hardly necessary to look further for an explanation.

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[1] The same process is at work often enough in adult everyday life. For instance, an individual who has taken a dislike to another is apt to assume that the other dislikes him, and may easily go so far as to account for his own antipathy as the result of the feeling which he thinks he has recognised in the other; and yet the second party may all the time be neutral or even friendly.

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A VINDICATION OF THE RESONANCE HYPOTHESIS OF AUDITION.

BY H. HARTRIDGE.

(*From the Cambridge Physiological Laboratory.*)

1. *Evidence is given that a short silence is heard as a short noise* (pp. 277–282).
2. *The perception of high and low notes can be explained if the mass and tension (as well as length) of the vibrating elements of the cochlea are taken into account* (p. 283).
3. *There is no evidence that the number of vibrating elements of the basilar membrane is insufficient to account for the number of different pitches which the ear perceives* (pp. 283–284).
4. *The perception of the staccato ending of a note can be explained on the resonance hypothesis if the effects of successive contrast are taken into account* (pp. 284–286).
5. *The perception of the commencement of a note at full strength is explicable on the resonance hypothesis* (pp. 286–287).
6. *The resonance theory can explain how treble and bass notes sounding and terminating together are heard to begin and end simultaneously* (p. 287).
7. *Summary* (pp. 287–288).

1.

PERRETT[1] has recently advanced a number of arguments against the Helmholtz hypothesis that the organ of Corti consists of structures which act as resonators. The more important of these arguments I propose to consider.

Helmholtz, in order to explain the perception of the separate notes which constitute trills in music, had to assume that, on the cessation of the sound, the resonating elements of the organ of Corti suffer a diminution of the amplitude of their movement to $\frac{1}{10}$ the initial value after 9·5 vibrations. With the voice pitched at 100 vibrations per second, therefore, an interval of ·1 of a second would be required for the intensity of the sensation to be reduced to $\frac{1}{10}$ and a still longer interval for it to be reduced to complete silence.

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[1] Perrett, *Some questions of phonetic theory*, 1919, ch. v, 32.

Perrett states that in the word 'utter' there is a short interval of silence between the two 't's' which can be shown by physical measurements to occupy about ·05 sec. (less than six vibrations with a note of 100 vibrations per second)[1], and that this interval is very much shorter than that required according to Helmholtz for the amplitude of the resonators of the organ of Corti to be reduced to $\frac{1}{10}$.

But the ear, which should not be able to detect this interval according to Helmholtz's estimate, is able to do so since it can differentiate 'utter' and 'udder' in which the gap between the two syllables is bridged by phonation.

Perrett therefore contends that the hypothesis of Helmholtz requires two incompatible conditions, for, in order to explain trills, a noticeable silence must not occur unless the silent interval is longer than the time period of ten vibrations, whereas to recognise the silent interval in 'utter' one corresponding in its length to less than six vibrations must be appreciated. But since Helmholtz's hypothesis cannot satisfy both these conditions at the same time it must give place to Wrightson's which can.

Against Perrett's conclusions two criticisms can be advanced: firstly, that the physical method of analysis is not sufficiently rapid or sensitive in its response to show the presence of vocal harmonics considerably higher in key than the fundamental note which may occur during the interval between the two syllables of 'utter.' Secondly, that if a silent interval between the syllables is the feature which causes the ear to differentiate 'utter' from 'udder,' then how is it that the ear can readily detect the difference between 'ut' and 'ud,' both of which are followed by silence?

Two questions, therefore, come up for consideration: firstly, can it be shown by experiment that a short silent interval can be appreciated by the ear, and secondly, if such is the case, can an explanation be found on the basis of Helmholtz's hypothesis?

With regard to the first question, it was found that one method, by means of which a short interval could be obtained, was with a syren consisting of a flat metal disc drilled with a circle of ninety-six equidistant holes, and capable of being rotated while a jet of compressed air was blown against the circle of holes. Preliminary tests having shown that a continuous musical note is obtained in this way, one or more consecutive holes were blocked with plasticine so as to produce an interval of silence of the required duration. Experiment showed that, with the ear within a foot or two of the disc, the interval caused by two consecutively blocked holes was clearly heard, and that the presence of one blocked hole could usually be clearly recognised. In fact it was found possible to gauge the speed of rotation of the disc by counting the number of times that the blocked hole made its presence heard per minute. Now with the disc rotating three times per second, 288 holes would pass in front of the jet per second and therefore one blocked hole would produce an interval of silence not greater than one 144th (·007) of a second. And with two consecutive holes blocked the interval cannot be greater than one 96th of a second (·01). These experiments, therefore, confirmed Perrett's conclusion that the ear can detect very brief intervals in the emission of sound vibrations, and indeed much briefer intervals than were given by him.

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[1] Perrett, *Nature*, May 16, 1918, 204.

In reference to the above syren experiment, an alternative explanation of the phenomenon might be advanced, namely, that it is not an interval of silence that is heard, but the beat of a tone. Now a beating tone would be produced by the summation of a note of 288 vibrations per second with another of 285 vibrations (since one hole in the disc is stopped), and therefore, beats would be heard occurring three times per second. This, however, is not what the ear perceives. There is not an alternate waxing and waning at the rate of three times per second, but a sharp click which occurs, once per revolution of the disc.

Now if, as Perrett said, the hypothesis of Helmholtz cannot explain the perception of an interval of silence of ·05 second, it would seem that this must apply with still greater emphasis to the shorter interval obtained by myself in the syren experiment. The following explanation, on the basis of Helmholtz's hypothesis, has occurred to me. When a regular series of vibrations falls on a set of resonators, at first a large number of them starts to oscillate in addition to the one which is in exact phase with the incident vibrations. But, one by one, they gradually get out of step with the incident vibration and are brought to rest, until finally only the one in tune with the vibrations is left in motion. If this gradual process of elimination occurs at the beginning of a series of vibrations, why should it not to some extent at all events occur if that vibration is interrupted for a time and then recommences? For this new phase would affect the resonators in the same way, to some extent, as it would have done if there had never been any previous phase, that is, for a short time afterwards other resonators are set in oscillation beside the one in tune with the vibration. The interval is perceived not because of the diminution in the amplitude of the swing of the chosen 'in tune' resonator, but by the feebler (and transient) swings of the 'out of tune' vibrators at the recommencement of the note.

This conclusion appeared to be sufficiently important (from the point of view of the Helmholtz hypothesis) to be worth experimental confirmation. The apparatus used is shown in Plate II (figs. 1—3), and will be seen to consist of a number of simple pendulums hung from a horizontal bar. This bar is given a to and fro motion by means of a rotating shaft, crank disc, connecting rod, etc., as illustrated. The shaft was rotated by an electric motor through suitable gearing. The motor was driven from accumulators so that its speed was moderately constant and could be adjusted between fairly close limits by means of a rheostat. Between the connecting rod and the pendulum mechanism was interposed a device by means of which the motion from the crank could be conveyed to the pendulums or be stopped for any required number of revolutions, but the arrangement was such that the connexion or disconnexion could only occur *at the dead centres*; a period of connexion or disconnexion could thus only consist of a certain number of half-rotations. When using this stopping and starting device, care was always taken that the crank was nearing the top of its throw, and thus each period of connexion or disconnexion consisted of a number of whole rotations. By this device, therefore, the motion conveyed to the pendulums could be started at dead centre, and after a certain number of complete vibrations stopped at dead centre again, and further, the interval of no motion (corresponding to silence) could be made to cover any required number of whole vibrations, while, on re-engaging the mechanism, the vibrations would recommence as before, at precisely the same rate and in precisely the same phase as those of the previous vibrations. By this means, therefore, the effects on a system of resonators of a musical note interrupted by periods of silence could be closely imitated.

In Fig. 1 is shown the effect of four complete vibrations starting from rest, the period of the vibrations being equal to that of the resonator marked *C*. It will be seen that beside the 'in tune' resonator *C*, other resonators on either side are in vibration, and these being out of tune with the 'in tune' resonator *C* will introduce that element of noise which is so noticeable a feature of the note emitted by a syren arranged to produce a series of groups of four consecutive sound waves followed by periods of silence. [In the above description the fact has not been overlooked that when the resonating system consists of a very large number of elements which differ from one another but slightly in period, a pure note will set not one element into oscillation alone, but a small group of

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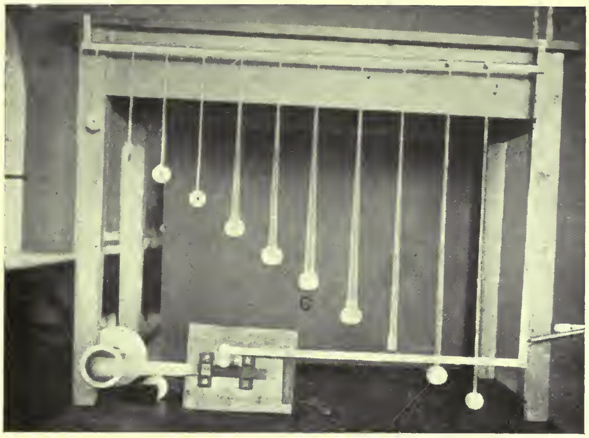

Fig 1.

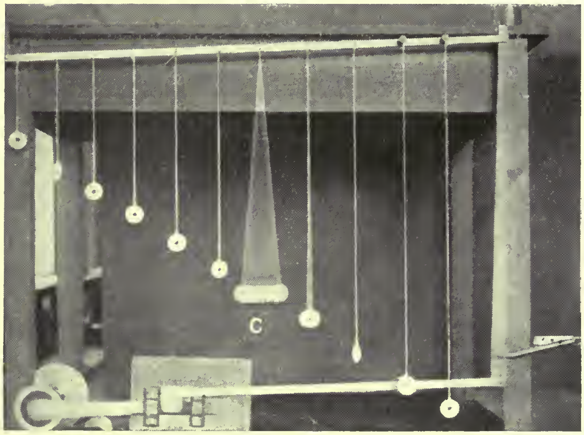

Fig. 2.

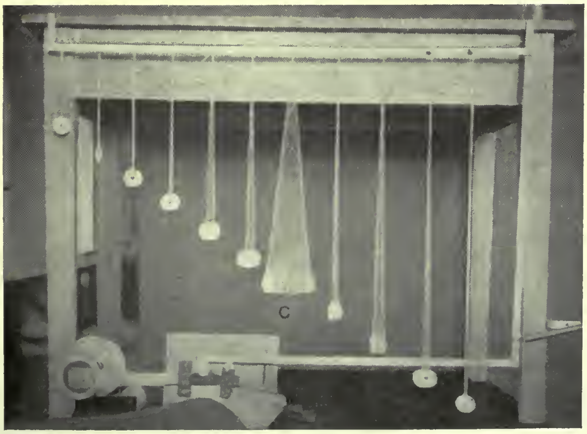

Fig. 3.

elements, the amplitudes of each one of which will obey some such mathematical curve as Maxwell's law of chance. In the above argument such a group of elements differing but slightly in pitch is, for simplicity of description, called the 'in tune' resonator.] If instead of four consecutive vibrations a large number be caused to act on the resonators, then all except the 'in tune' one get in turn out of step with the vibrations and come to rest. This condition is shown in Fig. 2. The small movements of this resonator may be due to the fact that the lever to which the pendulums are attached does not perform true simple harmonic motion because the connecting rod is not infinitely long (as it should be) and, therefore, at the top and bottom of the stroke, it makes a finite angle with the line joining the cross-head (the terms used here are taken from steam engine practice) and the power-shaft. Experiment shows that so long as the to and fro motion of the levers continues, so long does the 'in tune' resonator retain the maximum amplitude of its swing. If the motion of the levers be stopped for one or more whole periods and then started again, the 'in tune' resonator bridges the gap by continuing to swing with but slightly diminished amplitude (as Perrett stated that it would), but the resonators on either side also begin to swing again and continue to do so for a short time, not when the motion of the levers is stopped, but when it is started again (see Fig. 3). Applying these facts to the ear it would appear that it is not the silence which makes itself perceived by the resonators of the cochlea, but the recommencement of the sound waves, and further this recommencement does not show itself on the 'in tune' resonator but on the 'out of tune' ones. These, when they vibrate, introduce noise which begins at and continues for a short time after the recommencement of the sound. Now this conclusion can be tested, by means of the syren, by causing a brief noise to occur once per revolution, at the same time as a uniform note, and by comparing the sensations thus set up in the ear with those perceived when a silent interval occurs in a uniform note as in the first syren experiment. The arrangement used for producing the short noise was as follows: a single hole was drilled to one side of the row of holes previously used, and two compressed air jets were mounted so that one should blow as before on the row of 96 regularly spaced holes, while the other blew on the single hole whenever it came round. It was found that if the pressures of the air jets were suitably adjusted a change could be made from the original arrangement for giving a short silence (described earlier in the paper) to the plan just described for producing a short noise, without the ear being able to distinguish the one from the other. This would therefore seem to be clear proof that the ear does not perceive a short silence *as such*, but as the result of the noise produced when the sound waves recommence.

A further experiment was performed with the syren to demonstrate if possible the existence of a short noise at the commencement of a musical note. To a small loop in a length of fine silk thread was attached a ball of plasticine, which was pressed on to the end of the nozzle through which the compressed air was blown on to the 96 holes. With the syren disc rapidly rotating and with the compressed air turned on, the thread was suddenly jerked, causing the plasticine to leave the jet and thus allowing the air suddenly to impinge on the holes. In this way a uniform musical note was suddenly commenced from silence and the ear could distinctly hear the noise element at the commencement, which made the note commence as if with a 'c' or 'p' or some consonant between the two. Returning, therefore, to Perrett's original contention (that the organ of hearing as Helmholtz pictured it, *i.e.* as a system of resonators, would not be able to perceive the silence between the two 't's' of 'utter'), it would seem that if this interval of silence does indeed occur (and I have advanced above reasons against it), then it is the noise caused by the commencement of the second syllable and not the silence between the syllables that is in point of fact the mark of the interval. In the case of this word and certain other words that Perrett mentions (*e.g.* tick-tack) there are additional factors which affect the character of the recommencement of the second syllable. One of these is that during the vowel of the first syllable the air is being delivered by the lungs in a steady stream through the open air passages. When the consonant marks the end of the first syllable the escape of the air is prevented and therefore there is a rise of pressure of the air contained in the passages on the lung side of the obstruction (until finally such a value is reached that the lungs are brought to rest in spite of the continued contraction of the abdominal and thoracic muscles). Usually the second consonant (which marks the commencement of the second syllable) occurs long before this limiting pressure is reached, but not before it has suffered a considerable rise. On the commencement of the second syllable therefore this pressure inside suddenly returns to its original value with an outward rush of air, and this adds considerably to the element of noise introduced as explained above by the recommencement of the sound vibrations with the beginning of the second syllable,

2.

Perrett (*loc. cit.* p. 10) states, "The basilar membrane being according to Professor Keith less than three times as wide in its termination in the apex as at its beginning in the base of the cochlea, the idea that it contains thousands of resonators accurately graded to respond to differences of one vibration per second or even less, becomes perhaps a little extravagant."

Now the formula for the rate of vibration of a stretched cord is

$$\text{Rate} = \frac{1}{2 \times \text{length}} \sqrt{\frac{\text{Tension}}{MASS \text{ per unit length}}}\,[1].$$

So that beside length, tension and mass are important features. These appear to have been forgotten by Perrett. The argument that the basilar membrane has nearly the same width at the one end as the other, in no way prevents the masses and tensions being such that the one end shall respond to notes of sixteen vibrations and the other end to notes of 50,000 vibrations per second. Further, whatever light histology may be able to throw on the lengths and masses of the various fibres, it would seem difficult to devise any means of measuring the tensions, so that if at some future date an anatomist should find all the fibres of the basilar membrane to have the same length and the same weight that alone would not prove anything either for or against Helmholtz's theory.

3.

Perrett, quoting Auerbach (*loc. cit.* p. 11), states that two successive notes differing by one quarter vibration per second can be distinguished in the best cases. "This would give over the whole audible range a total of 64,000 distinguishable pitches. But the number of vibrating elements in the cochlea is between 4000 and 8000, *i.e.* each fibre must be able to perceive eight to sixteen different pitches. This would appear to be an impossible task."

The validity of this reasoning depends entirely on the reliability of the two values which Perrett gives, *i.e.* that of the number of fibres and that of the number of pitches. With regard to the former McKendrick was satisfied that there were about 11,000 fibres in all[2]. With regard to the latter it should be pointed out that the differentiation of notes differing by one quarter vibration per second may have been due to the ear noticing a difference between the over-tones. Thus, assuming both notes to have over-tones equal to x times their own period, then a difference in these over-tones perceptible by the ear would lead to a differentiation of the notes themselves. Thus, supposing x to equal four, then the ear, by differentiating the over-tones differing by one vibration per second, would give the appearance of differentiating between the fundamental notes themselves which differed by one quarter vibration only.

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[1] Poynting and Thomson, *Sound*, 1904, 88.

[2] McKendrick in Schäfer's *Text book of Physiology*, 1898, II, 1184.

The 64,000 estimate given by Perrett may therefore easily be two, three or four times too great. Further it should be noted that fine differentiation can only occur over a relatively small range, for, in the extreme bass and to a greater extent in the extreme treble, differentiation is much impaired. Relatively few fibres would suffice for audible acuity near the top and bottom limits and this fact greatly reduces the calculated total number that should be present. McKendrick, taking this point into consideration, estimated the total number of different pitches to be roughly 11,000, *i.e.* about the same number as there are vibrating elements of the cochlea. Even if, however, it was found that the number of vibrating elements in a human ear was definitely less than the number of different pitches that the ear of another individual could perceive, that would not constitute evidence against the resonance theory; to obtain evidence it would be necessary to count the number of pitches and the number of fibres for one and the same ear of the same individual and to find the former number so much larger than the latter as to be outside the range of experimental error.

4.

Perrett has inferred (*loc. cit.* p. 36) that the Helmholtz hypothesis is unable to explain how the ear can appreciate the *staccato* ending of a note; he has further stated (p. 38) that it is out of all reason to suppose that if there are resonators in the cochlea a loud note can be reduced to inaudibility in the same time as a soft note. My experiments with the model shown in Plate II indicate that so long as the to and fro motion of the levers continues, so long does the 'in tune' resonator retain the maximum amplitude of its swing. But if the motion be discontinued, then this resonator gradually comes to rest, the other resonators having remained at rest all the time. Now Helmholtz concluded from his experiments on trills and beats that in the case of the resonators in the cochlea the intensity of vibration would be reduced to one-tenth its original value after 9·5 unaided vibrations had occurred following the cessation of the musical note originating them. In the case of the mechanical model shown in the figures, a very much greater number had to occur before the amplitude of the swing was reduced to one-tenth. This in no way invalidates Helmholtz's value, because the resonators of the model, since they swing in air, are very lightly damped; whereas the damping of the resonators of the cochlea is much greater because they are immersed in liquid. When, therefore, a musical note ceases, the amplitude of the 'in tune' vibrator diminishes at a rate which is largely dictated by the degree of damping, and the silence cannot be perceived until the amplitude has so far diminished that, *contrasted* with the amplitude of the 'in tune' vibrators during the sounding of the note, the stimulus to the hair cells has become negligible. In other words, apart altogether from the phenomena (largely physical in their characters) which control the amplitude of vibration of any particular resonator in the cochlea, there are in addition the general physiological characters of the sense organs to be taken into account; since there is no reason to suppose that the hair cells are governed by conditions differing in any way from those which operate in the case of sight or touch. It would be expected therefore that simultaneous and successive contrast, phenomena so marked in their effects in the other sense organs, would operate also in the case of hearing. Thus, if in the case of the eye the illumination of a white surface at which the eye is looking is suddenly reduced to one-hundredth part at first the illumination appears to have fallen to zero, but as the eye adapts itself to the new conditions, the visibility of the white surface rapidly improves again. So, in the case of the ear, if, after a period of twenty vibration intervals from the cessation of a very loud sound, the resonators of the ear are still actually in oscillation, it may easily be that for the hair cells the sensation of motion will virtually have ceased, because the amplitudes of the oscillations have fallen to so small a value compared to those that were observed a few moments before. The statement made by Perrett[1] that the vocal harmonies are audible after the voice has stopped singing, *i.e.* while certain hair cells are suffering from the effects of successive contrast, is readily explained on the ground that resonators and hair cells involved in the perception of the vocal harmonies are different from those affected by the previous sound waves. Further there seems to be no reason why a loud note cannot be reduced to inaudibility in the same time as a soft note, for Helmholtz showed that in ten vibrations the amplitude is reduced to less than one-tenth. The rate of reduction is therefore unaffected by the initial amplitude of the vibrations. Whatever value this may have at first, the time taken for it to fall to a certain fraction will always be the same, in other words the fall is a geometric one. The same would appear to be true in the case of a sense organ affected by successive contrast, namely, that whatever value the initial stimulus may have a reduction to a certain fraction of that stimulus will cause it to fall below the (temporary) threshold value of the sense organ. Combining this with the preceding statement, we may say that the time taken for a stimulus falling geometrically to reach the threshold of a sensory end organ influenced by successive contrast is approximately the same for all stimuli whether they be large or small. That is to say, a loud sound and a soft sound will cease to be heard at the same instant.

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[1] Perrett, *Nature*, Nov. 7, 1918, 185.

5.

Perrett asks (*loc. cit.* p. 25),—Since the more accurately each resonator of the cochlea is tuned the greater will be the number of periodic impulses required to produce the full effect, how then does the resonance theory explain the fact that we can hear a note beginning at full strength and then gradually diminishing in intensity, such as a piano note which is powerful only at the moment when it is struck and rapidly decreases in strength?

It has been shown above (see Plate II, fig. 1) that the first complete vibration (corresponding in its period to fifty-five per minute) is sufficient to bring the top vibrator of eighty per minute completely and the bottom vibrator of thirty-eight nearly to rest. After four vibrations all except the three vibrators above and below the chosen one are at rest as the figure shows. After ten vibrations one only on either side is left in vibration. Even with this mechanical model in which the impulse is conveyed by shifting the point of support first to one side and then to the other of the centre of gravity, the rate at which the 'out of step' vibrators are eliminated is very rapid. If the vibrators could be immersed in liquid as they are in the ear and if the motion could be conveyed to the liquid itself, a much more rapid elimination would occur, because as soon as one of the 'out of step' vibrators lagged behind or got ahead of the oscillation of the liquid, the eddies set up would immediately bring it to rest. Calculation shows that in a beneficial case eight complete vibrations would limit the 'out of step' vibrators to those of one semi-tone on either side of the chosen one (eight complete vibrations would occur in ·03 sec. approximately in the case of the middle *C* of the musical scale). After this short interval of time, therefore, not only is the total number of vibrating resonators limited to one semi-tone on either side of the 'in tune' one, but also, as may be shown by calculation, the latter has attained nearly 90 per cent. of its final amplitude. Any changes in the loudness of the musical note occurring after this interval of ·03 sec. will be faithfully followed (with of course a small time lag). The resonance theory can thus explain how it is that we hear a note beginning at full strength and diminishing in intensity, as easily as we can hear a note beginning soft and becoming louder or another keeping at the same intensity.

6.

Lastly Perrett (*loc. cit.* p. 25) has objected that, since a longer time must be required for the vibrators of the cochlea to reach their maximum amplitude in the case of a low note than in that of a high, "the perception of a bass note on the piano would therefore lag behind that of a note in the treble, if the two were struck simultaneously." It should be noticed, however, that the perception of a sound begins not from the time that its 'in tune' resonator reaches its maximum amplitude, or even from the instant when the eighth vibration has been completed, but from the beginning of the first vibration of that resonator. It may be true that this beginning will consist partly of the noise created by the accompanying vibrations of certain other 'out of tune' resonators, but almost before the hair cells have accommodated themselves to the onset of the note from silence these 'out of tune' resonators have ceased to vibrate and have left the pure note to continue.

7. SUMMARY.

(1) It has been stated that the Helmholtz hypothesis of audition is unable to account for certain properties of the organ of hearing: first, its ability to appreciate a short interval of silence and to differentiate 'utter' from 'udder.'

I find by experiments with a syren and a resonator apparatus that the ear perceives a short silence as a short noise and that this perception is readily explained on the Helmholtz basis.

(2) Objections have been made that the difference in the widths of the different parts of the basilar membrane is quite insufficient to permit the vibration of one part in response to high notes and of another part in response to low. I find that besides width, mass and tension will affect the period of vibration of the basilar membrane. If these are also taken into account the perception of high and low notes is readily explicable.

(3) The statement that the number of separate elements in the basilar membrane is too few to account for the number of different pitches which the ear perceives appears to be without foundation.

(4) The objection that on the Helmholtz basis the ear should not be able to perceive a note ending *staccato* can be met if the effects of successive contrast are taken into account.

(5) The resonator theory can explain the perception of a note beginning at full strength.

(6) The resonator theory can also explain how it is that treble and bass notes sounded and terminated together are heard to begin and to end simultaneously.

(*Manuscript received* 6 *November*, 1920.)

A MINOR STUDY OF NYCTOPSIS.

By J. C. FLÜGEL.

(*From the Psychological Laboratory, University College, London.*)

I. Introduction.

Although the measurement of an individual's ability to see in daylight has for some considerable time constituted an essential part of most medical examinations, the corresponding measurement of Nyctopsis, or Night Vision, has usually received but scant attention. During the War, however, it became manifest that it was a matter of considerable military importance to appoint to certain tasks only such persons as possessed good powers of seeing in the dark and to exclude from these tasks all who suffered from nyctalopia or whose nyctoptic ability fell below a certain standard. It therefore became necessary to devise some test by which individuals with the required nyctoptic qualifications could be conveniently selected. With this end in view, a study of Nyctopsis was included in the various branches of war research undertaken in the Psychological Laboratory of University College, London, under the direction of Professor Spearman; the results here recorded being a portion of the data obtained from this study.

In this paper we shall deal only with the *relative* abilities of different individuals and with such conclusions as can be drawn from a consideration of their relative abilities. The accurate determination of an individual's absolute ability, in terms of stimulus intensity, and of the shape of his 'adaptation' curve—depending as these do upon delicate photometric measurements—is a matter which demands very considerable care and precision and which could not be undertaken with the desirable degree of thoroughness at a time when all the energies of the very small laboratory staff were required for the solution of urgent practical problems in a variety of fields. The consideration of these matters will therefore be left to a second paper[1] by my colleague Dr Ll. Wynn Jones, who continued under more convenient circumstances the work begun by me and whose method permitted of greater accuracy and refinement than mine. Measurements of relative nyctoptic ability are, however, comparatively easy and the results obtained in these earlier experiments of mine are, it would appear,—in spite of the necessarily somewhat rough and hurried manner in which these experiments were carried out—not altogether wanting in accuracy or in interest. In view of the comparatively little study that has hitherto been devoted to the subject, the publication—in brief form—of these results, as a preliminary to the report of Dr Wynn Jones's investigations, seems therefore to be justified.

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[1] This *Journal*, 1921, XI, 299–319.

Before passing on to a description of the experiments, I take the opportunity of acknowledging my indebtedness to Prof. Spearman, without whose very valuable guidance and assistance the research could not have been made, and to Dr Wynn Jones who, as already indicated, carried on the investigation when—owing to other claims upon my time—I was no longer able to do so, and who freely gave me the benefit of his advice as to the form in which the results could most conveniently be dealt with and expressed.

II. METHODS.

Four distinct experiments were undertaken in the work here reported.

(1) Visual acuity in ordinary daylight, as measured in the usual way by the ability to read Snellen's types at a distance of six metres, the individual ratings being made on the basis of the number of letters correctly read. For convenience of description this test will be referred to as θ[1].

(2) Light sensitivity, *i.e.* ability to distinguish light from darkness. A 'pointolite' electric lamp of 100 candle power was enclosed in a box with a single aperture which permitted the rays from the lamp to illumine a square patch of the (black) wall of the dark room, the patch being of such a size that it subtended an angle of 30° from the eye of the observer. The intensity of the illumination could be varied within very wide limits by means of a suitable resistance, and the aim of the experiment was to determine the lowest illumination at which the observer could just distinguish whether the patch was illuminated or not. For this purpose the rays of light were intercepted at irregular intervals by a black cardboard screen being placed noiselessly in front

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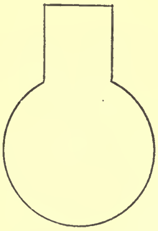
[1] The abbreviation employed to designate this and the other tests agrees with that used for the same purpose for the most nearly corresponding tests of Dr Wynn Jones; but when comparing the present results with those obtained by him, it must throughout be borne in mind that the conditions under which the two sets of results were obtained only roughly correspond, there being in every case important differences between his experiments and mine.

of the aperture of the box (and in such a position as to be itself invisible to the observer); the observer receiving the instruction to say "Dark" whenever the light was intercepted and "Light" whenever the intercepting screen was removed. For convenience of reference this experiment will be designated *P*.

(3) Visual acuity in dim light, as measured by the ability to perceive the position of certain figures. The figures chosen for this purpose were roughly of the shape of a square resting on a circle (as indicated in the accompanying diagram), the side of the square being of the same length as the radius of the circle. Twelve such figures of varying size were constructed, the smallest having a radius of ·66 mm., the largest one of 76 mm.; each figure being of such a size that its radius was longer by approximately 50 per cent. than that of the next smaller figure. The figures were cut out of stiff white cardboard and were fixed (in three columns and in order of their magnitude) to a large sheet of dull black cardboard in such a way that they could be freely rotated about the centre of their circular portions but would yet remain in whatever position they were placed. The task of the observers was to detect the position of the projecting square portions of the figures. The observers' judgments were communicated to the experimenter by treating the projections as if they were the hour hands of clocks; thus an upward vertical position of the projection was designated '12 o'clock,' a downward vertical position '6 o'clock,' a rightward horizontal position '3 o'clock,' etc. The whole series of figures on their cardboard mount was evenly illuminated by the 'pointolite' lamp, which (in contra-distinction to experiment *P*) was made to give a light of constant intensity throughout the experiment, this intensity being in the neighbourhood of ·1 candle power. This experiment will be referred to as *X*.



(4) This experiment (which will be designated *Y*) was in all respects similar to *X*, except that the illumination was increased by about 100 per cent., corresponding thus roughly to ·2 candle-power.

The actual procedure adopted in the experiments was as follows:

The observer, having arrived in the laboratory in a light-adapted condition (all the experiments were conducted in the morning or early afternoon), was immediately put through the Snellen daylight test. He then entered the dark room and for a period of 2 minutes gazed continuously at a large white sheet illuminated by the rays of an electric lamp of 1000 candle power; the object of this being to ensure approximate uniformity of light-adaptation before beginning the process of dark-adaptation. The observer then took his seat at a window communicating with a second dark room, in which was placed the apparatus already described and in which the experimenter was stationed. This window was normally covered by a close-fitting dark curtain, so that no light penetrated to the eye of the observer, even when the experimenter's room was brightly illuminated.

The 1000 candle power lamp being extinguished, the observer waited in the darkness during a period of 1 minute, after which, on receiving a signal from the experimenter, he lifted the curtain and gazed upon the illuminated patch on the dark room wall in preparation for experiment *P*. This illuminated portion of the wall was situated directly opposite the observer's window and at a distance of 2 m. from his head, he being provided with a chin rest in order that this distance might remain constant throughout the experiments. The experimenter now proceeded to intercept the light in the manner described. After four movements of the intercepting screen (*i.e.* two of interception and two of withdrawal), he reduced the intensity of the illumination by a predetermined amount (this being facilitated by means of small strips of luminous paper pasted on to the resistance apparatus at certain points) and repeated the four movements of the screen. This procedure was continued until the observer had failed to report (or had wrongly reported) six movements in succession.

The experimenter then instructed the observer to close the curtain. As soon as this was done, he turned on the ordinary lighting in his own room, noted the threshold of the observer (in terms of resistance), recorded the total number of correct judgments and quickly made the necessary preparations for experiment *X*, by placing the cardboard sheet with the white figures in the path of the rays from the 'pointolite' lamp and by adjusting the intensity of the light from this lamp to the (higher) degree required for the new experiment. He then extinguished the ordinary room lights and gave the signal to the observer for the curtain to be raised; whereupon experiment *X* began. During this the experimenter pointed with a small wand of luminous paper to each of the figures in turn (from large to small progressively), the observer indicating in the way already mentioned what he judged to be the position of the figures. The experimenter counted the number of correct judgments and, at the close of the series, interposed his body between the observer's window and the figures and quickly removed these latter into fresh positions; after which the process of judging immediately began again. Two such repetitions were undertaken, making three series of judgments in all.

The total number of correct judgments in the three series afforded the measure of the observer's ability[1].

The curtain was then closed once more, whereupon the experimenter adjusted the lighting to the (still higher) intensity required for experiment *Y*. Experiment *Y* was then conducted in precisely the same manner as experiment *X*.

With a little practice on the part of the experimenter, it was found possible to conduct the whole set of experiments *P*, *X* and *Y*, as just described, in a period not exceeding 2½ minutes in duration. This set of experiments being completed, the observer rested until there had elapsed a period of 10 minutes from the time when he had been first exposed to the darkness. The whole set of experiments *P*, *X* and *Y* was then repeated and similar repetitions were undertaken after the lapse of 20, 30, 40 and 50 minutes. Six measurements were thus obtained for each kind of nyctoptic ability, so that the observers could be compared with one another at all stages of adaptation. The whole test for each individual (including the daylight experiment (θ), and the preliminary adaptation with the 1000 candle power lamp) occupied thus about one hour.

All the experiments (including θ) were performed binocularly.

Twenty-three observers took part in the experiments, of whom five were personal friends of the experimenter, fourteen students of University College and four boys from Stanhope Street (London County Council) School—eleven of these being males and twelve females, the ages varying from about 12 to over 70 years. I am much indebted to all the observers for their kind and patient collaboration.

III. RESULTS.

The results of the experiments were in the first place recorded in a table representing the number of correct judgments made by each observer in every test. This table need not be reproduced here, as the results would have to be translated into luminosity values before they would become of general interest in themselves, and this process of translation would, for the reasons already stated, be of somewhat doubtful accuracy in the present case. The conclusions that concern us here can be most conveniently expressed in the form of correlation coefficients. Such coefficients (calculated by the product-moments formula) were therefore worked out for all possible correlations between the different tests and are presented in the accompanying table, in which θ, P, X and Y indicate the experiments above described, while the numbers after these letters refer to the time that had elapsed between the beginning of dark-adaptation and the beginning of the particular test concerned.

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[1] To save time, whenever it appeared that the largest figures could be judged without difficulty, no judgments were made for these figures, the score being calculated as if correct judgments for these figures had actually been recorded; similarly, whenever three incorrect judgments in succession had been recorded, no further judgments were made in that series, the score being counted as if no judgments (or incorrect judgments) had been made for the remainder of the figures.

The principal conclusions that may be drawn from a study of this table are as follows:

(1) A satisfactory guarantee of the reliability of the experiments for our present purpose is afforded by the very high correlations between the measurements for 40 mins. and 50 mins. in the case of P, X and Y. Thus $\frac{P\,40}{P\,50}$ (as the correlation between $P\,40$ and $P\,50$ may conveniently be written) $= \cdot 95$, $\frac{X\,40}{X\,50} = \cdot 87$ and $\frac{Y\,40}{Y\,50} = \cdot 94$.

These high correlations show at the same time that for practical purposes in examining individuals from the point of view of nyctoptic ability, it is probably unnecessary to prolong the examination beyond 40 minutes.

(2) This last consideration leads naturally to the question as to whether a shorter period than 40 minutes will suffice for such examination. Our present data are of course insufficient for a final answer to this question, but some interesting results bearing on the problem are afforded by an inspection of the inter-correlations between the different measurements of P, X and Y respectively (shown in the three sections of the table running diagonally from top-left to bottom-right). The general tendency to positive sign and the frequency of high values show at once that the order of the twenty-three observers tends to be the same in the successive tests and that some information as to the true position of any individual is therefore provided by nearly every one of the tests. If, with a view to greater precision, we take the measurements after 50 minutes as being those most nearly representing nyctoptic ability after complete dark-adaptation (and as being therefore the most important—both theoretically and practically—for the majority of purposes) and compare with these the measurements after 40, 30, 20, 10 minutes and 1 minute respectively, we note that there is a tendency for the correlations to decrease steadily in magnitude (see sixth or bottom row of each of the above-mentioned sections of the table). The same statement holds good, if, instead of using the 50 minute measurements as our

Table of Correlations between (1) *visual acuity in daylight* (θ); (2) *light sensitivity* (*P*); (3) *visual acuity in a dim light* (*X*); (4) *visual acuity in a slightly brighter light* (*Y*); *after varying periods of dark-adaptation* (*figures represent adaptation times in minutes*). (*Probable errors are indicated in brackets.*)

	θ	*P* 1	*P* 10	*P* 20	*P* 30	*P* 40	*P* 50	*X* 1	*X* 10	*X* 20	*X* 30	*X* 40	*X* 50	*Y* 1	*Y* 10	*Y* 20	*Y* 30	*Y* 40	*Y* 50
θ		—·17 (·137)	—·06 (·140)	·08 (·140)	·06 (·140)	·08 (·140)	·02 (·141)	·47 (·11)	·73 (·065)	·83 (·044)	·71 (·07)	·76 (·06)	·79 (·054)	·66 (·08)	·62 (·085)	·64 (·082)	·60 (·09)	·63 (·085)	·56 (·097)
P 1	—·17 (·137)		·43 (·115)	·02 (·141)	—·35 (·124)	—·32 (·124)	—·26 (·13)	·19 (·137)	·02 (·141)	·12 (·138)	·08 (·140)	—·06 (·140)	·08 (·140)	·10 (·139)	·11 (·139)	·26 (·13)	·10 (·139)	·05 (·140)	·02 (·141)
P 10	—·06 (·140)	·43 (·115)		·14 (·137)	·49 (·112)	·31 (·124)	·44 (·115)	·27 (·13)	·27 (·13)	·22 (·135)	·19 (·137)	·08 (·140)	·04 (·140)	·26 (·13)	·21 (·135)	·35 (·124)	·34 (·124)	·36 (·124)	·37 (·123)
P 20	—·08 (·140)	·02 (·141)	·14 (·137)		·79 (·054)	·69 (·074)	·65 (·083)	·35 (·124)	·10 (·138)	·36 (·124)	·07 (·140)	·16 (·137)	·07 (·140)	·24 (·135)	·26 (·13)	·26 (·13)	·29 (·129)	·24 (·135)	·34 (·124)
P 30	·06 (·140)	—·35 (·124)	·49 (·112)	·79 (·054)		·85 (·04)	·77 (·06)	·28 (·129)	·38 (·123)	·26 (·13)	—·10 (·139)	·11 (·139)	·02 (·141)	·38 (·123)	·41 (·118)	·35 (·124)	·36 (·124)	·35 (·124)	·36 (·124)
P 40	·08 (·140)	—·32 (·124)	·31 (·124)	·69 (·074)	·85 (·04)		·95 (·014)	·13 (·138)	·09 (·138)	·01 (·141)	—·07 (·140)	·19 (·137)	·09 (·140)	·23 (·135)	·35 (·124)	·25 (·135)	·39 (·123)	·32 (·124)	·36 (·124)
P 50	·02 (·141)	—·26 (·13)	·44 (·115)	·65 (·083)	·77 (·06)	·95 (·014)		·10 (·138)	·08 (·140)	·02 (·141)	—·06 (·140)	·22 (·135)	·11 (·139)	·16 (·137)	·29 (·129)	·34 (·124)	·37 (·123)	·33 (·124)	·45 (·115)
X 1	·47 (·11)	·19 (·137)	·27 (·13)	·35 (·124)	·28 (·129)	·13 (·138)	·10 (·138)		·27 (·137)	·11 (·139)	·27 (·13)	·26 (·13)	·33 (·124)	·55 (·099)	·52 (·105)	·43 (·114)	·26 (·13)	·20 (·135)	·35 (·124)
X 10	·73 (·065)	·02 (·141)	·27 (·13)	·10 (·138)	·38 (·123)	·09 (·138)	·08 (·140)	·27 (·13)		·77 (·06)	·35 (·124)	·61 (·087)	·69 (·074)	·65 (·083)	·62 (·087)	·63 (·085)	·68 (·076)	·75 (·06)	·74 (·065)
X 20	·83 (·044)	·12 (·138)	·22 (·135)	·36 (·124)	·26 (·13)	·01 (·141)	·02 (·141)	·11 (·139)	·77 (·06)		·74 (·065)	·76 (·06)	·72 (·07)	·61 (·087)	·55 (·099)	·59 (·091)	·79 (·054)	·65 (·083)	·61 (·087)
X 30	·71 (·07)	·08 (·140)	·19 (·137)	·07 (·140)	—·10 (·139)	—·07 (·140)	—·06 (·140)	·27 (·13)	·35 (·124)	·74 (·065)		·85 (·038)	·80 (·05)	·40 (·118)	·48 (·112)	·50 (·105)	·47 (·11)	·46 (·11)	·44 (·118)
X 40	·76 (·06)	—·06 (·140)	·08 (·140)	·16 (·137)	·11 (·139)	·19 (·137)	·22 (·135)	·26 (·13)	·61 (·087)	·76 (·06)	·85 (·038)		·87 (·037)	·46 (·11)	·53 (·105)	·55 (·099)	·59 (·091)	·54 (·099)	·50 (·105)
X 50	·79 (·054)	·08 (·140)	·04 (·140)	·07 (·140)	·02 (·141)	·09 (·140)	·11 (·139)	·33 (·124)	·69 (·074)	·72 (·07)	·80 (·05)	·87 (·037)		·53 (·105)	·61 (·087)	·61 (·087)	·67 (·076)	·63 (·085)	·60 (·087)
Y 1	·66 (·08)	·10 (·139)	·26 (·13)	·24 (·135)	·38 (·123)	·23 (·135)	·16 (·137)	·55 (·099)	·65 (·083)	·61 (·087)	·40 (·118)	·46 (·11)	·53 (·105)		·88 (·037)	·77 (·06)	·80 (·05)	·74 (·065)	·77 (·06)
Y 10	·62 (·085)	·11 (·139)	·21 (·135)	·26 (·13)	·41 (·118)	·35 (·124)	·29 (·129)	·52 (·105)	·62 (·087)	·55 (·099)	·48 (·112)	·53 (·105)	·61 (·087)	·88 (·037)		·85 (·038)	·85 (·038)	·82 (·045)	·86 (·036)
Y 20	·64 (·082)	·26 (·13)	·35 (·124)	·26 (·13)	·35 (·124)	·25 (·135)	·34 (·124)	·43 (·114)	·63 (·085)	·59 (·091)	·50 (·105)	·55 (·099)	·61 (·087)	·77 (·06)	·85 (·038)		·90 (·027)	·93 (·019)	·89 (·037)
Y 30	·60 (·09)	·10 (·139)	·34 (·124)	·29 (·129)	·36 (·124)	·39 (·123)	·37 (·12)	·26 (·13)	·68 (·076)	·79 (·054)	·47 (·11)	·59 (·091)	·67 (·076)	·80 (·05)	·85 (·038)	·90 (·027)		·96 (·012)	·96 (·012)
Y 40	·63 (·085)	·05 (·140)	·36 (·124)	·24 (·135)	·35 (·124)	·32 (·124)	·33 (·124)	·20 (·135)	·75 (·06)	·65 (·083)	·46 (·11)	·54 (·099)	·63 (·085)	·74 (·065)	·82 (·045)	·93 (·019)	·96 (·012)		·94 (·017)
Y 50	·56 (·097)	·02 (·141)	·37 (·123)	·34 (·124)	·36 (·124)	·36 (·124)	·45 (·115)	·35 (·124)	·74 (·065)	·61 (·087)	·44 (·118)	·50 (·105)	·60 (·087)	·77 (·06)	·86 (·036)	·89 (·037)	·96 (·012)	·94 (·017)	

standard, we use the 40 minute or 30 minute measurements for this purpose (fifth or fourth rows respectively). This indicates that the shorter we make the experiment, the less accurate our results will be—as measurements of the observers' ultimate nyctoptic power. Just how much the experiments can be shortened depends of course upon the degree of accuracy required. From our present results (which of course are insufficient for the establishment of any definite rule) it would appear that in the case of P fairly accurate measurements can be taken after 20 minutes and in the case of X after 10 minutes, while in the case of Y even the first measurement—that taken after 1 minute—correlates highly with the ultimate record after 50 minutes.

An inspection of the original table of the records of the observers' performances (not given here) reveals the fact that the relatively low (or negative) correlations of P 1 and P 10 with P 30, P 40 and P 50 are largely due to the results obtained, (*a*) from one observer who suffered from unusually slow power of adaptation and who therefore did badly in the earlier tests, but who ultimately took a high place as regards nyctoptic ability, and, (*b*) from three observers who, though good at initial adaptation, made very slow progress in the later tests. If these results can be regarded as at all typical, they would seem to indicate that there are two small classes of observers whose anomalous behaviour reduces the correlation between early and later measures of light sensitivity, *i.e.* those whose curves of adaptation show an unusually slow or an unusually rapid rise at the beginning, as compared with their ultimate performance[1].

(3) Visual acuity in daylight (θ) does not correlate significantly with light sensitivity (P) at any adaptation time. Practically, this result is of considerable importance, as it shows that no inference whatever can be drawn from a person's performance in the usual Snellen test as to his ability to see in a very dim light. Theoretically, it is of interest as affording evidence for the independence of the mechanisms involved in the two kinds of visual ability—an independence such as is demanded by the Duplicity Theory of Vision.

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[1] It is noteworthy that three of the four observers here referred to have the three lowest of all our twenty-three records for daylight vision (θ). If there is any significance in this, it is probable that experimentation with a selected group of persons who had previously passed a fairly rigorous test for daylight vision would reveal a higher correlation between the earlier and later results than was found in the present investigation. The results obtained by Dr Wynn Jones lend some support to this view.

A correlation between faulty visual acuity in daylight and anomalous dark-adaptation would of course be of considerable interest on its own account, if proved.

(4) Visual acuity in daylight (θ) correlates moderately with visual acuity in a dim light (both X and Y). Practically, this means that those who do well in the Snellen test tend to have good visual powers also in a rather feeble light, though no safe inference can be made in any individual case. Theoretically, this result is of course perfectly reconcilable with the Duplicity Theory, if we suppose—as we should probably do in any case—that both rods and cones are to some extent active under illumination of the intensity employed in experiments X and Y, and that the mechanism of refraction which plays such an important part in θ is also to some extent operative in X and Y. The fact that θ tends to correlate somewhat more highly with X than with Y would be difficult to account for, if corroborated, but the significance of this difference is doubtful in the present case, as it is only about twice the size of its probable error.

(5) Light sensitivity (P) tends to correlate slightly with visual acuity in dim light (X and Y). From the practical point of view this correlation (if genuine) is quite insufficient as a ground for drawing any helpful inference from an individual's performance in X or Y as to his ability in P or *vice versa*. Theoretically, this result may perhaps be regarded as complementary to that recorded under (4). Just as there it appeared that the mechanisms involved in daylight vision were probably to some extent involved in vision in a rather dim light, so here it seems that the latter vision involves also to some extent the mechanisms that come into play in dealing with visual stimuli of quite minimal intensity; though it should be noted that the participation of these latter mechanisms in dim vision is (if we may judge from the magnitude of the correlations) less than that of the daylight mechanisms $\left(i.e.\ \frac{P}{X} < \frac{\theta}{X} \text{ and } \frac{P}{Y} < \frac{\theta}{Y}\right)$.

(6) Visual acuity in dim light (X) correlates only moderately with visual acuity in dim light of rather higher intensity (Y).

This result (which is not due merely to the unreliability of the measures correlated, as is shown by the—quite significantly—higher values of the inter-correlations within the X and Y groups themselves), if corroborated, is of some practical importance, since it indicates the necessity for caution in inferring from a person's ability in one degree of dim light his ability in illumination of rather higher or lower intensity.

Theoretically, we should be inclined at first sight to account for the relatively low correlations between X and Y, as compared with the higher ones within the X and Y groups themselves, on the assumption that X and Y differed as regards the relative proportions in which the mechanisms of daylight vision (corresponding to θ) and the mechanisms of true night vision (corresponding to P) were involved. Thus it would be easy to suppose that X involved the latter (P) mechanisms to a greater extent and the former (θ) mechanisms to a less extent than did Y. As against this interpretation, however, it must be pointed out that the correlations between P and Y are higher than those between P and X and that (as we have already observed) the correlations between θ and X are higher than those between θ and Y—in both cases contrary to what on *a priori* grounds might have been expected (though in both cases the differences in question are only of about twice the magnitude of their probable errors). Possibly further investigation will demonstrate (as has so often been the case before) that the real state of affairs is more complex than had been contemplated by current theories and that instead of one there are two or more distinct nyctoptic mechanisms which come into play at different degrees of stimulation.

The present data are however quite insufficient to serve as the basis of any elaborate theoretical constructions. All I can do here is to point out that the method here adopted (*i.e.* the use of correlation with data obtained by a relatively rough and simple procedure) is capable of throwing very considerable light upon questions of both practical and theoretical importance within the sphere of psycho-physics with which I have been dealing.

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A METHOD OF MEASURING NYCTOPSIS WITH SOME RESULTS.

By LL. WYNN JONES.

(*From the Psychological Laboratory, University College, London.*)

I. *Introduction* (pp. 299–301).
II. *Apparatus for testing nyctopsis* (pp. 301–304).
III. *Method of procedure* (pp. 305–308).
IV. *Results* (pp. 308–316).
V. *Summary of results* (p. 316).
VI. *Tables* (pp. 317–319).

I. INTRODUCTION.

A TEST for Nyctopsis or Night-vision has not hitherto been included among the visual tests which are usually employed in vocational selection. It is true that taxi-cab drivers have to pass a sight test on first obtaining their licences, but there is no periodic re-examination, and there is no test for night-vision or night-blindness imposed. It is notorious also that anyone may obtain a licence to drive a private or trade motor-car, not only without passing a night-blindness test but without passing a vision test of any kind. On further ascending the velocity scale we come to the driver of a night express. In his case, again, so far as I am aware, there is no test for night-blindness. Finally we come to the trained aviator who has to approach an object at twice the speed of an express train. His work is of so exacting a nature that it requires a high standard of physical fitness. Especially so with respect to his vision. If he flies at night, a test for nyctopsis becomes not merely desirable but essential.

The primary object of the following experiments was to gather data towards the establishment of an administrative test for nyctopsis for night pilots in the Royal Air Force. It was found that whereas one pilot may experience difficulties in flying by night, another may have a brilliant record for night-flying, although both may be equally endowed as regards their visual acuity in daylight, as shown by the results of the standard eye tests which they have passed.

It would, therefore, be very desirable if a test were devised which would detect not only those persons who suffered from night-blindness, but also those whose nyctoptic capacities fell below certain standards. Before such a test can be established it is evident that a considerable amount of research must be undertaken in order to fix on the standards to be demanded. Quite apart, however, from the military requirements which gave rise to the research, the above considerations show that nyctopsis is a field of study of wide application in view of the increase both in speed and in volume of land, sea and air traffic. It may well be that a study of the nyctopsis of miners will prove of theoretical and practical value.

Twelve observers took part in the experiments[1]. All were officers in the Royal Air Force with experience as pilots overseas. To them and to those of my friends who acted as observers in the preliminary experiments, I am much indebted for their patient assistance. I wish also to express my thanks to my Commanding Officer, Wing Commander Martin Flack, C.B.E., Royal Air Medical Service, for his assistance in many ways and for his interest in the progress of the work.

Before describing the apparatus I may be allowed to discuss briefly what is generally known as night-blindness. In its widest sense the term denotes that condition in which one sees well by day, but by night or under feeble illumination sees poorly or not at all. We may distinguish between three main forms of night-blindness.

1. *Night-blindness due to diseases producing opacities in the media.* Opacities in the media excite the symptoms of night-blindness when they occupy the periphery and leave the centre free, as in the case of peripheral opacities of the cornea and lens. Under brilliant illumination, when the pupil is contracted, these opacities no longer fall within the area of the pupil, while when the illumination is diminished and the pupil is dilated, they fall within the area of the pupil and interfere with sight. Again, when there are faint diffuse opacities distributed uniformly over the entire cornea, the sight is often better when the pupil is contracted because the dazzling due to diffused light is then less. In all these cases, therefore, the width of the pupil is what determines

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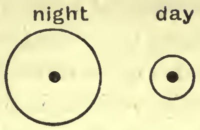
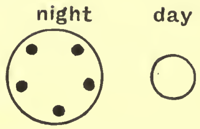
[1] The experiments here recorded were carried out at the Psychological Laboratory of University College, London, under the direction of Professor Spearman, to whom my warmest thanks are due, both for devising the necessary apparatus and for his invaluable advice and guidance throughout the course of the work. I wish also to express my indebtedness to my colleague Mr Flügel who had already been engaged on the same problem and who very kindly gave me the benefit of his experience and allowed me the perusal of his protocols.

the visual power. The following diagram will, perhaps, make this clearer.



Central opacity with day-blindness.



Peripheral opacities with night-blindness.

NOTE. The black dots represent opacities, and the circles the pupillary area.

2. On the other hand, *night-blindness due to disease of the light-perceiving apparatus* has nothing to do with the width of the pupil. The diseases to be considered in this connexion are idiopathic night-blindness on the one hand, and on the other hand retinitis pigmentosa, together with other causes of atrophy of the retina, *e.g.* such as occurs after retinitis, choroiditis, glaucoma. The nature of the visual disturbance, however, in these categories is essentially different. In idiopathic night-blindness there is a reduction of the light sense, probably as a result of insufficient regeneration of the visual substances: there is a disturbance of nutrition of the retina. Now the fovea centralis has a feebler light sense than the periphery of the retina, and hence in idiopathic night-blindness stops functioning simultaneously with the periphery or even somewhat earlier if the illumination is reduced. Many patients state that when the illumination is feeble they see a dark spot in the centre of the visual field. In retinitis pigmentosa the reverse obtains. Here the periphery of the retina is diseased and, when the illumination is reduced, it no longer functions, while central vision is still intact. Then the visual field becomes so small that it no longer suffices for the patient's orientation[1]. Under this head we may conveniently consider hereditary or congenital night-blindness which appears to be due to a defective power of the rods to respond to the stimulus of a feeble light.

3. *Traumatic night-blindness.* The concussion following modern explosives, for example, leads to a great variety of fundus lesions. The injuries are caused by the violence communicated through the bones of the skull or by air vibration.

II. APPARATUS FOR TESTING NYCTOPSIS.

When the research was started we had the following aim in view—to devise an exact apparatus for the purpose of investigation in general, and for ascertaining the minimum requirements of any administrative test. The following problems amongst others immediately presented themselves—the relation of daylight visual acuity to the acuity of the dark-adapted eye: a comparison of central and peripheral vision for the dark-adapted eye: the accurate determination of an individual's sensitivity to light in terms of the intensity of the stimulus for various degrees of adaptation. In addition it was hoped to study various individual differences in some or all of these problems. The apparatus eventually employed we shall now proceed to describe.

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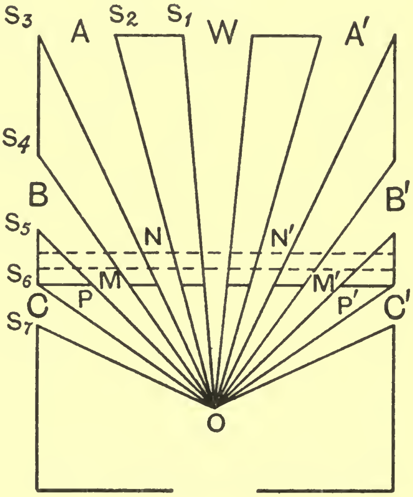
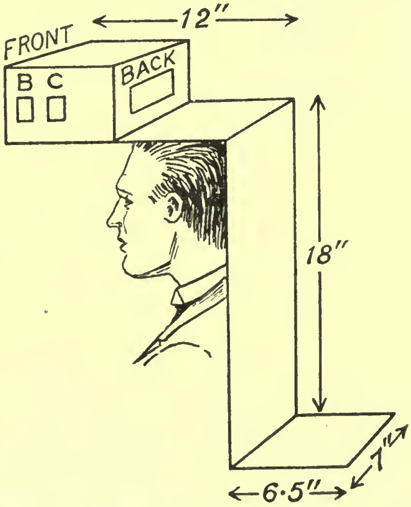
[1] Fuchs's *Textbook of Ophthalmology*, transl. by Duane, 1919, p. 635.

(1) *The Nyctoptometer.* This is a rectangular box made of 3-ply wood measuring 6 inches long by $4\frac{5}{8}$ inches broad by $3\frac{3}{16}$ inches high. Inside is a Siemens 20 candle-power, gas filled lamp (11·3 volts, 33 mm. bulb).



It is absolutely necessary in these experiments to have *standard lamps* which give constant illumination as long as the current passing through is constant. Theoretically any lamp which gives the required luminosities would answer the purpose as long as a graph connecting its luminosity with the current passing through was provided. But in practice it is a great advantage to have lamps so accurately standardised that each gives the same illumination for any given current. Through the courtesy of Messrs Siemens we were allowed to choose lamps which gave very nearly the same candle-powers for a given current. This was done in our presence at the Research Laboratory of Messrs Siemens, and I am indebted to Mr Le Maréchal for his kindness in superintending the test. I may add that the lamps were already standardised to a certain degree of accuracy, though not to that extreme degree which was desirable for our purpose; hence the necessity for the test just mentioned. One lamp has been kept as a standard at the Psychological Laboratory of University College. The lamp used in the following experiments was compared with this standard and found to be in the same condition after the experiments as it was at the beginning.

In the diagram (p. 302) which is a horizontal section through the middle of the box *W*, *A*, *A′*, *B*, *B′*, *C*, *C′* are windows each subtending an angle of 10° at the point *O* where the tungsten filament of the lamp cuts the horizontal section. As the filament can be regarded as a short vertical line, the illumination should be the same at all points on the section which are equidistant from the lamp (except for points in the shadow of the lamp's holder). There is also a window at the back of the box to enable the experimenter to read or write. Each window can be noiselessly opened and shut by means of a metal shutter. The space between each of the seven windows and the next also subtends an angle of 10° at *O*, so that the lines OS_1, OS_2, OS_3, OS_4, OS_5, OS_6 and OS_7 are inclined at 5°, 15°, 25°, 35°, 45°, 55° and 65° respectively to the central line; and similarly in the corresponding lines on the other side. Along *PP′* there is an aluminium diaphragm to prevent any rays of light falling on the inside of the box and being reflected through any of the windows on the screen. Along *MM′* and *NN′* are slits to admit a ground glass plate or a coloured glass plate if required for any purpose. The inside of the box is blackened. As the windows *B*, *B′*, *C* and *C′* are nearer to the light than the windows *A*, *W* and *A′*, their dimensions are made smaller in proportion so as to ensure that the illuminated rectangular area seen by the observer on the screen (mentioned below) should always be of the same size for each window.



(2) *The Nyctoptometer stand.* The box is placed on a wooden stand 18 inches high which is screwed to a table about 33 inches high.

The observer sits on a chair with the back of his head resting against the stand. The box is placed so that the cornea is directly below the light, and thus any illuminated area on the screen subtends approximately the same angle at its source as it does at the observer's eyes.

(3) *The screen.* A wire was bent to an arc of a circle whose radius was one meter and a white casement cloth was hung from it by means of rings. As the light reflected from the screen varies with the nature of the screen, it is necessary to have a standard screen as well as standard lamps in case the experiments are repeated by others. A sample of the cloth used has been kept at the Psychological Laboratory of University College.

I am indebted to Mr W. J. Jones of Messrs Siemens's Research Laboratory, Upper Thames Street, E.C. 4, for his kindness in determining the reflecting power of the screen by means of a 'lumometer.' It was found that 53 per cent. of the light incident on the screen is reflected by it[1]. We are thus in a position to measure the intensity of the threshold lights in our tests. The lamp is 1 meter distant from the screen. Hence if we multiply the candle-power of the lamp corresponding to the threshold by 0·53 we get the intensity of the minimal lights detected by the observers. The result will be in meter-candles.

The screen is attached to a wooden rod fixed on the wall and is hung at such a height that the illuminated areas fall along the median horizontal section. The screen is big enough to receive the light from the extreme windows C and C'. On the screen, opposite (but slightly below) the central window W, is a circular spot of weak red light to serve as a fixation point. A suitable light is obtained by placing a small electric lamp in a box and making a circular aperture one centimeter in diameter *above the level of the lamp* (so that the filament is invisible) and covering it with a sufficient number of red gelatine papers to make the light just visible in the dark when the box is placed immediately behind the white casement cloth. Red light is used to prevent the observer seeing the fixation point indirectly, which might be the case with lights of other colours. But for this reason a spot of luminous paint on the screen would have served as a fixation point.

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[1] Additional corroboration of this result is that Mr Le Maréchal had kindly determined the transmitting power of the screen independently by a different method at his own laboratory and found it to be 0·23. This agrees very closely with the value 0·24 obtained by Mr Jones with the lumometer.

III. METHOD OF PROCEDURE.

(1) *Visual acuity test in daylight.* A sheet of Snellen's Test Types was fixed on the wall of a room open to daylight at a suitable height for each observer. There are nine rows of black type on the sheet. The top row consisting of one capital letter can be read by an individual with satisfactory eyesight at a distance of 60 meters. We may call it the 60 meter line. The 2nd row of two letters is the 30 meter line, the 3rd of three letters is the 21 meter line, the 4th of four letters is the 15 meter line, the 5th of five letters is the 12 meter line, the 6th of six letters is the 9 meter line, the 7th of eight letters is the 6 meter line, the 8th of twelve letters is the 4·5 meter line, the 9th (or bottom row) of twelve letters is the 3 meter line.

The sheet was further illuminated by a Mazda electric lamp (B.T.H. Patent, 200 volts), placed one foot away and slightly below the card. The observer stands 6 meters away and each eye is tested. One eye is covered while the other is tested. In order to carry out this test in a scientific manner it is important to keep the conditions constant, for the estimate of acuity varies considerably according to the illumination of the test types. Yet no official regulations of any government department state the illumination of the standard, and in actual practice the illumination may vary from bright diffuse daylight to dull artificial light! Neither is there uniformity of procedure. A common method is to stop at the line where the person tested makes two mistakes. Such a procedure, however, is too greatly influenced by mere chance. In our tests credit was given for every letter read correctly no matter what its position is on the card. For the purpose of evaluating the results, Spearman's method[1] of right and wrong cases for unequal steps was adopted. By such a procedure each observer gets an exact mark representing his ability. The calculation is quickly made and the advantages are obvious since the observers can be arranged in order of merit, and it is also possible to calculate what correlation exists between an observer's visual acuity in daylight and any other function for which exact data are available.

(2) *Standard Adaptation.* To ensure that all observers are similarly adapted at the start, a high candle-power lamp is placed for 5 minutes on the stand above the observer's head. A Siemens 1000 candle-power XL lamp (220 volts, gas filled) was found suitable. During this time the observer looked on the white screen, but in order to prevent fatigue he

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[1] This *Journal*, II. January, 1908.

does not fixate a particular point. By placing the lamp in the above position there is no shadow on the screen and all parts of the retina are similarly adapted.

(3) *Main Experiment.* The 1000 candle-power lamp is extinguished and the observer is in darkness. After an interval of 2 minutes (taken to avoid irregular judgments):

(*a*) The central window *W* is opened. The resistance (see below) is decreased by tenths of an ohm until the light on the screen is visible to the observer. The observer is told beforehand that the light will appear immediately above the luminous fixation point. The window is shut after the observer says that he sees the light. The metronome is now started, beating seconds. The resistance is increased by a tenth of an ohm at a time and after each increase the observer has to judge. He is told to concentrate his attention on the screen and to fixate the luminous point after receiving the signal 'Now,' and to say when the light appears. He is told that the light will appear at any time between 1 and 11 seconds after the signal and that it will only appear for a short time (about 1 second). By this procedure the possibility of giving correct answers by mere guesswork is practically eliminated. The observer is allowed 1 second for his answer and is told to relax his attention after each judgment and to shut his eyes. When the observer has failed three times (not necessarily successive) to perceive the light, it is assumed that the threshold has been passed, *e.g.* if he fails at 3·7 ohms, 3·8 ohms and 3·9 ohms, it is assumed that the resistance corresponding to the threshold is 3·6 ohms.

(*b*) The windows *A* and *A′* are opened and the resistance decreased by tenths of an ohm until the two lights are visible to the observer who fixates the luminous fixation point. Then the same procedure as above is adopted, but, in this case only one window (either *A* or *A′*) is opened and the observer answers 'Left' or 'Right' as the case may be as soon as he sees the light. The light appears as before at any time between 1 and 11 seconds after the signal. By this procedure the observer cannot gain anything by not conscientiously fixating the luminous point.

(*c*) The same procedure is repeated with windows *B* and *B′*.

(*d*) The same procedure is repeated with windows *C* and *C′*.

(*e*) *Acuity tests for the dark-adapted eye.* For this purpose two tests were employed.

Test 1. A card was prepared containing six letters, arranged in two rows as in the accompanying sketch. The size of the type was that found in the '9 meter line' on Snellen's card. The card is placed on the screen and is illuminated by opening the central window *W* (the observer being one meter distant from the screen as in all the dark-room experiments). The initial illumination is too low for the observer to be able to read any of the letters, but the resistance is gradually reduced by a tenth of an ohm at a time, the experimenter saying 'Now' after each change, until the observer can read four letters correctly. The corresponding resistance is noted.

C T G

O E L

Test 2. A similar procedure is adopted with the test type which is found in the '3 meter line' on the Snellen card.

The whole procedure, that is (*a*), (*b*), (*c*), (*d*) and (*e*) completes the first experiment, and this is gone through three times again without a break, the observer's eyes becoming more and more dark-adapted. The time during which the observer has been kept in darkness, *i.e.* the adaptation time, is given for each reading in Table A and Table B (pp. 317, 318). The correct time for starting each experiment was noted by means of a radiolite watch. Four different cards were prepared for (*e*) *Test* 1 and similarly for (*e*) *Test* 2, namely, one for each experiment. The cards were so arranged that no letter appeared twice in the same position and so that no letter followed the same letter twice. Thus the observer was far less likely to give a correct answer by guesswork[1].

Becker's Three Dial Resistance Box was used to measure the resistance. Opposite the zero on each dial we placed a small circular piece of luminous paper. Similar pieces could also be fixed on the end of each of the three arms if necessary. The dials are hundredths, tenths and units, so that the total resistance of the box is 9·99 ohms. When each observer entered the dark room the 'compensating resistance' is noted (about 0·6 ohm). By this is meant the resistance required in the circuit in order to reduce the current passing through the lamp to 0·7 ampere, which was our starting point for all observers. This 'compensating resistance' is required owing to the fact that no accumulator can be found whose voltage remains perfectly constant. The 'compensating resistance' is also noted after the testing of each observer is concluded. This was found generally not to vary from its initial value. If it varied slightly, then the mean of the two readings was taken as the zero for that observer. The resistance values actually obtained in the experiments were afterwards corrected by subtracting from them the value of this 'compensating resistance.' Then the resistance values were translated into current values (amperes) and eventually to illumination values (candle-powers) by obtaining a graph relating current to illumination by means of a photometer. For this purpose one lamp (No. 3) is kept as a standard and all other lamps are compared with it.

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[1] Letters were used in the tests for the dark-adapted eye as well as in the daylight test for visual acuity in preference to tests that are often recommended, namely Snellen's illiterate E-test, Landolt's C-test and the counting of dots. With the illiterate E-test as well as with Landolt's test there would be some difficulty in the dark owing to the observer making mistakes in his answers even when he has correctly perceived the stimulus, and it is notorious that some observers are very inaccurate in counting. Besides, there would be a probability of answering correctly by chance. This could be largely eliminated, it is true, by having a dozen possible positions for the E and the C, but in that case it would be a difficult matter for an untrained observer to judge the exact position.

Griffin's Moving Coil Ammeter was used to measure the current. It is dead-beat in action and has a range from 0 to 1 ampere, which is divided into hundredths on white celluloid. As the amperage range required for the experiments varied between 0·50 and 0·79 amperes, it is seen that an ammeter with a fine gradation is required, for the candle-power of the lights increases rapidly with increase of current, thus,

amperes	candle-power
0·788	0·294000
0·500	0·000007

For the curve connecting candle-power and current is a logarithmic one of the form

$$x = Pa^{Q},$$

where x denotes candle-power, a denotes amperes, P and Q being constants.

The current was obtained from an accumulator—Type No. 2583 (4 volts) of the Rotax Motor Accessories Co., Ltd.

IV. Results.

(*a*) *Results of other investigators.*

Aubert found the light sensitivity after two hours' dark-adaptation to be only thirty-five times that at its beginning. His method was to use a platinum wire whose glow was just visible to the observer before dark-adaptation, then by altering the length of the wire the threshold value for any adaptation-time could be calculated. According to Nagel[1], however, the method had certain defects, one being that the state of light-adaptation, from which the observers started, could not be determined.

Nagel[1] himself, by means of his adaptometer, found the increase in sensitivity to be enormously greater than that found by Aubert, as is seen in the following table:

Adaptation (mins.)	0·5	4	9	14	19	23	26	31	39	51	61
Sensitivity	20	75	1850	10400	26300	69500	94700	174000	195000	208000	215000

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[1] von Helmholtz, *Physiologische Optik*, 1911, II.

In order to get his observers uniformly adapted, Nagel kept them out of doors in bright sunshine from 20 to 30 minutes. They were directed to look on bright objects, their brightness however not being sufficient to produce dazzling. After this preparation he states that a white surface subtending an angle of 10° at the observer's eye is just visible before dark-adaptation when illuminated by a 'normal candle' placed one meter away. Thus the sensitivity of Nagel's observers at the start is 1.

For at least three reasons the results obtained in the present paper cannot be compared directly with those of Nagel—

(1) Nagel gives no particulars concerning his observers, *e.g.* number, occupation, etc.

(2) The methods of insuring standard adaptation at the beginning were not the same.

(3) Nagel's threshold values were monocular, in the present paper they were binocular in order to conform to an aviator's actual practice, and it has been ascertained by Piper that in dark-adapted eyes binocular summation of stimuli occurs, so that the sensibility is about twice as great with both eyes as with only one, though individual variations occur[1].

It should be noted, however, that in the S tests where the vision is 60° peripheral the amount of binocular summation would be very small. This may account for the higher values obtained for the thresholds in the S tests.

After dark-adaptation of 4 minutes the average sensitivity of our observers is much greater than that found by Nagel, but after 40 minutes the reverse obtains. It should however be added that one observer L has a threshold after 40 minutes' adaptation which corresponds with that obtained by Nagel.

Further, our average threshold for Test P is higher than it otherwise would be, owing to the abnormal figures obtained for observer B.

(*b*) *Own Results.*

Notation. For the sake of brevity the letters P, Q, R and S refer to the tests for light sensitivity with the following windows respectively—central, 20° peripheral, 40° peripheral and 60° peripheral; the letters X and Y refer to the tests for the acuity of the dark-adapted eye with the big and small type respectively, while θ refers to the test for ordinary visual acuity in daylight. The number after each letter refers to the time in minutes for which the observer has been dark-adapted. At the end of this paper will be found tables giving the numerical results obtained. Table A gives the threshold values measured in candle-meters for each observer for tests P, Q, R and S. Table B gives the same for tests X and Y (see p. 317 *et seq.*).

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[1] Cf. J. Herbert Parsons, *Colour Vision.* Cambridge Univ. Press, 1915.

Table C gives the threshold values for each observer for test θ, as calculated by Spearman's method of right and wrong cases[1]. In this table the smaller the number, the greater the acuity; for the number refers to the size of the type which can be read at a distance of six meters.

Table D gives the correlation coefficients calculated from Tables A, B and C by the product-moments formula. Thus, for example, it is seen in the table that the correlation between $P\,4$ and $P\,40$, or $\frac{P\,4}{P\,40}$ as we may call it, is 0·25. Before considering the deductions which can be drawn from these coefficients, we may point out certain features which are already apparent in Tables A, B and C.

(1) *Individual Differences.* We are at once struck by the magnitude of certain individual differences. This is the more remarkable as all our observers belonged to a special class, namely flying officers who did not differ much in age from one another and who had all passed the standard tests for vision of the Royal Air Force. Thus confining our attention to the test for central vision, it is seen that observer E, after an adaptation of 4 minutes is only sensitive to a light of 0·00127 candle-meter, while C can see a light of 0·00009 candle-meter. C is therefore fourteen times as sensitive as E.

Again, after an adaptation of 40 minutes the values for B and L are 0·000153 candle-meter and 0·000006 candle-meter respectively. Thus L is twenty-five times as sensitive as B. Yet if we examine the figures obtained for these observers in the daylight visual test no striking differences are seen.

Again, if we calculate the mean threshold for light sensitivity for our observers for central vision after 4 minutes and after 40 minutes adaptation, namely 0·00041 candle-meter and 0·000031 candle-meter, we see that the former is over thirteen times as great as the latter. This ratio, *i.e.* the sensitivity to light after 40 minutes' compared with that after 4 minutes' adaptation, varies from a maximum value of fifty-six for one observer to a minimum of four for another. A larger number of observers would be required before standard values could be obtained for these thresholds for various periods of adaptation. But our data tend to show that if a large number of persons were tested, after an adaptation of 40 minutes there would be some individuals belonging to each of the following classes:

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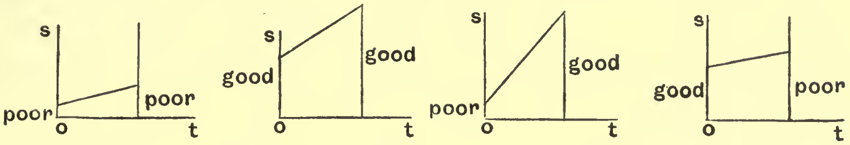
[1] This *Journal*, II. January 1908.

(1) Poor 'adapters' both at the beginning and end.
(2) Good 'adapters' both at the beginning and end.
(3) Poor 'adapters' at the beginning and good at the end.
(4) Good 'adapters' at the beginning and poor at the end.



(1) (2) (3) (4)

os—sensitivity. ot—time.

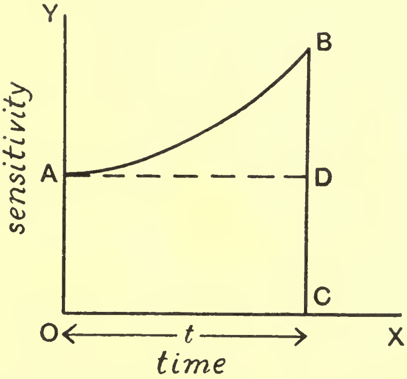
Further research would give the relative sizes and distribution of these classes. It is easily seen that a person's light sensitivity after an adaptation of *t* minutes can be considered to be made up of

(1) his sensitivity at the beginning of adaptation,
(2) the increase in sensitivity that has taken place during *t* minutes.

Thus in the diagram

$$CB = OA + DB.$$

So that if we find an individual's sensitivity after an adaptation of *t* minutes to have the value *CB*, this tells us nothing about its initial value, neither does it tell us what increase has taken place during the time *t*. The fullest information is given by a complete adaptation curve, but a study of the correlation coefficients in Table D will tell us whether initial ability correlates with final ability or not. It is only by such a method after testing a sufficient number of individuals that it will be possible to judge the relative importance of the threshold values at the beginning and at the end of an adaptation period.



Nagel uses the term 'Adaptionsbreite' for the ratio of the sensitivity of the eye after one hour's dark-adaptation to that of the light-adapted eye. The latter as we have seen he takes to be unity. It will be seen later that probably a more useful term for practical purposes would be the absolute value of the sensitivity after dark-adaptation of some 16 minutes.

Turning now to study the correlation coefficients given in Table D the following are the conclusions to be drawn:

(2) *Central and peripheral vision.*

(*a*) A study of the light sensitivity of our observers, as measured by each of the tests P, Q, R and S, shows that the results obtained by any two of these tests correlate highly with one another. It is true that, owing to the procedure adopted, the times of adaptation are not exactly equal in the different tests, but as regards the following coefficients the differences in time are not big enough to alter the significance of the result, thus:

$$\frac{P\,16}{Q\,18} = \cdot 86;\ \frac{P\,16}{R\,20} = \cdot 72;\ \frac{P\,16}{S\,22} = \cdot 86;\ \frac{Q\,18}{R\,20} = \cdot 75;\ \frac{Q\,18}{S\,22} = \cdot 90;\ \frac{R\,20}{S\,22} = \cdot 82.$$

$$\frac{P\,28}{Q\,30} = \cdot 85;\ \frac{P\,28}{R\,32} = \cdot 95;\ \frac{P\,28}{S\,34} = \cdot 81;\ \frac{Q\,30}{R\,32} = \cdot 90;\ \frac{Q\,30}{S\,34} = \cdot 81;\ \frac{R\,32}{S\,34} = \cdot 65.$$

$$\frac{P\,40}{Q\,42} = \cdot 85;\ \frac{P\,40}{R\,44} = \cdot 94;\ \frac{P\,40}{S\,46} = 1\cdot 0;\ \frac{Q\,42}{R\,44} = \cdot 96;\ \frac{Q\,42}{S\,46} = 1\cdot 0;\ \frac{R\,44}{S\,46} = 1\cdot 0.$$

We thus arrive at the important result that in testing for nyctopsis it is not necessary to examine both central[1] and peripheral vision as the ranking of the observers with respect to the latter would agree with that obtained with respect to the former. This means a great saving of time. Thus if our tests were repeated, it would be unnecessary with normal observers to use the peripheral windows, so that tests Q, R and S could be left out. It is, however, necessary to add that the results for central and peripheral vision would not be expected to agree for an observer with peripheral opacities of the cornea. Such a condition, however, could be detected by other means.

(*b*) If we inspect the values obtained for the average threshold for light sensibility in Table A, we see that the lowest values are obtained in Test Q that is, for 20° peripheral vision. This result would be expected in view of the researches of Breuer and Pertz[2] who showed that the peripheral rise in sensibility in dark-adaptation is rapid from 1° to 4° around the fovea, then slower to a maximum between 10° and 20° beyond which it falls. Breuer and Pertz, however, do not appear to have explored regions much beyond 20° towards the periphery, but the results obtained in the R tests (*i.e.* 40° peripheral) show that the fall in sensibility up to this point is very small (cf. average thresholds for R test in Table A). It is true that there is a greater fall when we come to the S test (*i.e.* 60° peripheral) but in this case it is probable that part of the fall is due to the thresholds approximating to monocular ones, for it has been ascertained that monocular thresholds for light sensibility are not so low as binocular.

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[1] 'Central' does not mean strictly 'foveal' in our tests, but means vision up to 10° peripherally, *i.e.* the vision obtained by using the central window W.

[2] V. Kries, *Ztsch. f. Psychol. u. Physiol. d. Sinnesorg.* 1897, xv. 327; see also J. Herbert Parsons, *Colour Vision.* Cambridge Univ. Press, 1915.

(3) *Critical time of adaptation.*

How long must an individual be kept in the dark when tested nyctoptically? Is it necessary that he should remain in the dark for an hour or will a much shorter period answer the purpose?

Duane[1] states that a period of adaptation of 10 minutes which the patient must pass with bandaged eyes in a perfectly dark room is sufficient for practical purposes for testing the light sense. Let us test this question by reference to our intercorrelations. For an adaptation of 4 minutes, we have

$$\frac{P\,4}{P\,16} = \cdot 30; \quad \frac{P\,4}{P\,28} = \cdot 23; \quad \frac{P\,4}{P\,40} = \cdot 25.$$

Thus the coefficients are all low, which shows that the results after 4 minutes do not correlate highly with those obtained with the longer periods. In other words 4 minutes is too short a time for our purpose. Similarly 6, 8 or 10 minutes fail to give us higher coefficients, thus:

$$\frac{Q\,6}{Q\,18} = \cdot 35; \quad \frac{Q\,6}{Q\,30} = -\cdot 20; \quad \frac{Q\,6}{Q\,42} = -\cdot 22.$$

$$\frac{R\,8}{R\,20} = \cdot 08; \quad \frac{R\,8}{R\,32} = -\cdot 17; \quad \frac{R\,8}{R\,44} = -\cdot 16.$$

$$\frac{S\,10}{S\,22} = -\cdot 08; \quad \frac{S\,10}{S\,34} = -\cdot 13; \quad \frac{S\,10}{S\,46} = -\cdot 24.$$

The P, Q, R and S tests yield no figures for 12 or 14 minutes. For these times we have the X and Y tests only. Here the coefficients are distinctly higher namely:

$$\frac{X\,12}{X\,24} = 1\cdot 00; \quad \frac{X\,12}{X\,36} = \cdot 58; \quad \frac{X\,12}{X\,48} = \cdot 59.$$

$$\frac{Y\,14}{Y\,26} = \cdot 80; \quad \frac{Y\,14}{Y\,38} = \cdot 73; \quad \frac{Y\,14}{Y\,50} = \cdot 67.$$

For an adaptation of 16 minutes or longer, we do, however, get a still higher average of coefficients, thus:

$$\frac{P\,16}{P\,28} = \cdot 94; \quad \frac{P\,16}{P\,40} = \cdot 86$$

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[1] **Fuchs's *Textbook of Ophthalmology*, transl. by Duane, 1919, 127.**

(the only exceptions being $\frac{R\,20}{R\,32} = \cdot 68$ and $\frac{R\,20}{R\,44} = \cdot 60$ which are not so high). We may therefore conclude that an adaptation of 16 minutes is sufficient for practical purposes.

This result agrees very well with Mr Flügel's estimate[1] of 20 minutes, especially when we bear in mind his statement that the anomalous behaviour of four of his observers has lowered some of the correlations.

(4) *Visual acuity in daylight* (θ) *does not correlate significantly with light sensitivity at any adaptation time.*

This is shown by the following coefficients:

$$\frac{P\,4}{\theta} = -0\cdot 10; \quad \frac{P\,16}{\theta} = -0\cdot 16; \quad \frac{P\,28}{\theta} = -0\cdot 10; \quad \frac{P\,40}{\theta} = -0\cdot 14.$$

$$\frac{Q\,6}{\theta} = -0\cdot 22; \quad \frac{Q\,18}{\theta} = -0\cdot 26; \quad \frac{Q\,30}{\theta} = -0\cdot 33; \quad \frac{Q\,42}{\theta} = -0\cdot 23.$$

$$\frac{R\,8}{\theta} = -0\cdot 04; \quad \frac{R\,20}{\theta} = -0\cdot 08; \quad \frac{R\,32}{\theta} = -0\cdot 22; \quad \frac{R\,44}{\theta} = -0\cdot 12.$$

$$\frac{S\,10}{\theta} = 0\cdot 02; \quad \frac{S\,22}{\theta} = -0\cdot 12; \quad \frac{S\,34}{\theta} = -0\cdot 26; \quad \frac{S\,46}{\theta} = -0\cdot 35.$$

In view of the fact that the above correlations all, except one, are negative, one might incline to the view that there is a slight negative correlation between the two functions. Such a result would have some theoretical bearing on the duplicity theory. But the deduction would be unwarranted without further research, as the coefficients are not big enough to be significant, even the highest, $-0\cdot 35$, being only about twice the size of its probable error ($0\cdot 17$). The deduction warranted by the figures is therefore as stated, and this agrees with Mr Flügel's result obtained by a different method.

(5) *Visual acuity in daylight* (θ) *correlates moderately with the visual acuity in a dim light.*

This is shown by the following coefficients, all of which, except the first, exceed three times the size of their probable error.

$$\frac{X\,12}{\theta} = \cdot 40; \quad \frac{X\,24}{\theta} = \cdot 57; \quad \frac{X\,36}{\theta} = \cdot 69; \quad \frac{X\,48}{\theta} = \cdot 72.$$

$$\frac{Y\,14}{\theta} = \cdot 49; \quad \frac{Y\,26}{\theta} = \cdot 71; \quad \frac{Y\,38}{\theta} = \cdot 78; \quad \frac{Y\,50}{\theta} = \cdot 77.$$

This result agrees with that obtained by Mr Flügel, although there is some divergence in the magnitudes of the coefficients.

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[1] This *Journal*, 1921, XI. pp. 289–298.

(6) *Light sensitivity does not correlate significantly with the visual acuity in dim light.*

This is best seen by inspecting the intercorrelations between the P's and the X's and also between the P's and the Y's in the top right-hand corner of Table D. Mr Flügel[1], however, finds a slight positive correlation between the two functions. From the practical point of view, nevertheless, both results show that no inference can be drawn from an individual's performance in X or Y as to his ability in P or *vice versa*. It might be pointed out that Mr Flügel's X and Y tests are not directly comparable with those of the present writer. In the former the figures were varied and the light was kept constant, in the latter the figures (letters) were kept constant and the light was variable. Moreover, the intensities of the lights differed in the two procedures. Mr Flügel's intensities were roughly 0·1 and 0·2 candle-power in tests X and Y respectively. In those of the present writer the lights were much dimmer. The mean thresholds in the X and Y tests after 48 minutes' and 50 minutes' adaptation were 0·0037 candle-power and 0·043 candle-power respectively. One would therefore expect, *a priori*, that the correlation between P and X in our experiments would be higher than in Mr Flügel's, for one would assume that P would be the more involved in X, the darker X was. The reverse, however, was found to be the case. This paradoxical result is in harmony with a similarly paradoxical result in Mr Flügel's experiments, as the correlation between P and Y was found greater than that between P and X. Moreover, in the experiments of the present writer the correlation between P and X is slightly less than that between P and Y, though not significantly so. I am indebted to Mr Flügel for pointing out these paradoxes. It is evident that the matter requires further investigation.

(7) *Visual acuity in the dim light* (X) *correlates rather highly with that in the much dimmer light* (Y).

This is seen from an inspection of the intercorrelations of the X's with the Y's. It will be noticed that these are higher than those obtained by Mr Flügel. One would expect this, *a priori*, in view of the fact mentioned in the previous paragraph, namely, that the lights in the present tests were much dimmer than in Mr Flügel's tests. Consequently we may surmise that the mechanisms of daylight vision will thus only have a small effect in the case of both X and Y, and that the intercorrelations between X's and Y's will therefore be rather high.

Finally the writer would lay stress on the general regularity of the coefficients which form the bases for each of the above conclusions. This regularity entitles the results to serious consideration in spite of their being obtained from such few numbers.

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[1] *Loc. cit.*

V. SUMMARY OF RESULTS.

1. Individuals differ much in light sensitivity. This holds for adaptation periods ranging from 4 minutes (where one observer was fourteen times as sensitive as another) to 40 minutes (where one was twenty-five times as sensitive as another). After an adaptation of 16 minutes the keenest observer in Test P could detect a light of 0·000009 candle-meter. His sensitivity therefore is $\frac{1}{0\cdot000009}$ or 111,111 units. On the other hand the sensitivity of the least sensitive observer was only 6536 units.

2. The testing for light sensitivity can be confined to a region of 10° round the fovea, as the results for 20°, 40° or 60° to the periphery give approximately the same ranking.

3. Light sensitivity attains its maximum for vision in the neighbourhood of 20° to the temporal side of the fovea.

4. Dark-adaptation for 16 to 20 minutes is sufficient for practical purposes for testing both light sensitivity and the visual acuity of the dark-adapted eye.

5. An individual's visual acuity in daylight is no criterion of his light sensitivity.

6. Visual acuity in daylight correlates moderately with the visual acuity in the dim lights used in tests X and Y.

7. No inference can be drawn from testing an individual's light sensitivity as to his visual acuity in dim light or *vice versa*.

8. Visual acuity in the dim light (X) correlates rather highly with that in the much dimmer light (Y).

VI. TABLES A—D.

Table A.

	Central vision *P* Thresholds in meter-candles				20° peripheral vision *Q* Thresholds in meter-candles			
Observer	After 4 mins.	After 16 mins.	After 28 mins.	After 40 mins.	After 6 mins.	After 18 mins.	After 30 mins.	After 42 mins.
A	·000153	·000048	·000026	·000021	·000064	·000026	·000021	·000023
B	·000636	·000153	·000153	·000153	·000153	·000090	·000064	·000064
C	·000090	·000021	·000025	·000021	·000053	·000017	·000017	·000017
D	·000477	·000025	·000023	·000023	·000200	·000023	·000021	·000021
E	·001270	·000053	·000025	·000023	·000425	·000048	·000023	·000021
F	·000477	·000026	·000023	·000023	·000262	·000064	·000023	·000025
G	·000200	·000053	·000026	·000026	·000064	·000053	·000023	·000048
H	·000262	·000042	·000024	·000021	·000010	·000042	·000042	·000025
J	·000334	·000064	·000026	·000023	·000477	·000064	·000023	·000017
K	·000425	·000026	·000012	·000021	·000200	·000026	·000021	·000021
L	·000318	·000009	·000006	·000006	·000064	·000006	·000006	·000006
M	·000318	·000017	·000017	·000012	·000334	·000017	·000006	·000006
Mean	·000410	·000045	·000032	·000031	·000200	·000040	·000024	·000025

	40° peripheral vision *R* Thresholds in meter-candles				60° peripheral vision *S* Thresholds in meter-candles			
Observer	After 8 mins.	After 20 mins.	After 32 mins.	After 44 mins.	After 10 mins.	After 22 mins.	After 34 mins.	After 46 mins.
A	·000025	·000048	·000025	·000023	·000048	·000048	·000023	·000023
B	·000010	·000053	·000090	·000090	·000153	·000090	·000090	·000090
C	·000023	·000021	·000021	·000021	·000090	·000021	·000025	·000025
D	·000262	·000025	·000025	·000023	·000318	·000048	·000048	·000048
E	·000262	·000053	·000026	·000023	·000334	·000064	·000048	·000025
F	·000064	·000026	·000025	·000025	·000064	·000053	·000053	·000053
G	·000010	·000048	·000048	·000048	·000090	·000053	·000064	·000064
H	·000090	·000050	·000050	·000026	·000090	·000050	·000042	·000025
J	·000318	·000048	·000023	·000017	·000318	·000053	·000026	·000017
K	·000153	·000026	·000021	·000017	·000200	·000025	·000023	·000021
L	·000021	·000004	·000004	·000003	·000026	·000004	·000006	·000006
M	·000425	·000021	·000017	·000017	·000795	·000023	·000021	·000017
Mean	·000154	·000035	·000032	·000028	·000211	·000045	·000040	·000035

NOTE. As the reflecting power of the screen is ·53, the above values when divided by ·53 will give the corresponding candle-power of the lamp.

Table B.

	Test *X* Thresholds in meter-candles				Test *Y* Thresholds in meter-candles			
Observer	After 12 mins.	After 24 mins.	After 36 mins.	After 48 mins.	After 14 mins.	After 26 mins.	After 38 mins.	After 50 mins.
A	·0151	·0095	·0106	·0106	·156	·156	·156	·156
B	·0035	·0033	·0033	·0022	·087	·051	·029	·025
C	·0023	·0020	·0028	·0023	·025	·029	·025	·025
D	·0035	·0033	·0035	·0035	·061	·051	·061	·051
E	·0069	·0047	·0042	·0035	·051	·035	·029	·029
F	·0033	·0023	·0028	·0023	·035	·041	·029	·029
G	·0042	·0035	·0035	·0035	·041	·030	·029	·029
H	·0085	·0037	·0033	·0028	·051	·032	·027	·017
J	·0069	·0058	·0042	·0035	·041	·035	·029	·025
K	·0069	·0035	·0033	·0022	·087	·088	·041	·029
L	·0047	·0033	·0042	·0042	·035	·035	·051	·051
M	·0151	·0028	·0028	·0028	·156	·061	·061	·051
Mean	·0067	·0040	·0040	·0037	·069	·054	·048	·043

Note. As the reflecting power of the screen is ·53, the above values when divided by ·53 will give the candle-power of the lamp in each case.

Table C.

Visual acuity in daylight, measured by Snellen's Test Types.

	Thresholds	
Observer	Right eye	Left eye
A	7·50	15·50
B	3·25	4·13
C	3·00	3·12
D	5·60	4·00
E	4·00	4·12
F	3·12	3·50
G	4·12	4·25
H	2·25	2·37
J	3·00	4·37
K	3·00	2·75
L	4·25	4·12
M	4·25	4·25

Note. Values obtained by Spearman's method of right and wrong cases. The numbers refer to the size of the type which can be read at a distance of six meters. Thus with the exception of observer A, all observers have 'supernormal' vision. In this particular case it was found advisable to use the values for the right eye in correlational work.

Table D.

	θ	P 4	P 16	P 28	P 40	Q 6	Q 18	Q 30	Q 42	R 8	R 20	R 32	R 44	S 10	S 22	S 34	S 46	X 12	X 24	X 36	X 48	Y 14	Y 26	Y 38	Y 50
θ	.	−·10	−·16	−·10	−·14	−·22	−·26	−·33	−·23	−·04	−·08	−·22	−·12	·02	−·12	−·26	−·35	·40	·57	·69	·72	·49	·71	·78	·77
P 4	−·10	.	·30	·23	·25	·63	.	.	.	·35	.	.	.	·23	.	.	.	−·22	−·05	−·16	−·22	−·11	−·21	−·16	−·06
P 16	−·16	·30	.	·94	·86	.	·86	.	.	.	·72	.	.	.	·86	.	.	−·14	·16	·05	−·05	·09	·04	−·14	−·12
P 28	−·10	·23	·94	.	1·00	.	.	·85	.	.	.	·95	.	.	.	·81	.	−·23	−·05	−·08	−·16	·12	·01	−·15	−·14
P 40	−·14	·25	·86	1·00	.	.	.	.	·85	.	.	.	·94	.	.	.	1·00	−·27	−·09	−·11	−·20	·10	−·01	−·18	−·18
Q 6	−·22	·63	.	.	.	.	·35	−·20	−·22	·65	.	.	.	·63	.	.	.	·15	.	.	.	·02	.	.	.
Q 18	−·26	.	·86	.	.	·35	.	·81	·82	.	·75	.	.	.	·90	.	.	.	·01	.	.	.	−·12	.	.
Q 30	−·33	.	.	·85	.	−·20	·81	.	1·00	.	.	·90	.	.	.	·81	.	.	.	−·08	.	.	.	−·24	.
Q 42	−·23	.	.	.	·85	.	·82	1·00	.	.	.	.	·96	.	.	.	1·00	.	.	.	−·14	.	.	.	−·19
R 8	−·04	·35	.	.	.	·65	.	.	.	.	·08	−·17	−·16	·93	.	.	.	·35	.	.	.	·34	.	.	.
R 20	−·08	.	·72	.	.	.	·75	.	.	·08	.	·68	·60	.	·82	.	.	.	·49	.	.	.	·15	.	.
R 32	−·22	.	.	·95	.	.	.	·90	.	−·17	·68	.	1·00	.	.	·65	.	.	.	−·13	.	.	.	−·24	.
R 44	−·12	.	.	.	·94	.	.	.	·96	−·16	·60	1·00	.	.	.	.	1·00	.	.	.	−·13	.	.	.	−·08
S 10	·02	·23	.	.	.	·63	.	.	.	·93	.	.	.	.	−·08	−·13	−·24	·48	.	.	.	·39	.	.	.
S 22	−·12	.	·86	.	.	.	·90	.	.	.	·82	.	.	−·08	.	·87	1·00	.	·17	.	.	.	·03	.	.
S 34	−·26	.	.	·81	.	.	.	·81	.	.	.	·65	.	−·13	·87	.	1·00	.	.	−·24	.	.	.	−·33	.
S 46	−·35	.	.	.	1·00	.	.	.	1·00	.	.	.	1·00	−·24	1·00	1·00	.	.	.	.	−·31	.	.	.	−·28
X 12	·40	−·22	−·14	−·23	−·27	·15	.	.	.	·35	.	.	.	·48	.	.	.	.	1·00	·58	·59	·86	·70	·69	·95
X 24	·57	−·05	·16	−·05	−·09	.	·01	.	.	.	·49	.	.	.	·17	.	.	1·00	.	·94	·90	·50	·80	·78	·79
X 36	·69	−·16	·05	−·08	−·11	.	.	−·08	.	.	.	−·13	.	.	.	−·24	.	·58	·94	.	1·00	·52	·86	·90	·93
X 48	·72	−·22	−·05	−·16	−·20	.	.	.	−·14	.	.	.	−·13	.	.	.	−·31	·59	·90	1·00	.	·52	·87	·93	·95
Y 14	·49	−·11	·09	·12	·10	·02	.	.	.	·34	.	.	.	·39	.	.	.	·86	·50	·52	·52	.	·80	·73	·67
Y 26	·71	−·21	·04	·01	−·01	.	−·12	.	.	.	·15	.	.	.	·03	.	.	·70	·80	·86	·87	·80	.	·96	·94
Y 38	·78	−·16	−·14	−·15	−·18	.	.	−·24	.	.	.	−·24	.	.	.	−·33	.	·69	·78	·90	·93	·73	·96	.	1·00
Y 50	·77	−·06	−·12	−·14	−·18	.	.	.	−·19	.	.	.	−·08	.	.	.	−·28	·95	·79	·93	·95	·67	·94	1·00	.

NOTE. All correlations in this table which are greater than 0·34 are greater than twice their probable error.
All correlations in this table which are greater than 0·46 are greater than three times their probable error.

(*Manuscript received* 2 *November* 1920.)

THE FUNCTIONS OF IMAGES[1].

By F. C. BARTLETT.

(*From the Cambridge Psychological Laboratory.*)

- § 1. *Description of the experiments* (pp. 320–322).
- § 2. *The experimental results* (pp. 322–330).
  - I. *The general influence upon sensory imagery of affective factors.*
  - II. *The main characteristics of direct sensory imagery.*
    - (*a*) *Visualisation and confidence.*
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    - (*c*) *Visualisation and an order of sequence.*
    - (*d*) *Changes in visual images.*
  - III. *How the use of words affects direct sensory imagery.*
    - (*a*) *Vocalisation and confidence.*
    - (*b*) *Vocalisation and clearness in reproduction.*
    - (*c*) *Vocalisation and an order of sequence.*
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- § 3. *Theoretical considerations* (pp. 330–337).
    - (*a*) *The most primitive type of sensory imagery.*
    - (*b*) *The image as an aid to a checked reaction.*
    - (*c*) *The image as an alternative reaction.*
    - (*d*) *Why words tend to be substituted for sensory imagery.*

§ 1. DESCRIPTION OF THE EXPERIMENTS.

THIS paper may be regarded as a continuation of an already reported "Experimental Study of some Problems of Perceiving and Imaging[2]." One of the conclusions of that study was that the content of acts of imaging is continually undergoing change, and that the specific nature and conditions of the change call for detailed investigation. In the course of succeeding experiments, the main purpose of which was to determine the general types of transformation of images, certain suggestions as to the functions of images arose, and in the present paper I propose to formulate and discuss these suggestions.

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[1] A paper (here modified) read at a meeting of the British Association (Psychology Sub-section), Cardiff, 26 August, 1920.

[2] See this *Journal*, VIII. 1916, pp. 222–66.

The experiments themselves fall into two main groups. The material used in the first group consisted of a number of objects so much alike that they could be readily related, but at the same time possessing definite individual characteristics. This material was presented for observation under specified conditions. Afterwards it was removed, and the subjects gave a description of what they had perceived, and answered questions relating to the objects presented. The materials used consisted of five picture-postcards, each bearing a representation of a face of a military or naval officer or man. Faces were chosen because they commonly arouse interest, and the faces of army or navy men because the experiments were conducted during the early days of the late war, when interest in members of the Services was more than ordinarily widespread and intense. Moreover the pictures, while they contained comparatively little detail, and were clearly related, had each a strongly marked individual character. The cards were placed face downwards on a table, always in the same order. The instructions were:

> On these picture postcards there are a number of representations of men's faces, one on each card. I want you to look at each card for ten seconds, noting carefully all the characteristics of the face, so that later on you may be able to describe them, and to answer questions concerning them. When I say "Now" take up the uppermost card, and look at it till I say "Now" again; then put it face downwards on the table and take up the second, looking at that also till I say "Now." Treat the other cards in the same way.

The first descriptions of the cards were given, and questions answered, after an interval of 30 minutes from the observation of the last of the five faces. After a week a second description was given, again after a fortnight, and, whenever possible, again after a much longer period. The cards were not exposed for observation more than once.

Nineteen subjects were examined. Of these 9 were women and 10 men, and all were of adult age.

In a second group of experiments the material to be reproduced consisted of picture signs with which certain words were associated. There were three different series of signs, many of them taken directly from pictographic writing. Signs and words were written upon plain cards, each associated pair having a separate card. A series of cards was placed face downwards on a table. The instructions were:

> On these cards are a number of signs for words. I want you to learn to associate the sign with the word, so that afterwards if you were given the word you could reproduce the appropriate sign. You may learn the signs in any way and in any order

that you please. You will have ample time to learn them but if you think you have established the associations before I tell you to stop, let me know and I will take the cards away.

At the completion of the learning period, after an interval of fifteen minutes, the subject was told:

> Now I am going to give you a piece of dictation, in which some of the words that had signs will be used. Whenever a sign-word comes, write the sign for it. Work as quickly as possible.

After the lapse of a fortnight a further piece of dictation was given, again after the lapse of another fortnight, and later still whenever possible. The actual signs were exposed for observation once only.

This 'method of picture writing,' as it may be called, was adopted, among other reasons, because the definite demand for a specific description was often taken to mean that a subject was being directly required to attempt to evoke a visual image. It was thought that the substitution of graphic representations, effected in the course of rapid writing, for detailed and deliberate description might leave a subject more free to adopt his ordinary methods of reproduction. And this was, in fact, what occurred.

For this group of experiments 17 subjects were employed, of whom 8 were women and 9 were men. As before, they were all of adult age.

In describing the experimental results obtained I propose to confine attention strictly to those that are relevant to the purpose of the present paper. Consequently no attempt will be made to analyse the results in very great detail. For the same reason it appears unnecessary to enter more fully into the exact characters of the material and methods employed. Sufficient has been said to render clear enough the nature of the situations which gave rise to the suggestions about to be advanced.

It will be seen that the paper deals directly with 'reproductive' images only.

§ 2. THE EXPERIMENTAL RESULTS.

I. The General Influence upon Sensory Imagery of Affective Factors.

As was expected, the post-card representations of soldiers and sailors aroused in nearly all subjects very considerable interest, and their perception was accompanied by no small amount of affective tone. This feeling, and the attitudes to which it was attached—of acceptance, satisfaction, dislike, disapproval, and so on—commonly assumed an important *rôle* in determining the process of recall. As a rule, the affective tone clustered about the 'general impression' of a face. It appeared as soon as a card was exposed to observation, and persisted throughout the course of the experiment. For example, a subject described one face as that of a "very serious-looking young man." This was a 'general impression,' as she said, not based upon any definite analysis of the picture, but in so far as it was attached to any particular feature, attributed vaguely to some effect of the chin. When the first description was called for, the affective accompaniment persisted or recurred. The face was characterized as 'serious,' and 'very determined looking.' The impression was, though indefinitely, set down to the chin, and accordingly the face was—wrongly—turned into complete profile. But the definite turning of the face into profile was rather the result of an attempt to account for a 'general impression' of seriousness, than a direct sensory image evoked immediately through the persistence of the affective accompaniment. After the lapse of three weeks the impression of seriousness had become greatly intensified: "This is the young man in profile to the right. He had a square face, and is very serious and determined looking." What occurred in this instance was that the feeling aroused by the original perception of the face persisted or recurred. With it came, in vaguest possible terms of sensory imagery, an 'impression' of a serious countenance. The seriousness was, equally vaguely, connected with the lower part of the face, and so in the end a definite square chin seen in complete profile was evolved.

In general, it appears certain that the image which is reinstated mainly by the aid of affective cues tends to be of a vague and unanalysed character.

> "I think," said one subject, "that this man was a sailor, but I'm not sure about him. He has almost completely faded out. That's all I can say. I have no picture of the man....I've a very vague impression of a rather jolly face."

When the whole of the experiment was over this subject saw the card in question again.

> "That's very curious," he said. "About that card, I had the vaguest possible impression of a genial countenance. It was a sort of undefined and shifting frame, but the real face would fit into it very well. It seemed all the while to hover on the brink of being an image. It was not a visual image, and yet I think it came near to being one; and perhaps it was only the extreme indefiniteness that stood in the way."

The whole bearing and evidence of this subject showed that his behaviour in this case was very largely affective.

> "What I seemed to be going upon chiefly," said another subject, "was the general impression. You see a thing, and that creates an atmosphere. Then the atmosphere seems to cling on, or to come up again, and you get an image of the thing."

One observer gave four sittings with the picture-sign material. At first she relied mainly upon direct cues which recurred in visual form. In the subsequent tests she showed herself distinctly more sensitive to affective factors. On the final occasion, after an interval, from the latest test, of exactly four months, she strongly maintained that absolutely *all* the reproductions she effected were now carried out by the help of a peculiar 'feeling,' which she could not properly analyse but which appeared to be a compound of affective and motor constituents. The reproductions now became characteristically vague, losing their individuality of form. Where several different signs were said to give rise to the same general feeling-tone, confusion of the signs in question practically always occurred.

In sum, the suggestion is clear, that the function of affection in reproduction was to reinstate a *situation* rather than a specific object, and consequently that the sensory imagery to which it might lead tended to be vague, schematic and relatively undifferentiated.

II. THE MAIN CHARACTERISTICS OF DIRECT SENSORY IMAGERY.

In what follows I shall take it that visual imagery is typical of direct sensory imagery of all kinds. Allowing, that is, for differences in mode, it will be assumed that visual reproduction serves the same functions as reproduction in terms of hearing, of smell, or of any other sense.

(*a*) *Visualisation and confidence.*

Throughout the whole of the experiments, as consistently as is ever the case in psychology, the occurrence of definite visual imagery tended to set up an attitude of confidence in the accuracy of the reproductions. For example, a subject who was not normally a good visualiser, had described one of the cards already referred to in a halting manner. He was then asked a question, and said: "That question brought back an image." He described the face minutely, adding: "It became distinct when you asked the question, and that was the first time I really had an image of the card." Another subject had returned a most indecisive series of answers to questions concerning some of the special characteristics of a face. Suddenly his whole demeanour changed. He became confident and said: "I had an image just then."

It was not only in the card series that this confident attitude was marked. With totally different material, and with different subjects, the picture-signs brought out exactly the same characteristic. Marked visualisers throughout worked rapidly and with the utmost confidence, although, as a matter of fact their reproductions were frequently erroneous.

(*b*) *The clearness of the visual image.*

While the general feeling-tone attaching to a whole situation undoubtedly facilitated its reproduction in crude image form, the actual clearness of the image usually seemed to be traceable to some 'striking' detail[1]. This appeared most distinctly from the results of the postcard series. For in this set of experiments it was possible to get definite information concerning the relative clearness of the different faces in the series for a given subject. If we allow for the well-known superiority of the first-presented member of a series in respect of the clearness of its reproduction, we shall find that in 75 per cent. of the total number of reproductions those of one face were clearer than those of any of the others. This face is marked by a large and noticeable moustache, and to the moustache the clearness of the reproduction was practically always attributed.

Thus if an object or situation as a whole merely gives rise to a general pleasing affect, while we are likely to recur to it readily, we are not likely to be able to reproduce it very definitely. But when some detail of a presented object is unusually outstanding, the other features are readily grouped around this detail, and the whole image tends to acquire a distinct and well-defined character.

The conditions which make any particular detail striking call for careful additional investigation. We gain nothing of psychological importance by saying that the 'striking' detail is one which appears 'comic,' or 'out of the way,' or 'pleasing.' For we are then merely left with the further question as to what types of detail do so appear, and why they produce their effect.

Every observer brings to his task of interpreting and responding to a presented situation more or less well-marked tendencies to behaviour and to affectivity. Some of these are general and instinctive; others are personal, and belong to the nature of individual interests or temperamental differences. Certain details in presented situations excite these tendencies more readily than do others. These constitute, probably, the most numerous class of the details which are designated 'striking,' and it is upon them that the formation of clear and distinct images in the main depends.

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[1] Cf. J. Philippe: "Qu'est-ce qu'une image mentale?" *Rev. philos.* 1902, LIV. 49–51.

(*c*) *Visualisation and an order of sequence.*

If we consider the postcard experiments, some very interesting suggestions arise with regard to the capability of sensory imagery for dealing with 'orders of sequence.'

Every one of the errors made in reporting on the order in which the faces had been presented was made by subjects who appeared to be relying mainly upon a visual mode of reproduction. Of the 19 subjects, 12 were dominantly of visual, and the other 7 more or less clearly of 'vocal' type; 8 of the 12 visualisers made mistakes as to the order of presentation, but none of the vocalisers.

This leads us at once to suspect that the difficulty of retaining an order of sequence is connected with the oft-noted characteristic individuality of the sensory image. The members of the series presented in succession are, that is to say, apprehended by the visualisers in relative isolation. General characters such as a law of order or of successive relation cannot be directly and conveniently dealt with by sensory imagery, owing to its dominantly concrete character (cf. pp. 329, 331 *et seq.*).

Again the very fact that in a succession of images the different members may display very great divergencies as to clearness, may itself lead to difficulty in the reproduction of order. For perhaps the members most clearly reproduced tend to get thrust forward into an early place in the series.

(*d*) *Changes in visual images.*

The most definite evidence as to changes introduced into successive reproductions is to be drawn from the picture-sign experiments. The total number of test words dealt with by my subjects was 920. The main transformations that occurred may be comprised under the heads of: omissions, reversals, blending, substitution, simplification and elaboration, and inventions. In all they give 470 instances of change, and when it is remembered that of 53 cases of blending, each involves at least two signs, while some involve three signs, it will be clear that changes affecting the reproduction of material of this nature are very frequent indeed.

An analysis of the modes and conditions of change is of great interest, but would lead away from the subject of the present paper. It is of direct interest, however, that visual images are affected by such transformations to a very great extent. For example one of the best of the visualisers introduced changes into 80 per cent. of the signs with which he dealt, and just over 50 per cent. of these transformations occurred after an interval of more than a week had elapsed since the actual observation.

This result may be fairly regarded as representative of what commonly occurred. That is, if reproductions are obtained in terms of sensory imagery at increasing intervals of time, transformations are unwittingly effected. Processes of change may continue for long periods and in fact for as long as the image may be evoked. The transformations may be of all kinds, but it is of interest to note that all of the subjects who showed any marked tendency towards inventions were adopting the plan of relying upon visual imagery. The presumption is, in fact, very strong that invention, the bringing in of totally new details, is particularly prone to occur in the course of the use of sensory imagery.

III. How the Use of Words affects Direct Sensory Imagery.

There is one medium of reproduction which may be used with equal suitability, whatever the mode of sensory experience to which the object was originally presented. We cannot, in any strict sense of the word, visualise a taste or a smell or a sound, but we can name them all equally well. Thus all those situations which often evoke sensory imagery may be dealt with by the help of words, and words can equally well be employed in other cases also. It is a matter of common observation that many persons, if they have occasion to recur in mind to some object or situation which has been presented visually, do so by finding a series of names for the whole and its parts, without the least resort to visualisation. The words, in this case, act in the place of the sensory images; and, in much the same way, sensory images may, on other occasions, be said to act in the place of words. Very often both are used, and it is in such cases especially that sensory imagery is apt to be of a shortened and fragmentary form.

Now in dealing with the relation between the use of words and direct sensory imagery the distinction between what may be called verbalisation and vocalisation is important. The term verbalisation simply indicates the 'employment of words.' But the mere use of words may not of itself displace sensory imagery. For words may be imaged just as well as any other objects, and may appear in visual or auditory forms. But in vocalisation, which is a special type of verbalisation, the words employed are produced in external or in internal speech; and it is perfectly possible for them to be employed entirely apart from any sensory imagery of the object to which the word refers or, in some instances, of the words themselves. This situation in fact was of very common occurrence in the experiments with which the present paper is concerned.

Further, it is apparent that words may be strung together and spoken by mere habit. There are word-movement habits, just as definitely as there are hand- or leg-movement habits, as the every-day conversation of most people will very well show. But this mere habitual repetition of phrases is to be distinguished from vocalisation proper.

Accordingly in what follows I propose to employ the term 'vocalisation' to indicate the use of words as a means of reproduction, when neither the words nor the objects to which the words refer are necessarily present in any form of sensory imagery; and further when the words are not merely strung together so as to form an automatic series. Such vocalisation affects sensory imagery in ways which it is important to discuss.

(*a*) *Vocalisation and confidence.*

As definitely as the visualising subjects were confident the vocalising subjects tended to be doubtful. They were prone to enter into long explanations and justifications of their attempted reproductions. An extract from my notes, typical of the behaviour of these subjects, may be given:

> A fortnight later (*i.e.* after the first exposure of the signs) there was a further test. Subject F hesitated at the word 'broad.' He was very uncertain, and said, "Oh, this is hopeless. No; I can't think of anything. But there may have been a sign." At nearly every word he continued to pause, saying: "Now let me see, have I had a sign for that?"

Similar remarks are made concerning the procedure of all of the vocalising subjects, and the doubtful attitude adopted was so constant and so marked as to make it appear to me exceedingly unlikely that the diminution of confidence was unconnected with the increased reliance upon words.

(*b*) *Vocalisation and clearness in reproduction.*

General affective factors and 'striking' details had precisely the same effects as in the case of visualisation, nor did they appear to be less commonly operative. But while the subjects who relied upon sensory imagery, as a rule, adopted a method of direct evocation of the reproduction required, the vocalisers all relied to a far greater degree upon indirect reinstatement through associations. The common procedure was that when a sign was first exposed the subject remarked—usually aloud —"that reminds me of so-and-so," or "that is like so-and-so." When the time for the reproduction arrived, first the phrase employed or the name for the associated object would be given, and then the desired sign. The associations used were very frequently indeed suggested by some detail of a sign, rather than by its form or meaning as a whole. Thus the use of words tended to set up a more analytic attitude, and as a consequence 'striking' detail tended to be somewhat more often noticed.

(c) *Vocalisation and an order of sequence.*

It has already been stated (see p. 326) that the vocalising subject attains a distinctly superior position as regards accuracy in the reproduction of an order of sequence. In the postcard series absolutely no mistake was made as to order of presentation by a subject who relied mainly upon words. This was connected with the tendency on the part of the vocalisers to analyse whatever was presented for their observation. Once an order of arrangement was definitely attended to, words became a great help. For serial relations can be named as definitely as concrete objects. On a first examination of the postcards the subject would say: "*First* A, *second* B, *third* C," and so on, and the order would thus be at least as definitely fixed as the nature of the members which made up the series. In subsequent reproduction therefore, the order was preserved correctly; for the analysing attitude induced by the use of words directs attention upon the ways in which the members of an order are related one to another; and once the relations in question have been discriminated, words form entirely suitable instruments by which they may be retained.

(d) *Vocalisation and changes in reproduction.*

Reproductions effected by the aid of the use of words were just as subject to transformation as were those expressed in direct sensory imagery. Moreover, the changes were of an exactly parallel character, and continued over as long a period. But in one important respect there was a difference. While the visualiser with his direct individualising tendencies, was rather more inclined to invent new details, the vocaliser, with his bent towards analysis, was prone to blend and confuse the old details. Both with the postcards and with the picture-signs the vocalisers tended to group together some, or all of the members of a series and to formulate some law of their relation. Twice, for example, in the postcard series, subjects who relied upon words formulated a 'law' as to the 'direction of regard' of the different faces. They averred that "the successive faces turned gradually from complete left profile to complete right profile." The influence of such formulated 'laws,' which were generally too simple to meet the actual facts, definitely increased with lapse of time, and often led to a transference of detail from one card and its blending with that of another. To this, largely, was due the greater tendency on the part of the vocaliser to condensation or blending.

§ 3. THEORETICAL CONSIDERATIONS.

(a) *The most primitive type of sensory imagery.*

Upon the basis of the experimental results which have been briefly described, I propose now to offer some suggestions as to the parts played by images, and by the use of words, in the psychological processes of reproduction.

The first question is as to the most general circumstances in which a sensory image is evolved in order to facilitate a reference to some object or situation not immediately present. In all experiments the image is called up under the direct influence of a 'demand for reproduction.' But clearly this is not the case, as a rule, outside the laboratory. We normally make use of sensory imagery, not to satisfy either an experimenter's or our own curiosity, but to aid our adaptation to some practical situation which has arisen.

Now the image which is merely 'of a situation' appears to be most clearly connected with a general affective tendency (see pp. 322–324). Such affective tendencies, and also moods, and attitudes, are never specialised to particular and individually discriminated objects. They belong to situations as such. They are relatively persistent factors in human response, and may attach to situations which among themselves differ widely.

As we approach the primitive type of organism we find that the modes of response become relatively fewer, and presumably the number of clearly differentiated situations become fewer also. Very detailed and distinct imagery, which is bound up with some narrow and particular set of circumstances, would thus appear often to be a hindrance rather than a help. For it would bring before attention the differences between situations rather than their general likeness. It is hence plausible to suggest that primitive sensory imagery must be of a vague, schematic and undifferentiated character. As such it might well facilitate a *type* of response, while it would in no way lead to very specialised modes of behaviour.

The experiments suggest strongly that sensory imagery of this character is most frequently found to be connected with the persistence or reinstatement of some affective tendency. Whenever an affectively determined image of this kind occurred it was strikingly inadequate to effect a desired detailed reproduction. It was common at one time to speak of schematic images which are general because, owing to growth in experience, they have *lost* their particular characteristics[1]. But it has been less usual to speak of a stage in the development of image forms, in which the imagery, though it is of a sensory character, is essentially vague and schematic, because it has not yet *acquired* individual characteristics. I believe that such a stage does occur, that the function of the image at this stage is wholly to initiate and further some familiar mode of behaviour, and that its occurrence requires the persistence or reinstatement of general affective tendencies, of attitudes, or of moods, which have accompanied prior acts of perception.

It is not, of course, urged that the subjects of my experiments had actually returned for the time to a primitive state, but it *is* urged that in so far as their sensory imagery was affectively determined, they were reacting in a relatively primitive manner. And this is not surprising since the factors of affective tone, moods and attitudes, which are undoubtedly of an elemental nature, persist and may retain their old functions throughout the whole story of mental development.

(*b*) *The image as an aid to a checked reaction.*

But the sensory image in its normal occurrence, both under experimental conditions and in everyday life, has a distinctively individual character. Like the content of an act of perception, it bears all the marks of particularity. This is well explained if we take it that the direct sensory image arises in two typical situations: (1) when a set of circumstances, being only partially reinstated, the necessary practical adaptation is temporarily held in check, and (2) when response to some exciting condition is called for, but for some reason or another, ordinary forms of practical behaviour are prevented from occurring.

Supposing a reaction similar to one previously performed is checked, then a reproduction in image form of the situation in response to which it formerly occurred may arise, and may act as a reinforcing stimulus to bring about the needed reaction. This was what happened in the course of my experiments. Whenever the demand for reinstatement of some already apprehended content was checked, a definite sensory image was more likely to appear. But the principle seems to operate independently of a direct demand for reproduction. For example, as a result of very different experiments on "Conditions which arouse Mental Images in Thought," Fox maintains that his investigations "show that any delay or conflict in consciousness is a favourable condition for arousing a relevant mental image, that is one that will in some way help towards a cessation of the conflict[1]."

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[1] See *e.g.* discussion and refs. in Stout's *Analytic Psychology*, London, 1909, II. ch. IX.

It is interesting to consider that the holding in check of an active tendency—whether to know or to do something—is precisely the most characteristic condition for the arousal of certain kinds of specific emotion, as contrasted with that merely general affective 'atmosphere' of which mention has already been made. The nature of the relation between specific emotion and sensory imagery is a matter demanding much further investigation, but nobody can doubt that the connexion is a very close one. Intense emotional discharge frequently turns an indecisive reaction into one that is both rapid and emphatic. Sensory imagery and certain of the specific emotions, therefore, may well arise under much the same conditions, and may act in concert to promote the same end.

Supposing this view to be correct, we have a perfect explanation of the confidence which, in my experiments, commonly accompanied the appearance of sensory imagery. For the primary function of the image is to manifest itself when a reaction is hesitating, and to lead to a response which is decisive. It was no accident therefore that the visualising subjects all showed superior certainty, since a sensory image, by its nature, leads to decisiveness of response. And this decisiveness is produced independently of the actual accuracy of the image.

Take next the apparent inadequacy of the sensory image in the reproduction of a correct order of sequence. This character is also perfectly consistent with the view that it is a leading function of imagery of this kind to help to produce an immediate and emphatic reaction. For the more narrow, and individual, and definite the limits of apprehension of an object or a situation, the more emphatic will the response tend to become. We often speak of the decisive man as responding "as if there were nothing else in the world but the one object of his reaction." Hence it is not to be wondered that the characteristic image reproduces as exactly as possible the sense marks of a particular situation. It is the clearness and vividness, not the order of arrangement, of the detail which are important. Order is, in fact, a general characteristic which can be represented in sensory terms only by the use of some cumbrous figure scheme, of the sort employed, for instance, by many of Galton's subjects[2].

The function of maintaining an order of sequence is, indeed, one to which nearly all forms of sensory imagery are strikingly ill-adapted while, as we have already seen, words are as strikingly well-adapted. I say "nearly all forms of sensory imagery" because it may be maintained that kinaesthetic imagery is in some respects specially qualified to perform this function. Whether this is so or not is entirely a question for experiment, and my own data throw no light whatever on the matter. But it should be pointed out that in so far as a kinaesthetic series maintains correctly an order of sequence, it never does so by virtue of isolating and responding to the relations themselves which are illustrated in the series. The kinaesthetic series is really of the nature of a habit response. If any member of the series should lose any of its characteristics, then the order of the series is also likely to be disturbed. For in such a series passage is made from one member to the next solely by virtue of some special characteristic belonging to the former. This is not necessarily the case in the vocalised series, where the relations of order may be themselves analysed, and response may be made distinctively to such relations. For then, even though in their reproduction the separate members of the series may suffer change, the order itself may—though, of course, it does not in all instances—remain undisturbed.

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[1] This *Journal*, VI. 430.

[2] See *Inquiries into the Human Faculty*: section on *Number-Forms*.

The general working hypothesis of this section demands that sensory imagery should possess such a direct clearness and vividness as would facilitate immediate and emphatic response. Now the experiments show that the clearness of the visual image is in the main a matter of 'striking' detail, and they suggest that the 'striking' character of particular detail is due to its relation to pre-existing active and affective tendencies in the individual concerned. But my visualising subjects, as contrasted with the vocalisers, reproduced in a direct and immediate fashion, and not by virtue of associations. That is, the 'striking' detail in the first case itself 'hit off' the tendency so to speak, whereas in the second case it did so often only by virtue of its association with something else. In this way, it may be suggested, the clearness and vividness of sensory imagery are direct and immediate, and belong entirely to the object or situation imaged.

The necessity for securing definiteness and individuality in the sensory image, however, comes into conflict with the equally great necessity for obtaining ready adaptation to the constantly changing demands of daily life. Thus even the sensory image undergoes constant and prolonged transformation. This constitutes at once an advantage and a danger: an advantage because, except in the psychological laboratory, occasion practically never arises for sheer, blank repetition;

and a danger because, owing largely to their incommunicable nature, those changes in sensory imagery which may lead to serious error are exceedingly likely to go uncorrected[1]. If I go into a room in the dark, thinking that there is nothing in the way, and hurt myself, the pain corrects for me my error, and I shall go warily upon another occasion. But if I merely picture a church as having a spire on it, when as a matter of fact it has none, my image cannot conflict with anything to its own detriment, and may remain incorrect. It is interesting that these experiments indicate the possibility that inventions are somewhat more likely to occur in the case of sensory imagery than with the use of words. For the introduction of new detail is what might perhaps be expected in a mode of reproduction which for the most part deals directly with individual instances.

In the relative freedom of the sensory image from correction, we have perhaps another reason for the confidence which attends its use. The image must change, since it has to serve the demands of practice in a world which never remains the same from moment to moment. But its changes induce no access of dissatisfaction since, in the main, they tend to be beyond the reach of correction.

(c) *The image as an alternative reaction.*

Imagery of the sensory type, it was suggested, is likely to occur in two typical situations. The first, that of the checked reaction, has already been considered, the second, that of the alternative reaction, remains to be discussed. In this instance response to some exciting conditions is called for, but normal adjustments are out of the question for some reason or other. The imagery which arises under these conditions has, it appears to me, a line of development entirely its own. We get here the extremely vivid sensory imagery which is characteristic of dreams, of reverie, of fatigue, of certain states of drug intoxication, and the like. This is the type, and this is the general condition, of a considerable amount of the imagery which has a large part to play in the development of the arts, and its consideration is fundamental to the construction of a sound psychological aesthetics. The majority of the factors which determine the form, subject-matter, and clearness of such images operates in the manner which Rivers has called 'unwitting[2].' But the experiments which I have described in the early part of this paper do not deal with the image as an alternative reaction, or with its peculiar problems. Consequently I do not now propose to attempt to discuss the questions involved.

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[1] Cf. J. Philippe: "Sur les Transformations de nos Images Mentales," *Rev. philos.* 1897, 481–493.

[2] *Instinct and the Unconscious.* Cambridge, 1920, 16.

(*d*) *Why words tend to be substituted for sensory imagery.*

As we have seen, the marked individual character of sensory images, and the attitude of certainty which attends their use, arise in the main from the function of such images as aiding a checked reaction. But the whole trend of mental development is away from the particular and the individual. Analysis and construction come to play a greater and greater part, and by their aid, situations, which, as regards many of their characters, differ very widely, are brought into relation. No method of reproduction which is largely confined to the reinstatement of individual instances can possibly remain adequate if intellectual and practical development is to proceed. In particular it has often been pointed out that the apprehension of general relation of likeness, which may lead to all kinds of arguments from analogy, is of the greatest importance for intellectual advance.

Now the data of the experiments suggest as strongly as possible that it is precisely the analysing and relating attitude that the use of words favours. As compared with the visualiser, who goes directly to his particular material, the vocaliser commonly secures his reproduction of an absent object or situation by an indirect method. He relies upon association. Moreover the associations are in the majority of instances suggested, not by the object considered as a whole, but by some feature of the object. Indeed many divergent associations are often formed, some with this detail and some with that.

Here we find one of the reasons why vocalisation as compared with visualisation proved to be accompanied by an attitude of uncertainty. The associations formed in connexion with a single object frequently varied so greatly, that when, at the period of reproduction, a tendency arose now to accept and employ this association, and now to revive and employ that one, hesitation was bound to supervene. For according to which association was used the actual reproduction would vary in its details.

Again very probably the decrease of confidence is due to the fact that, on the whole, the employment of words in reproduction leads away from the reinstatement of material in close relation to emotions. Why and how this should be the case are matters of extreme interest, but are beyond the limits of the present paper. It is worth noting, however, that while development along the line of the sensory image proper may lead, as has already been suggested, to the arts, development along the line of reproduction through the medium of language leads to thinking in the strict sense, with its abstract character and its relative freedom from emotional disturbance.

A third reason for the doubt which often attends vocalisation methods may be due to the fact that words are our habitual instruments of discussion, while obviously no person can conduct a dispute in terms of sensory images merely. A false statement can be as readily checked as, under certain conditions, can a false overt movement. And this consideration should make it clear that the lack of confidence consequent upon the use of words is no serious drawback, for it may easily be dispelled. It does not need to await the sort of Jack-in-the-box, haphazard, appearance of some new sensory image, but can be subjected to the confirmation or denial of a second observer.

The very fact that, by the aid of words, objects and situations are retained and reproduced the more clearly according as they are brought into relation with others, suggests a further great advantage of vocalisation. In these experiments while sensory imagery broke down badly in its attempt to deal with an order of serial relation, the use of words dealt adequately with such an order. Not only do words facilitate an attitude of analysis, but they provide instruments by which general relations may be analysed, and may be indicated fully as definitely as particular objects. In fact if there is any characteristic error incident to the use of words in reproduction it is that of blending or confusion. And this is traceable to that very function which sensory imagery is barely able to perform, the formulation and response to rules and laws by which individuals are related.

Yet with these advantages words lose nothing of the necessary fluidity characteristic of sensory imagery. The capacity for transformation is clearly essential, and, so long as it can readily be checked, wholly advantageous; for all modes of reproduction are primarily meant to serve the purposes of practical adjustment.

It thus appears that words are able to maintain the advantages of sensory imagery, while in addition their ease of communication, and the greater range of situation with which they can adequately deal, make them capable of rendering further development more ready and certain.

An interesting chapter in genetic psychology yet remains to be written on how words actually come to supplement, and, in some cases, to replace, direct sensory imagery, and how, at the same time, the sensory image is not only normally retained as an element in mental life, but may develop along lines of its own, which lead to types of human response of the very greatest psychological interest.

In this paper I have, for the purposes of clearness, spoken sometimes as if sensory imagery and methods of vocalisation are separate, and even, to some extent, antagonistic. But in actual life, as need hardly be said, they are most frequently mingled. The nature and conditions of the precise help that each can give to the other must be left over for future discussion.

(*Manuscript received* 27 *August*, 1920.)

A CASE OF SOMNAMBULISM.

By S. M. HAGGARD.

In 1916 I was teaching at a girls' school and chanced to set some of the elder girls, aged about 16½ years, the following essay: How far were Shakespeare's Sonnets characteristic of the Poetry of the Elizabethan Age, and how far did they differ from it?

One of the girls, who had been vainly trying to think of points for her essay, took up to her bedroom a pencil and paper in case she should wake early the next morning. She left them on her dressing table and on washing in the morning was astonished to find them on a table by her bedside. She then noticed that the paper was covered with pencil notes, in her own handwriting (see accompanying plate), which she had no recollection of thinking out or of writing.

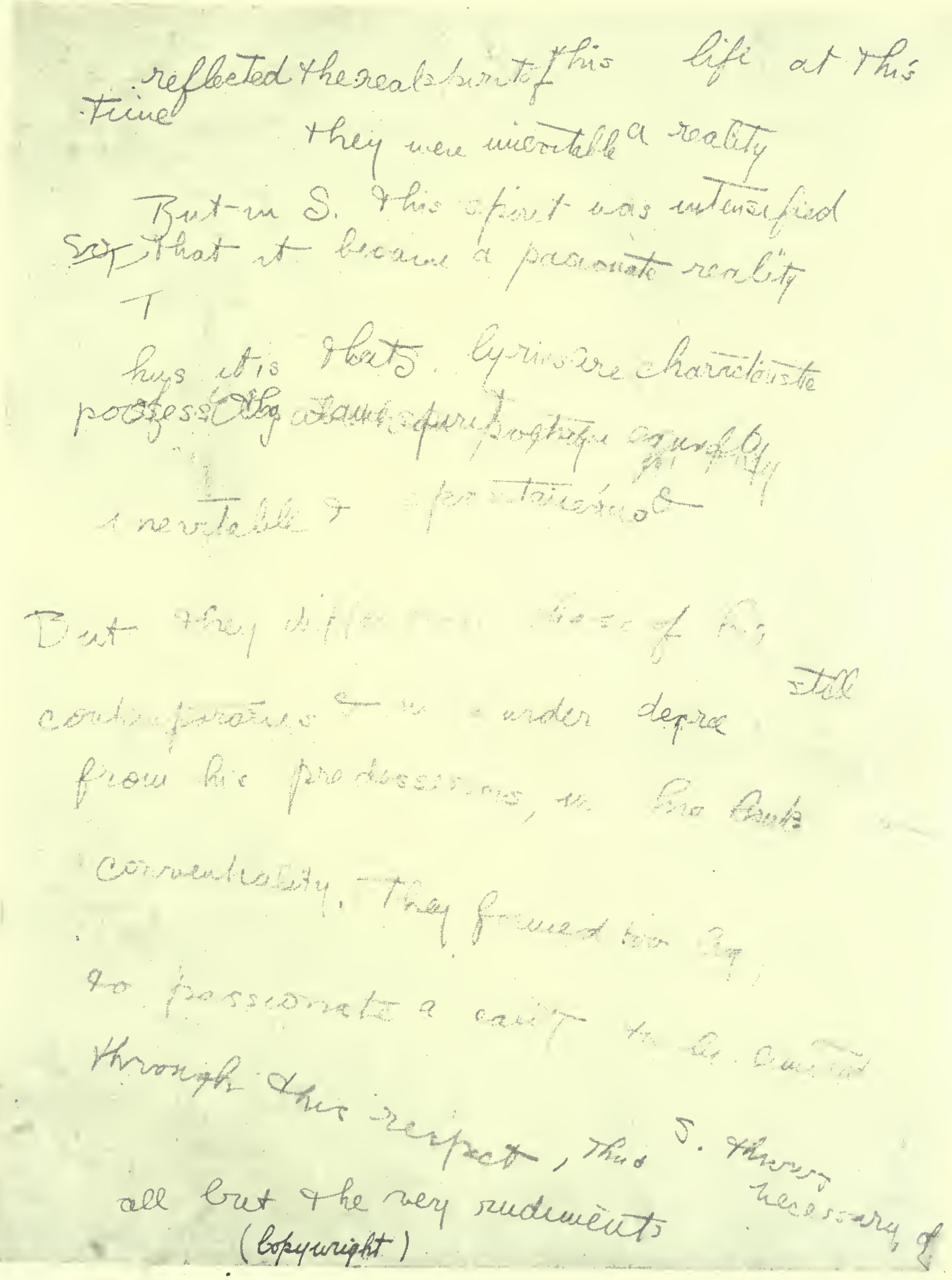
She brought the notes to me to ask if she might make use of them for her essay in view of the circumstances in which they had been written. She thought that it would have been unfair to have done so without asking permission. At the same time she pointed out that after having seen what she had written during her sleep, it would be quite impossible for her to compose the essay on any other lines.

Permission was given to make use of the notes, and the girl's essay was the best sent in, considerably above the usual level of her work at that time. I can vouch for the fact that the ideas contained in the notes were in no way suggested by my own class teaching, nor had the girl had access to any books bearing on the subject.

The following is the transcript of the notes written. The original is reproduced in Plate III.

...reflected the real spirit of this life at this time, they were inevitable, a reality. But in S.[hakespeare] this spirit was intensified so that it became a pasionate reality.

Thus it is that S.[hakespeare] lyrics are characteristic of the Elizabethan poetry for they possess the same spirit, were equally inevitable and spontaneous. But they differ from those of his contemporaries and in a wider degree from his predessessors in his lack of conventiality. They formed too big, too passionate a reality to be limited through this respect. Thus S.[hakespeare] throws all but the very rudiments... necessary of....



reflected the realshirt of this life at this time
they were inevitable a reality

But in S. this spirit was intensified
So that it became a passionate reality

T
hus it is that S. lyrics are characteristic
possessing the same spirit poetique equally
inevitable & [illegible] [illegible]

But they differ [illegible] those of his
contemporaries [illegible] under degree still
from his predecessors, in [illegible] [illegible]
conventionality. They formed [illegible] [illegible]
so passionate a cult [illegible] [illegible] [illegible]
through this respect, thus S. threw
all but the very rudiments necessary of
(Copyright)

It will be observed that the girl has jestingly written the word 'copyright' (mis-spelt) at the bottom of the sheet of notes. This word she wrote before finally parting with the paper and serves as the only available means of comparing her normal handwriting with the handwriting of the notes, as the event occurred over four years ago, and the girl has long ago left school. The following sentences, however, are of psychological interest, extracted from letters written by her to the Editor of this *Journal*: "I am afraid I had forgotten the incident of this essay or that any notes were in existence, until I received Miss Haggard's letter a few days ago." "I cannot remember what the notes were or what they looked like, and though I have enquired about the subject of the essay, the title of the subject conveys nothing to me." "Now that I have seen the notes, I certainly seem to recognize them as having been written by me, that is to say, I seem to recognize the idea running through them... only I cannot understand how I should forget having written them." "You ask if I have ever been known to do any sleep-walking. As far as I know I have done so twice. Once while I was ill and once later for no apparent reason. [Then] I did not leave the room or do anything else than walk up and down the bedroom. I have a brother who sometimes talks in his sleep, but no other members of the family have been known to do anything."

CRITICAL NOTICE.

BY F. C. BARTLETT.

The Origin of Man and of his Superstitions, by CARVETH READ, M.A., Lecturer on Comparative Psychology (formerly Grote Professor of Philosophy) in University College, London. Cambridge: The University Press, 1920. pp. xii + 350. 18*s*. net.

Poets, priests and philosophers have always sung, dogmatized and speculated about the origin of man. Since the birth of scientific method they have been joined in their quest by the most varied company of investigators. Biologists, physiologists, neurologists, anatomists, zoologists, geologists, anthropologists of every kind and degree have all passed beyond their more immediate concerns to suggest the modes by which man may have lifted himself to his present place in the world. "The quest of origins," says Prof. Carveth Read, "is fascinating"; and, of the many statements made in this book, here is one which is perhaps least of all likely to arouse controversy.

It is as a psychologist that Prof. Read approaches his problem. He loses no time in stating his hypothesis, and no opportunity of repeating and enforcing it. The very first section of the book is entitled "The hypothesis," and the position on which the whole of the work is said to be based is stated clearly in the second paragraph of this section:—the chief differences "between man and his nearest relatives" may "all be traced to the influence of one variation operating amongst the original anthropoid conditions. That variation was the adoption of a flesh-diet and the habits of a hunter in order to obtain it." Further a flesh-diet apart from hunting "could have done nothing for the evolution of our stock." The adoption of the hunting life, therefore, is the essential variation upon which everything else depends. Nothing, the author is assured, could be more certain than that some members, or member of the anthropoid stock adopted a hunting habit, and that from this follows the whole long story of human development.

In the first two chapters a variety of mental and physical characteristics of man are traced more or less immediately to the toils, pleasures or duties of the chase. Man's life on the ground; his customary erect attitude; the specialisation of legs and feet and particularly of arms and hands; reduction in the size of jaws and teeth; the development of a big brain; the adoption of social life; the birth of language proper; the production and use of fire; a slight shortening of the alimentary canal; the loss of seasonal marriage; the acquisition of a relatively naked skin; the cannibalism prevalent at certain stages; the extreme variability of racial characteristics; disposition to slay and destructiveness; perceptive, contagious and effective sympathy; hostility to all who are outside the hunting group; the institution of property; leadership; subserviency; emulation; social orders of precedence; intelligent coöperative behaviour; the constructive impulse; monogamy; war and all of its results; play; social eating and weeping; the gradual moralisation of man—all these are traced directly or indirectly to hunting practices. The development of the higher mental functions can be traced to the same source also: "The necessity of learning the whole art of hunting from its rudiments," he writes, "without the help of instinct or tradition, by sheer observation, memory and inference, put extraordinary stress upon the brain. At first by knowledge, strategy, coöperation and persistence of will, later by devising weapons and snares, evolving language and discovering the ways of making and utilizing fire, man found means of entirely changing the condition of his life." Thus Eoanthropus needed, and developed, a big brain, and with this "a continually increasing fecundity of ideas." The life of the imagination grew directly out of the manifold activities of the hunter, and once it was rendered possible, assumed most important functions in strengthening and reinforcing the social bonds which were first forged by the chase. This it did by virtue of its intimate relationship to belief. So the book passes from a consideration of the origins of man to a study of the growth of superstition.

At this point the volume takes a great leap forward in interest and importance. The first two chapters contain some facts, no small amount of most ingenious speculation, and much excellent writing. But many readers will feel that hunting is made to bear an over-large burden. There seems little reason to suppose that the primitive hunter worked entirely without the aid of instinct or tradition, or that hunting is more than one of the many conditions of human development. In fact the author himself indicates (p. 48) that other kinds of associations, besides that of the hunting pack, might well favour the development of certain of the characteristics of man. Why should it be necessary to trace all types of human response back to one common motive?

Chapter III consists of a most admirable study of Imagination, its relation to Belief, the main causes and grounds of Belief at different levels of mental development, and the influence upon primitive modes of behaviour of Reasoning and Ideas. The study of 'play-belief' is of particular interest, though it may be doubted strongly whether the savage has 'no seniors,' and only 'tardy experience or social fatigue to check' the vagaries of his imagination. It was also well worth while to insist upon the part which "general ideas at the savage level" may play, and there are a number of good illustrations of this part in the subsequent discussions of Magic and Animism.

Chapters IV–VII deal with Magic, Animism, the Relations between Magic and Animism, and Omens. Here are gathered together numerous illustrations of the operation of the processes discussed in the third chapter. Magic is defined as a connexion of events imagined as constant, and dependent upon some impersonal agency. This is something like Frazer's idea of Magic as a sort of primitive natural science, but the author dissents definitely from Frazer's views as to its relation to Animism. Magic comes in when the cohesion of the hunting pack has weakened; its main social purpose is to strengthen the authority of the elders. Animism is either 'hyperphysical' when it is an imagination-belief that various natural occurrences are due to the direct action of some kind of spirits; or 'psychological,' when it marks the attitude of regarding all things, animate or inanimate, as having a certain consciousness, and all reactions as voluntary. On the whole the author inclines to the view that Magic is prior to Animism, though commonly the two are found closely intermixed. "At any rate, their origins are independent": their subsequent relations may be varied.

Leaving without comment the three extremely interesting chapters on Omens, The Mind of the Wizard, and Totemism, we come to the final study on Magic and Science. "Magic and Science have, indeed, some common ground; for both are products of our poor human mind, which is sorely pestered in explaining its experiences by the notion of 'forces' that somehow bring about events, and which cannot get on at all without assuming uniformities of nature." But, each starting from Common Sense, Magic soon distorts its facts, while Science systematizes them. The differences which arise are due, in fact, to psychological considerations. For the determining beliefs of magic are "incoherent and vague," those of science are "coherent and definite." Though the procedure of Magic and that of Science differ, yet the magician is certainly the precursor of the scientist. "In the course of thousands of years some men grew more interested in the positive than in the magical side of their profession, and became scientists."

In this discussion it may be that the author allows hardly sufficient importance to the tendency that may occur, or develop, to seek direct relations of likeness among apparently divergent facts. Perhaps one of the important determining conditions of magic is the attempt to guard certain groups of observations from outside disturbance. It seems possible that sometimes the scientific imagination is incoherent and vague, and the magical 'coherent and definite.' But the latter may be excessively limited. Earlier, in fact, Prof. Read says, "The natural progress of reason consists in relegating analogies to poetry and rhetoric" (p. 98). It may be doubted whether a study of the history of scientific advance would justify this statement. He repeats the suggestion in the final chapter: "for scientific progress it was necessary that reasoning by analogy should be abandoned." As a matter of fact, however, abandoned it never really has been, and science has grown nevertheless.

The book closes upon a gloomy note. Many of the most noble and desirable of the qualities of man, particularly as these have been manifested in social life, have, it is urged, been both acquired and maintained largely "under the influence of animistic illusions." These illusions are fast being dissipated, and with them may go many of the virtues of loyalty, solidarity, and social sympathy upon which human well-being is based. Further, from our beginnings in the hunting pack we have relied to the greatest extent upon leaders. Thus individuality has been but slightly developed, and the majority of mankind are of low average capacity. We grasp and let slip imagination-beliefs in the most careless manner, and irrespective wholly of their worth. But after all, is the conclusion, it is not much good to guess: "The only safe reflexion is that he who lives longest will see most."

Thus closes a thoroughly interesting book. The volume does not stand or fall according to the fate of its 'hunting' hypothesis. Altogether apart from that, it possesses the greatest importance. It raises a very large number of fascinating questions, and discusses them all with great skill and decision. It represents a vast amount of careful research, and is as pleasant to read as it is profitable. The psychological portions of the discussion are the best; and that is to say a very great deal, for it is here chiefly that the majority of the books which have dealt with similar subjects have been most sadly wanting.

CRITICAL NOTICE.

BY F. C. BARTLETT.

The Group Mind. A Sketch of the Principles of Collective Psychology with Some Attempt to Apply them to the Interpretation of National Life and Character. By WILLIAM McDOUGALL, F.R.S. Cambridge: The University Press. 1920. Pp. xvi + 304. 21*s*. net.

It is safe to say that no book on psychology published in this country has been more widely read or more keenly discussed than Prof. McDougall's *Introduction to Social Psychology.* But that volume was proposed merely as a first rough sketch of the subject matter, and principles of a vast field of study. A very large number of readers have thus awaited with interest the promised sequel. To some extent many of them have meanwhile withheld criticism. Though they may have been convinced, for example, that McDougall's treatment of instincts, interesting and suggestive as it is, is incomplete and schematic, they have restrained their impatience, waiting for the fuller discussion that was promised. Now the time of their freedom has arrived, and it is greatly to be hoped that they will take advantage of it.

The Group Mind, its author says, "builds upon" *An Introduction to Social Psychology.* This at once suggests that he is perfectly satisfied with the foundation laid twelve years ago. So much is this the case that the present book, in spite of the great interest and insight of many of its passages, must strike most readers as adding practically nothing of a fundamental nature. Indeed, the author himself recognises this: "Although my fellow-travellers have not entirely approved my outfit, I have launched out to put it to the test"; and "I cannot hope that my readers will follow me if they have not at their command a similar outfit—namely a similar view of the constitution of human nature." Perhaps the best review of *The Group Mind* would consist of a thorough criticism of *An Introduction to Social Psychology.*

However, to describe this book by itself: it is divided into three parts, the first concerned with "General Principles of Collective Psychology"; the second with "The National Mind and Character"; and the third with "The Development of National Mind and Character."

To the whole is attached an Introduction in which McDougall deals with the "province of collective psychology." He there makes his position clear: that social psychology has to do with the reactions of groups as such, and this at once plunges him into the extremely ancient and arid discussion as to whether there is a 'group mind.' Since 'mind' is to McDougall "a system of purposive forces," and it is clear that any group must illustrate a more or less perfect organization of individual tendencies each of which can be regarded as possessing some direction, or goal, it follows that he finds it easy to talk of 'group mind.' And just as in the *Social Psychology*, he treated individual human behaviour as the result of a conflict, or an integration, of a few root tendencies, so in the present book he considers group reactions as the result of a conflict, or an integration, of the tendencies of the individuals who make up the group. But there is an important difference between the two cases. In his earlier book McDougall did, at least, attempt an original study and analysis of human conduct. In the present book he starts with all his root tendencies laid out before him. All the time one feels that this is hampering to any real, unbiassed, analysis.

The first Part describes general characteristics of different types of group organization, as exemplified in the loosely organized crowd, and the strictly organized army. The second Part concentrates upon the "most complex and important kind of group mind, namely the mind of a nation-state." Many most interesting topics are here discussed: what constitutes the mind of a nation; how far a nation-state demands mobility in every sense on the part of its members; the rôle played by leaders in national life; what can be meant by a national will; how it comes that ideas are said to exercise a determining influence upon national destiny; and what characterizes the 'higher' type of nation. Part Three discusses, with a considerable wealth of historical illustration, how nations have grown up; how, when they have already become established, they may yet suffer important change; and finally how their development in the period of their most rapid growth can be differentiated from the character of the advance made once they have won their way to maturity.

In the very first chapter of the book, McDougall reflects on the oft-noted opinion that while "participation in group life degrades the individual," yet "only by participation in group life does man become fully man." "The resolution of this paradox," says he, "is the essential theme of this book," which shows how group organization "counteracts degrading tendencies; and how the better kinds of organisation render group life the great ennobling influence by aid of which alone man rises a little above the animals and may even aspire to fellowship with the angels." The reader will wonder whether he is going to be concerned with psychology or with ethics. As a matter of fact, the two tend to jostle one another all the way through. No doubt it was under the influence of ethical enthusiasm that McDougall turned aside to frequent vituperation of the German, and to repeated laudation of the American and French races. The attitude was natural, and perhaps admirable, during the war. It is a blot upon a book intended to be permanent, and helps to give to many of its pages a character of merely occasional interest, as if one might be reading in a newspaper.

Nevertheless every chapter throughout the book, and particularly in Parts II and III, provides material which might well form a basis for prolonged discussion. For if McDougall introduces no strikingly new principles, he certainly attempts to apply the principles he has elsewhere formulated to the most important problems in the character and development of nations. Take, for example, his treatment of the place of ideas in national life. This follows immediately upon a discussion of the 'will of a nation.' Consequently it begins by a reiteration of the part that ideas may play in volition. Just as in the case of individual acts of will, consideration of a situation in relation to the whole self is involved, so in the case of the 'national will,' the situation desired must be considered in reference to the idea of the nation as a whole. This 'idea of the nation,' however, is by itself ineffective, and as effectiveness is contributed to the idea of the self by the self-regarding sentiment, so the nation-idea is able to exert actual influence upon practice by the development of the nation-regarding sentiment. "The idea alone, as an intellectual apprehension, cannot exert any large influence;...it determines judgment and action only in virtue of the sentiment that grows up around" its object (p. 170).

McDougall gives an illustration as, in fact, he is always ready to do:—"Imagine a people in whom anti-nationalism (in the form of cosmopolitanism, syndicalism, or philosophic anarchism) had spread, until this attitude towards the nation-state as such had become adopted by half its members, while the other half remained patriotic. Then there would be acute conflict and discussion and the idea of the nation would be vividly present to all minds; but the nature of the sentiment attached to it would be different, and opposite in the two halves; one of attachment and devotion in the one half; of dislike, or at least indifference (*i.e.* lack of sentiment) in the other half."

Surely it is an incomplete analysis to present this as a case in which different sentiments are attached to the same idea. A cosmopolitan usually has his own idea of a nation-state, and this idea differs from that of the nationalist. There is in general, nowadays, far too marked a tendency to assume that because ideas do not operate *apart* from sentiments, or from some definite affective accompaniment, ideas really contribute nothing *in conjunction with* sentiments, or whatever helps to bring them into play. For all his emphasis upon the importance of the development of self consciousness, and group consciousness; for all his insistence upon progress through 'emancipation of the intellect' (see particularly pp. 297–301 of the present book)—McDougall appears at times to share this tendency. We require a new, thoroughgoing and unbiassed analysis of the part that ideas as such may contribute to a determination of the directions and ends of conduct. Particularly is this necessary in social psychology. A first-hand study of modern labour movements, for example, carried out from a strictly psychological standpoint, would probably throw new light upon the effectiveness of ideas in helping to shape social organization and institutions. It is not enough to assert that this idea, or that one, has been important in social development. We must know how, if at all, an idea, once it has been formed, helps to determine its own emotional colouring. We must be prepared to show how, if at all, the idea, as an idea, operates, and what varied effects different types of idea may produce. And discussion of these matters, if it is to be convincing, must not be shy of passing into details.

Any idea that can be truly called national must, as McDougall most rightly urges, have reference to 'the whole,' and not merely to 'all the members of' a nation. But his argument as it stands in this chapter (Pt. II, Ch. XII) is hardly conclusive. A nation has an indefinite future, it is urged, and hence 'all' at any moment, never really includes 'all' of the group.

Now suppose we should consider, instead of the nation, a short-lived group, formed with a limited membership, for a prescribed period. In this case it might be possible to consider the needs of literally 'all' the members of a group. Yet many would still maintain, and probably McDougall among the others, that the 'good of all' would not be identical with the 'good of the whole.' In social grouping the whole is "much more than the sum of the existing units," not simply "because it has an indefinitely long future before it, and a part to play through indefinitely long periods of time" (p. 171), but because social coöperation does actually bring new psychological factors into being, which, when we are considering individuals alone, do not come before observation. However, these remarks point to a deficiency in this particular passage only, for McDougall himself, in other places, urges as strongly as possible the uniqueness of many of the factors produced through social grouping.

The consideration of "ideas in national life" concludes by a study of "The Value of Nationality." No small part of this most interesting section has a distinctly ethical trend, yet there is much important psychological material in the discussion of how the ideas of "liberty, equality, progress and human solidarity" have helped to shape social organization. Still the precise manner in which ideas operate is left in need of some explication. In this part of the book the terms 'idea' and 'ideal' are used, without comment, almost as if they were interchangeable terms. A careful psychological analysis of their nature and their relations is most desirable.

The study of the development of nations, to which Part III of the volume is devoted, is throughout deeply interesting. The discussion of racial changes during the historical period—which most explicitly denies any "considerable changes of innate qualities"—and the comparison of the nature of the progress of nations in youth and in maturity, raise an enormous number of questions of the first importance. And upon all these questions McDougall is, by a very admirable quality, ready with a decisive answer. The last two chapters are particularly stimulating. McDougall has no doubts as to the character and basis of human progress, past, present and to come; and his views are expressed with a vigour and swing that no reader can fail to enjoy.

Nobody can deny that *The Group Mind* is worth reading. It has many interesting things to say; it provides much food for thought; in many parts it is delightfully written; and it will doubtless form the starting point for many a vigorous debate. Yet it is undoubtedly a disappointment. Twelve years ago its author constructed a scheme, and into it he fitted many of the facts of human behaviour. It was a scheme boldly and brilliantly conceived, but in the opinion of many it was too simple to meet the demands laid upon it. Now with the same scheme in mind—nothing added, and nothing retracted—he strives to set in order the enormously greater complexity of group conduct. In the result it is the scheme that must suffer.

CRITICAL NOTICE.

By T. H. PEAR.

Instinct and the Unconscious, by W. H. R. Rivers, M.D., D.Sc., LL.D., F.R.S. Cambridge: The University Press. 1920. pp. viii + 252. 16*s*. net.

The general aim of this book is "to put into a biological setting the system of psychotherapy which came to be generally adopted in Great Britain in the treatment of the psychoneuroses of war." And, since the exposition is consistently psychological, one may profitably inquire before reading it what conception its author holds of the scope of psychology; an important question in these days. It is given on page 43: "the special task of psychology" is "to understand the place of consciousness in relation to behaviour."

"The main function of a psychoneurosis," Dr Rivers states, "is the solution of a conflict between opposed and incompatible principles of mental activity." In learning the nature and evolution of those principles and the manner in which their conflict is solved the reader will be introduced to an argumentative texture of extraordinarily close weave, containing scarcely a stitch too many; exhibiting a striking and, in many ways, a novel design. Whether, on reaching the end of the book, the student of psychology will find that this new design can be harmonized with the rest of his thought-patterns will be determined by his opinion concerning the closeness with which this garment of theory fits the facts. Dr Rivers himself clearly points out loosenesses here and there.

One demand upon the reader is made which renders any rapid progress through the book impossible. From the very beginning he will be required to disabuse his mind of most of the meanings which it has attached painfully, and with many setbacks, to the current terms of psycho-analysis; even of psychology itself. The Unconscious, Repression, Dissociation, the Complex, Instinct, by the time the end of the book has been reached all these have been 'taken down' and re-built. But the author compensates to some extent for this by freely allowing the reader to see him at this task of dissection, and by explaining, step by step, why such re-moulding seems to him to be necessary.

This re-making process begins, not unexpectedly, with the Unconscious. Three examples are given to illustrate the author's conception of 'unconscious experience,' the distinguishing mark of which is that it "is not capable of being brought into the field of consciousness by any of the ordinary processes of thought or association, but can only be recalled under certain special conditions, such as sleep, hypnotism, the method of free association, and certain pathological states." And "if an idea springs spontaneously into the mind without obvious antecedents in consciousness" Dr Rivers does not speak of it as unconscious but as 'unwitting.'

Repression, with its relation to the process which in this book is called Suppression, is discussed at length, for a grasp of the difference between them is vitally necessary to any understanding of the main theory. And here is seen the use of the term 'witting,' introduced in the preceding chapter. The process by which experience becomes unconscious is called 'suppression,' but 'repression' is used for the process by which we wittingly endeavour to banish experience from consciousness. Repression, moreover, is only one of the ways in which suppression occurs; a conception which is helpful in answering the common objection to the theory of active forgetting: why, when we try to forget a thing, do we usually fail?

In this treatment of forgetting, of which suppression is one form, the great debt owed to Freud for emphasizing the rôle of repression is fully acknowledged. In fact, the treatment of forgetting follows orthodox Freudian lines fairly closely, and it is not until the next chapter, "Suppression and Inhibition," that the book leaves the beaten track. But now its deviation is at right angles to the path, for it plunges into physiology. Its entry is made at that point in time when, in 1902, a few snicks in Dr Henry Head's arm changed the dull text-book chapters on cutaneous sensation into material for the most fascinating speculations upon the behaviour of our own remote ancestors, and, incidentally, gave science two words whose utility is matched only by their euphony: 'protopathic' and 'epicritic.' It is these terms, and the meaning which has grown up around them, which are pivotal to much of Dr Rivers's argument. He claims that when, after operation, epicritic cutaneous sensibility returns, the earlier protopathic sensibility does not persist unaltered side by side with the later development, but that certain of its characters fuse or blend with the epicritic, while others are not only suppressed, but—this is the important point—they persist in a latent form, ready to come again into consciousness if the appropriate conditions are present. That is to say, Freud's conception of the unconscious finds its parallels even at the sensory level.

Dr Rivers then passes to the work of Head and Gordon Holmes, who discovered, between 1911 and 1912, a relation between the cerebral cortex and the optic thalamus very similar to that existing between protopathic and epicritic sensibility. When the cortex cerebri is put out of action, by injury or otherwise, stimulation of the skin produces sensations overweighted affectively, very similar to the experiences of protopathic sensibility, while there is in them an absence of objective character very similar to that observed in this form of sensibility.

He believes that under ordinary conditions the cortex suppresses the affective over-response of the thalamus.

It is, however, the recent work of Head and Riddoch which suggests the most impressive confirmation of the theory of suppression. In 1918, their observations of a number of patients in whom the spinal cord had been completely divided enabled them to examine the functions of the lower end of the cord when isolated from the rest of the nervous system. The diffused and generalised 'mass-reflex' which they discovered (a reflex possessing characters quite unknown when the nervous system is intact,—with its generalised character, its absence of discrimination and localisation, its strong suggestion of a mechanism adapted originally to remove the whole rather than a part of the animal from noxious stimulation—) is considered by Dr Rivers to have suffered, in the normal human being, complete suppression in favour of reflexes delicately graduated according to the locality and, to some extent, according to the nature of the stimulus. This method of reaction he believes to have lain hidden in man, as a potentiality which is scarcely ever realised; as a living embodiment of an earlier phase in history. In the history of *the* man or of Man? Dr Rivers inclines to the latter view.

> In this case, and in the case of protopathic sensibility, we are not dealing with the suppression of individual experience, but with the suppression in the race of experience belonging to the earlier phases of its history. Through a special experimental procedure, or through the accidents of war, it has been possible to follow the suppression of this experience in the individual. The fact that this is possible suggests that the racial suppression is repeated in every individual as part of the recapitulation of the racial history. If this be so, however, the suppression takes place at so early an age that its detection is impossible. It would never have been suspected if the experiment and the clinical observations of Head had not pointed the way thereto.

Suppression, then, is exhibited as a process which may take place on the sensori-motor, the reflex or the highest levels, and the 'becoming-unconscious' of experience is merely a special variety of the process of inhibition, common to every phase of animal activity. But the relation of lower to higher performance in the nervous system is carried still farther. Just as on the sensori-motor level certain elements of protopathic experience are utilised by the process of fusion while others are suppressed, so, it is suggested, two kinds of elements in the unconscious may be distinguished:

> Corresponding with this distinction two kinds of elements in the unconscious might be recognised—those which have only disappeared from consciousness in their original form, but continue to exist in the different form they have assumed through the process of fusion, and those whose disappearance has been more complete so that they do not enter into consciousness even in an altered form under normal conditions, though their continued existence is shown by their reappearance under peculiar conditions such as those which accompany the regeneration of a divided and reunited nerve.

To the second class only, to the dwellers in the mind's Siberia, does Dr Rivers propose to give the name unconscious.

Here indeed is food for thought, but the food is so novel that one scarcely knows how to attack it. Let us begin with what is, to our taste, its most attractive part.

The British Psychological Society's symposium, in 1914, on *The rôle of repression in forgetting*[1] left in the mind of the present reviewer two vivid impressions. The first was that no scientific progress would be made in defining further the concepts underlying Freud's incalculably valuable practical hypothesis of repression until it ceased to be regarded merely as something morbid, reprehensible, unwise, shortsighted, and began to be approached from the positive standpoint, the standpoint of psychology, in short. The other was a growing appreciation of the futility of attempting to explain forgetting by invoking pseudo-physiological conceptions which were neither invented nor sanctioned by physiologists. When we understand the mechanism of a 'neurogram' even half as well as we now understand that of a telegram or a phonogram it may be time to lean upon such a conception, but not before.

Now Dr Rivers clearly presents suppression and repression as processes which are necessary to the efficient working of the mind. In certain circumstances they may lead to unfortunate results for any particular individual, but this fact does not constitute an insoluble biological problem, rather the reverse; for breakdowns often reveal more clearly the nature of a mechanism. To them he adds the conception of fusion, of elements integrated into men's present experience, perhaps of

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[1] This *Journal*, 1914, VII. 139–165.

> ...stepping stones
> Of their dead selves...,

but, by their harmonious combination, distinguished from those elements which, being neither stepping-stones nor dead, appropriately enough form the most prominent material for the psychological discussions of this troublous twentieth century. There is, however, one question which still remains unanswered: what kinds of experience become fused? If we suppose that the compatible, the dull, the inoffensive in our past subside into a compact, undifferentiated mass; that the numbers of our tram-car tickets, or of our hotel rooms, the details of our everyday meals, of our casual conversation, fuse into a drab solid background, and are thenceforth inextricable as individual experiences; how do we account for the surprising way in which we occasionally encounter them years later in dreams, or for the hypnotist's power of dragging up and saving from oblivion the minute details of a recent experience?

Dr Rivers may reply, and probably justifiably, that this is evidence that such recallable material was *not* fused, and that for every item so dredged up there are thousands which lie for ever at the bottom; that the fact that any part of our past appears in a dream is strongly suggestive that it is not of a fusible character. But it all takes us farther and farther away from the calm pages of Ebbinghaus, and inclines us regretfully to search for some other name for the 'curve of forgetting.' Yet it must not be forgotten that these illuminating suggestions are themselves based upon analogies with physiological findings. And those findings themselves must, of course, be distinguished in thought from the interpretation placed upon them by physiologists. To discuss the validity of these latter interpretations is beyond the powers of the present reviewer. But the particular part of Dr Rivers's speculations which seems to require most careful thought—all the more careful because of its inherent fascination—is the suggestion that the result of any injury to a formerly intact nervous system is or represents a past phase in the development—phylogenetic, ontogenetic or both—of that system. Dr C. S. Myers has pointed out elsewhere[1] that if owing to some nervous lesion a man develops a new gait, we cannot assume that this gait represents a more primitive form of progression. So, it may be argued, the appearance of an unusual mental state when a breakdown of higher control occurs cannot be regarded as proof that this state represents a primitive phase in man's ancestry.

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[1] *Discovery*, November, 1920, 339.

A view of Instinct is now proposed which is compatible with the beliefs already set forth. An attempt is made to distinguish instinctive from intelligent behaviour by "something in the nature of the behaviour itself." The instincts are divided into two classes. The first, or protopathic, are distinguished by "the absence of exactness and discrimination of appreciation, and of graduation of response," "the character of reacting to conditions with all the energy available," and "the immediate and uncontrolled character of the response"; the second, or epicritic, by their freedom from this 'all-or-none' principle and their capacity for graduation of response. It is impossible here to give any account of the evidence adduced for this division, and the reader will be reminded of a recent discussion of this part of the theory[1].

A central feature of the succeeding chapters of the book is now introduced in the discussion of what are termed the 'danger-instincts.' Five chief forms of reaction to danger are distinguished; flight, aggression, manipulative activity, immobility and collapse with trembling. An attractive explanation of the last-named biological puzzle is offered; in it is the penalty which Man has to pay for the pliancy of his danger-instincts, for their failure to become systematized or fixed in any one direction.

It is the place assigned to the undoubted and undoubtedly important reaction of manipulative activity which, however, seems difficult to understand. That it is classed, and not inadvertently, with the instinctive reactions seems clear (see p. 56, "these five main forms of instinctive reaction to danger"). Yet the paramount fact about this happy method of meeting danger seems to be that, in civilised man at least, it is not instinctive, but in most cases involves the possession of some highly artificial skill; often, too, of some intricate machine through which such skill may operate. Dr Rivers himself cites two ultra-civilised examples, those of driving a motor car and piloting an aeroplane. And this raises the important question whether in such very un-instinctive reactions the undoubted fact that there is often no affect can be taken as evidence that the affect has been suppressed. Is it not probable that in many cases there is no realisation, conscious or unconscious, of the danger? The performance in traffic of the average youthful rider of a motor-cycle does not strike one as an example of suppressed affect, though it may become one if he has recently had a bad smash. To point out the importance of manipulative activity in the presence of danger is timely and significant, but we believe that it has crept into the wrong part of this book.

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[1] This *Journal*, 1919, x. 1–42.

One puzzling phenomenon of human behaviour—'the faint'—might profitably have been considered in this connexion, for in many ways it seems to be relevant here. Is fainting an example of the reaction of immobility; and, if so, what are its functions? Can we yet explain the tendency possessed by some persons to faint at threatened pain, and at the sight or the sound of the sufferings of others? It is certain that in the second case the individual often sympathetically feels the other's pains, but why does he faint? Or again why faint at the sight of blood, and why, in some cases, perversely enough as it seems, at the sight of other people's, rather than one's own?

A new concept of dissociation is proposed, that of "a process and state in which suppressed experience acquires an independent activity," which "carries with it independent consciousness." It follows therefore that suppression often exists without anything which can be regarded as dissociation. This proposal, however, appears to raise the necessity of deciding, in any particular case, whether we are justified in assuming the presence or absence of independent consciousness, a task which unfortunately seems to imply all those very real difficulties connected with the criteria of consciousness which have driven some psychologists prematurely to the extreme left wing of behaviourism.

This difficulty reappears in the chapter dealing with the 'complex.' The conception of the complex, described by Bernard Hart in the first popular English exposition of Freud's and Jung's ideas, *The Psychology of Insanity*, is rejected as "almost identical with the 'sentiment' of the orthodox psychologist." The differences between a sentiment and a complex, according to Dr Rivers, are first, that the sentiment is *more* complex than the 'complex,' the characteristic features of which are its relation to the unconscious and its affective importance; secondly, that the sentiment is a far more epicritic product than the complex; sentiments "take part in the most finely graduated processes, and are connected with discrimination of the most delicate description."

This distinction cannot be dealt with in a sentence or even in a paragraph, for it is the view which it expresses, rather than the suggested definition, which offers itself for criticism. Dr Rivers's opinion is that "nothing but confusion can result from the inclusion in one category of definitely pathological processes and such absolutely normal and necessary processes as the sentiments." But might it not be added that it is just these practically useful distinctions between health and disease, which obscure important theoretical issues by suggesting hard divisions where none actually exist? The terms 'clean,' 'honest' and 'sober' may be indispensable when one is writing a testimonial, but they are merely challenges for thought, labels for problems, to the student of hygiene, ethics or physiology. Sane and insane, psychoneurosis and psychosis, *mens sana in corpore sano*—how do they help us except as blunt practical distinctions?

So what one man considers to be his sentiments another may term his complexes. And do not some sentiments, *e.g.* those of love for a person of the opposite sex, or for one's native land, often suggest, by their relative absence of delicate discrimination and of graduation, perhaps even by the repression which their growth necessitates, the characteristics of the 'complex'?

It is certain that many other reviewers will prefer to deal more fully with the latter part of the book: that which aims at providing a foundation for a biological theory of the psychoneuroses[1], and for this and other reasons the present essay attempts to treat the earlier chapters in detail. The view put forward is that mental health depends upon the presence of a state of equilibrium between instinctive tendencies and the forces by which they are controlled. The psychoneuroses arise as attempts, successful or unsuccessful, to restore the balance between the instinctive and the controlling forces.

The relative simplicity of the war-neuroses is due to their origin in disturbance of the relatively simple instinct of self-preservation, while the great majority of the neuroses of civil practice depend on the failure of balance between the less simple sexual instinct and the very complex social forces by which this instinct is normally controlled.

The two great divisions of the psychoneuroses: the hysterias and the anxiety-neuroses, with their different symptoms and their characteristically different affective colourings (an important aspect, well brought out) are re-named 'Substitution Neurosis' and 'Repression Neurosis' respectively. They are explained along the lines for which the earlier writings of Dr Rivers have prepared the reader. His theory of the former seems more likely to arouse dissentients than that of the repression neurosis. It is that hysterical symptoms are modified forms of the instinct of immobility, and that hysteria results as a product of the two processes of suppression and suggestion; the latter term having been given a somewhat special meaning. But he believes that two forms of hysteria may be distinguished, differing in their aetiology; and thereby supplies a subject complex and many-sided enough for a whole international congress.

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[1] See, for example, the review by Dr T. W. Mitchell; This *Journal*, Medical Section, 1921, I. 165–173.

Features important to a complete understanding of the book, which unfortunately cannot be treated here for lack of space, are the conceptions of Suggestion, Intuition, and the treatment of hypnosis and sleep. The appendices, especially those entitled "A Case of Claustrophobia" and "Freud's Conception of the 'Censorship,'" are so full of matter that no just account which is at the same time brief, is possible.

Perhaps Dr Rivers would agree if one ventured to call this novel book the record of an impression. The present review is no more than an attempt to write down an impression of an impression.

PUBLICATIONS RECENTLY RECEIVED.

The Religious Consciousness: a psychological study. By Professor JAMES BISSET PRATT. New York: The Macmillan Company. 1920. Pp. 486. $4.00.

In many ways this is the best work that has yet appeared on the psychology of religion. The author has succeeded in describing the religious consciousness from the point of view of an unprejudiced observer who has no thesis to prove. At the same time, he has that sympathetic insight into religion which is necessary even for purely descriptive work.

The two chapters on conversion are new and extraordinarily interesting. There is a careful study of the purely emotional conversion, of which Bunyan and Brainerd are examples, which became the conventional religious crisis of some forms of protestantism. The last five chapters are on mysticism, and give an account of that difficult subject which is not unsympathetic to the mystic, while it allows due weight to the point of view of mental pathology.

Prof. Pratt has written a very valuable work which, however, does not claim to be a complete treatment of its subject. Probably the chapter on "Religion and the Subconscious" does not attach sufficient weight to the importance of the work of Freud and his followers. It is, however, a book which no student of the subject can afford to neglect.

Movement and Mental Imagery. By Professor MARGARET FLOY WASHBURN. Boston and New York: Houghton Mifflin Co. 1920. Pp. xv + 252. $1.75 net.

Acting on the belief that "in psychology especially one may be permitted to push theory ahead of fact" the author puts forward in this book the sketch of a motor theory of mental process, attempting to show how the whole of the inner life is correlated with and dependent upon bodily movement. Taking as a working hypothesis the idea that all consciousness is related to movement, that it, in fact, accompanies "a certain ratio of excitation to inhibition in a motor discharge," the essential nature of the different mental processes is reviewed in the light of this theory. So, the checking of the normal motor response to any external stimulus by the influence of some antagonistic motor response gives the conscious experience known as sensation. Attention is the actual performance of movements. Movements lie at the root of all associations..."associative dispositions consist of lowered resistances between the kinaesthetic pathway involved in the performance of one movement and the motor pathway involved in the performance of another."

But most interesting, perhaps, is the way in which the theory is used to explain the nature of images..."whenever a motor pathway is at the same time excited by a sensory pathway and partially inhibited by an antagonistic motor excitation a process occurs in all sensory pathways connected with the motor pathway by low-synaptic resistances, including the sensory pathway that is exciting the motor pathway in question. This process is accompanied by consciousness....When it occurs in a sensory pathway that is being excited by an outside stimulus it gives rise to the type of consciousness that we call a peripherally excited sensation. When it occurs in a sensory pathway that has no outside excitation it gives rise to the type of consciousness that we call a centrally excited sensation or mental image."

Images, too, then, are nothing more than inhibited movements. Trains of images are successive movement systems carried on by the operation of motor responses which, though slight, are none the less actual. Differences of imagery are due to differences of attention, that is, to differences in motor response to stimuli. Kinaesthetic 'images'—marked off as peculiar in type by many investigators—are not images at all but sensations.

The laws of memory and the nature of purposive thought are also brought under review—while finally an explanation is offered of those 'imageless' processes so often claimed as the only conscious accompaniment of thought. These processes, it is suggested, are at root like all others kinaesthetic in nature, but since kinaesthetic processes need not be localised many of them are never fully cognised as sensations, but are experienced, as for example, in rapid thought, in forms to which no definite name can be applied.

The whole book is a skilful and suggestive exposition of an interesting theory as to the true nature of mental activity.

Das Problem der Gültigkeit in der Philosophie David Humes: ein kritischer Beitrag zur Geschichte der Erkenntnistheorie. Von Dr HEINRICH HASSE. München: Ernst Reinhardt. 1920. Pp. 194.

Kant's acknowledgment that it was Hume who had roused him from his dogmatic slumber, has not been altogether to Hume's advantage. Hegel started the view, adopted by most writers, that Hume's whole significance in the history of philosophy lay in this service of awakening Kant. Dr Hasse holds that Hume's philosophy has an intrinsic interest of its own, and deserves an independent study without reference to Kant. In the volume before us Dr Hasse deals fully with the problem of the validity of human knowledge, which he regards as the central theme round which all the philosophical questions discussed by Hume really revolve. Dr Hasse contends that Hume did not clearly distinguish between psychological and logical questions, constantly confusing the problem of the genesis of ideas with that of their validity. Hume's basic principle, that all ideas must be traced back to impressions, was narrow and ambiguous. Nevertheless, Hume himself unwittingly corrected some of his initial errors, and so much may be learned from his wrestlings with epistemological problems.

The Problem of the Nervous Child. By ELIDA EVANS. With an Introduction by C. G. JUNG. London: Kegan Paul. 1920. Pp. ix + 299. 12*s.* 6*d.* net.

This book, by a pupil of Dr Jung, is primarily intended for parents and teachers, and deals with many important educational problems. It contains much that is sound, more that is questionable and a little that is far-fetched. The statement that "the experienced psycho-analyst can clearly see during the narration of the first dream the exact cause of the trouble," savours somewhat of 'wild' psycho-analysis, and it certainly seems amazing to expect any ordinary man to be satisfied with the explanation given of the patient, who "all his later life had the feeling of being unloved" because he was neglected and laid aside for a few hours after birth. A whole chapter is devoted, though not convincingly, to 'muscle erotism,' and the case quoted at length is not taken far enough to enable a diagnosis of 'moral imbecility' to be excluded. Apart from the appearance of many loose sentences, such as "at the age of puberty she has literally and physically lost her boy," and some mistakes such as the interchange of the seventh and eighth commandments, the book is clearly and simply written; but the difficulty in discerning the wheat from the chaff makes it very doubtful whether it will be of any value to those for whom it is intended.

The Aesthetic Attitude. By Dr HERBERT SIDNEY LANGFELD. New York: Harcourt, Brace & Howe. 1920. Pp. xi + 287.

The book contains, apart from the introductory discussion of the range and nature of Aesthetics, an attempt to describe the nature of the aesthetic appreciation from, what the author would call the "psychological aspect of Beauty." The substance falls roughly into five sections, viz. an account of the Aesthetic Attitude, Empathy, Unity and Imagination, Balance and Proportion and a concluding discussion on the Art Impulse. Several of these sections are illustrated by applications of the points discussed to the Fine Arts, exemplified by photographs from well-known works of art.

The best that can be said for the book is that it contains a more or less useful summary of work, both speculative and theoretical, done in Aesthetics sometime before the War. But its utility is very relative, for in point of fact very little use has been made of the material accumulated or published in any language but English. It is true that reference is made to two or three French authors like Guyau, none of them modern, and that some German writers like Lipps or Vischer, Lotze or K. Gross, are quoted; but of recent research little is noticed and none utilised. Modern French Aesthetics seems, as far as the author is concerned, not to exist—a feature not peculiar to this author;—so too with Italian aesthetic work, criticism and theory, with the solitary exception of a passing reference to 'Professor Croce.' More particularly in a subject like Aesthetics which deals with experiences and responses, not merely of a generally human, but of a specifically cultural character which is historically and otherwise perfectly well-known, such a limitation becomes a definite blemish. It suggests not merely an ignorance of the actual conditions which inspired a work or conditioned its appreciation, but lack of cultural balance, nowhere more harmful than in a subject such as Aesthetics.

The writer's treatment of the subject is curiously lacking in criticism. Apart from apparent misunderstandings such as that of Bullough's meaning of the term 'Distance' as "the amount of appeal" (p. 72) which leads for the next 30 pages to a continual misapplication of the conception, the prominence given to the Empathy doctrines in their various formulations suggests that the author is unaware of the criticisms directed against that theory ever since it passed its zenith of popularity after about 1905. Neither the general trend of aesthetic theory nor Croce's incisive teaching on that point—going back in Italian aesthetic criticism to the 18th century—has here prevented the reappearance of the old discussion of the relations of 'Form' and 'Content' which we had believed to be safely dead. There is no relation between the various topics he chooses to discuss in the different sections of his book.

The chapter on "Unity and Imagination" roused hopes of some really helpful contribution to the burning problem of artistic Imagination, merely to offer a disquisition of attention which belongs to general Psychology, ending in a purely functionalist theory of the worst type which differs in no way from the old eye-movement doctrine, disproved, as the author points out himself (p. 125) by Stratton's admirable experiments. Nor does the exposition of the Art Impulse add anything to that well-thrashed straw, the Play theory, or to the excellent summary of anthropological material of Y. Hirn.

The Child Vision. By Dorothy Tudor Owen (Mrs D. Truman). Manchester: The University Press, and London: Longmans, Green & Co. 1920. 6*s*. 6*d*. net.

The title of this book indicates far less and far more than its subject, which is a very clear exposition of a method of teaching composition in schools. The difficulty experienced by the average child, as Professor Archer expresses it in his preface, is that "when he is called upon to say something he has nothing to say," and therefore he is compelled "at one and the same time to go through two processes either of which is sufficient to tax his full powers, that of finding something to say and that of thinking how to say it." Mrs Truman's method is to take the visual images in the child's mind as the material for him to express in language which will convey his meaning to his classmates. They, in their turn, draw the picture from the description, and thus afford material for comment of two kinds: criticism by the original describer, of the various drawings, and criticism of his description by the artists. In this way the essential rules for good composition—arrangement, proportion, exactitude, vividness, detail and the like, are strikingly illustrated.

The book is well written, contains many convincing illustrations of the value of the method, and is full of suggestions which cannot fail to interest anyone, teacher or student, who is concerned with the difficult practical problem of thought-expression.

1. *A Classification of Association Categories and their Use in testing General Intelligence.* 2. *A Test for the Quick Grading of General Intelligence by Groups.* By M. Dainow and E. R. Mason-Thompson. The Pelman Laboratory of Applied Psychology. N.D. Pp. 23. 5*s*. net.

In the first of these two essays, the authors classify associations under twelve classes. They then attempt to grade these classes in order of intellectual priority on the basis of their experience with 50 subjects. From these subjects they finally selected 15, who were graded in general intelligence by three persons who knew them well; and after applying a suitable weighting principle to the different classes of their associations, the authors come to the conclusion that "the association reaction type can be used as a good index of general intelligence." The test recommended in the second paper for the "quick grading of general intelligence" consists in reading aloud as rapidly as possible, so as to make sense of, a paragraph of 183 words in which the letters of each word are printed in reversed order, *e.g.* 'dna' for 'and.' The subject is given the key, if at the end of 1½ minutes he has not arrived at it, and he is then allowed to proceed. The writers claim that this test serves to divide subjects very sharply into broad grades of intelligence.

Instinct in Man. By Dr JAMES DREVER. Second edition. Cambridge: The University Press. 1921. Pp. x + 293. 10*s*. 6*d*. net.

The appearance of the second edition of this excellent book proves that its outstanding merits have been widely recognised. "The only significant difference between this and the first edition is the new appendix on 'The Emotional Phase of Affective Experience'" which "gives the gist of a paper read before a general meeting of the British Psychological Society on the 19th of June, 1920." The main object of this interesting essay is to show "that instinct feeling as such is not necessarily emotional," and that "we should speak of 'instinct feeling' where McDougall uses 'emotion' to designate the affective element in instinct experience."

NOTES ON RECENT PERIODICALS.

L'Année Psychologique (21st year) 1920.

With this volume *L'Année Psychologique* resumes publication for the first time since 1914. The first place among the original papers is occupied by a long study of *The Problem of Expectancy* (*Attente*) by Mlle Moraud, who compares the introspections of her subjects during the period intervening between the warning of an imminent signal and the perception of the signal itself, with graphical records of muscular tone and movement. She concludes that the state of expectancy is extremely complex in any given individual and varies greatly among different individuals according to whether their modes of thought are rich or poor in imagery.

M. Bourdon reports some experiments on the *Localisation of Sound*; he finds, among other results, that differences in direction are most easily detected in the case of sounds moving in a horizontal plane. The perception of 'up' and 'down' differences is particularly defective.

M. Foucault examines the *Persistence of Practice Effects* exemplified by the Kräpelin addition text. He concludes that:

(i) Capacities acquired by practice become weaker from inaction and this weakening, measured by the increased time necessary for performing the work, appears to be a function (not yet determined) of the period of inaction. The decline may be as much as 25 per cent. in a year.

(ii) The capacity thus weakened regains its former level after a very brief period of renewed practice; if the practice be continued the capacity may be further improved to an extent of some 10 or 15 per cent.

(iii) The resistance to the deleterious effects of inaction, the speed of recovery to the former level and the possibility of further improvement are dependent on the period and, probably, also on the method of practice.

M. Piéron, the Editor, contributes an account of researches on the *Memory of Forms* (consisting of groups of lines) and of tables of figures. He finds that the same laws as to the rate of forgetting, etc. hold good in each case. He attributes the effects in each case to "physiological laws affecting the formation and effacement of memory traces; traces consisting of a simple wearing of associative paths."

The same writer, in the section devoted to Notes and Reviews, gives a valuable summary of the work which was done by various nations during the war on psychological tests for aviators.

Drs Mignard and Gilles contribute an essay on the *Psychological Aspects of Psychoneuroses* based on observations at the Psychiatrical Centre of the Sixth Army.

A summary of work on this subject (mainly French) is also given by M. Wallon.

Mlle Vlaïcou studies the relations existing between the span of apprehension (or 'immediate memory'—a term which she rejects), the speed of learning and the power of retention of letters, figures and nonsense syllables.

She finds a positive correlation between the first two variants, none between the first and third, and a negative correlation between the second and third. There is a positive correlation between letters and figures, between figures and syllables and, especially, between syllables and letters. She also investigates sex differences with regard to these capacities and the effects of age upon them.

Mlle Grzegorzewska contributes a paper on *Types of Aesthetic Ideation.* She discriminates the descriptive, sensory, critical, interpretative, and emotional types and finds that of these the intellectual varieties are, in general, characteristic of men, the emotional of women.

Abstracts of about 600 psychological books in French, English, Italian and German are also given.

Archives de Psychologie, Vol. XVII.

No. 67, April 1919.

Instinct, Émotion et Sentiment. (J. Larguier des Bancels.)

Part 1 considers and accepts W. James's theories of instinct. The author then goes further and considers the instinctive reactions to Fear and Anger.

Part 2 deals with Emotions in relation to instinct. Emotions are regarded as substitutes for instincts in some cases of emergency and in the case of the psychoneuroses.

Part 3 deals with Sentiments in relation to emotions. All excessive sensations are painful. Sentiments are believed to be subjective and in no way dependent on organic reactions as are emotions.

(1) *Symbolismes en Psychopathologie.*

(2) *Quelques remarques sur le Symbolisme dans l'Hystérie.* (H. Flournoy.)

(1) Examples of dreams, hallucinations, illusions of pathological patients are cited and an explanation of the symbols used given, showing how a suppressed experience comes to the surface, often modified by the censor. Cures may be effected by means of psycho-analysis and free association.

(2) An account is given of cases of Hysteria, with the methods of curing them. Suggestion is largely used. Symbolic motions are considered as arising from some kind of suggestion. Many observations are needed to ascertain what a given symbol represents. There is a general discussion on the meaning, use and interpretation of such symbols.

Réflexions sur la Classification et l'Unification des Sciences. (Ch.-Eug. Guye.)

The bearing of the theory of relativity on our present conception of Time and Length is discussed. The sciences are then classified according as they deal with the increasingly complex conception of Numbers, Space, Time, Material, Life and Thought. A discussion follows of the connecting link provided by the Theory of Relativity between the material sciences and Psychology, maintaining that this theory has supplied the missing link necessary to connect all sciences together.

No. 68, December 1919.

Des Réactions Organiques accompagnant les États Psychologiques. (Lucien Cellerier.)

The bodily reactions to sensations, both of pleasure and pain, are the same. Variations of respiration, rate and amplitude of heart-beat, blood-pressure, and the volume of the limbs and brain are considered, and extensive experimental data given. Conclusion is that the change in bodily state is due to change in affect quite independent of the nature of the affect.

A Propos d'un Disciple d'Unternährer. (Raymond de Saussure.)

An account is given of the life and history of Unternährer and his religious sect, describing the sexual nature of his doctrines. The explanation of the fact of the existence and continuance of this sect is due to the inability of certain abnormal individuals to suppress their sexual tendencies. The study of the sect and its doctrines is of especial interest and value to the Freudian school.

Recherches sur la Corrélation entre la Mémoire des Mots et la Mémoire des Images. (Yvonne Delhorbe.)

An account of work done to see if children with a good memory for words have also a good memory for images. A correlation of ·42 was obtained, most children having the best memory for images.

(1) *Percentilage de quelques Tests d'Aptitude.*

(2) *De la Constance des Subjects à l'égard des Tests d'Aptitude.* (Ed. Claparède.)

(1) Account of four types of tests given to school children: 1. Memory of 15 words. 2. Permutations of four letters. 3. Rapidity of writing. 4. Arithmetic calculation. The value of the results is discussed showing that the median of results should be taken as representing the group most truly. The memory test shows natural aptitude, while the other three tests show intelligence. The mental age of the subject cannot confidently be estimated from these tests.

(2) Memory tests were given four and six times to find how constant various subjects were in their reactions. A definite constancy in the capacity of the individuals was observed, but the number of observations necessary for the accurate classification of a group of people is not decided, though four or six is suggested.

Brain, Vol. XLIII.

Part I, May 1920.

The Results of Secondary Suture of Peripheral Nerves. (J. S. B. Stopford.)

Microgyria and its Effects on other Parts of the Central Nervous System. (W. Page May.)

A Psychological Interpretation of Essential Epilepsy. (L. Pierce Clark.)

A study of the behaviour of potential epileptic individuals. Essential Epilepsy is dependent upon a primary congenital defect of natural adaptations to reality. Psychologically considered, the fit is a protective mechanism, reducing the subject's attachment to reality.

The Psychogalvanic Reflex: A Review. (E. Prideaux.)

An account of the observations and conflicting opinions of many observers, showing the need for further investigation.

Forms of Peripheral Neuritis among Troops serving with the Egyptian Expeditionary Force, 1915–19. (F. M. R. Walshe.)

Part II, July 1920.

Aphasia and Kindred Disorders of Speech. (Henry Head.)

An account of results based on the investigation of young men suffering from the effects of gunshot wounds in the head. Chap. I: Serial tests employed. Chap. II: Criticism of previous explanations of Aphasia. Chap. III: Functions affected in Aphasia, etc. grouped as "Symbolic thinking and expression," because the most definite disturbance is to be found in the use of words, figures and other symbols. Chap. IV: Various dissociated forms comprised under four heads: (*a*) Verbal defects; (*b*) Nominal defects; (*c*) Syntactical defects; (*d*) Semantic defects. Chap. V: "Symbolic thinking and expression." A full summary of this investigation has been given by Dr Head in this *Journal*, 1921, XI, 179–193.

Congenital Familial Spinal Muscular Atrophies and their Relation to Amyotonia Congenita. (Knud H. Krabbe.)

Part III, November 1920.

On the Symptom-Complexes of Lethargic Encephalitis with Special Reference to Involuntary Muscular Contractions. (F. M. R. Walshe.)

On Decerebrate Rigidity in Man and the Occurrence of Tonic Fits. (S. A. Kinnier Wilson.)

Relation of the Form of the Knee-Jerk and Patellar Clonus to Muscle Tonus. (Henry Viets.)

Alterations of the Golgi Apparatus in Nerve Cells. (Wilder G. Penfield.)

Journal of Educational Psychology, Vol. XI.

No. 1, January 1920.

The Translation Method of Teaching Latin. (W. H. Fletcher.)

Translation of Latin into English constitutes the greater part of each exercise. Everything new is taught in its functional relation within a sentence composed of material otherwise known. An outline of a typical course of lessons is given.

The Psychology of Riddle Solution: An Experiment in Purposive Thinking. (Thomas Russell Garth.)

Riddle-solution shows the use of the method of trial and error in the solution of problems. The best results were obtained by those who showed neither paucity nor redundancy of suggestions.

An Inquiry into the Statistical Basis of a Conclusion Concerning Sex Differences. (George W. Frasier.)

The conclusion put forward by Yerkes, Bridges and Hardwick in *A Point Scale for Measuring Mental Ability* is criticized on the ground that it is based on the modal value, which varies with the size and position of the class-interval.

Communications and Discussions: Age-Grade Distribution and Intelligence Quotient. (Allen J. Williams.)

The increase of 'overageness' up to the fourth grade is explained by the fact that 2 per cent. of all children cannot do more advanced work. The over-age group should be balanced by an under-age group.

No. 2, February 1920.

The Administration of the Psychology Prerequisite to Courses in Education. (Clifford Woody.)

Data gained by a questionnaire show that the current practice is for courses in education and psychology to be controlled in independent departments. The department of education usually determines its own prerequisites. The need for the definite standardisation of the prerequisites is emphasised.

The Use of Tests in the Evaluation of Methods of Instruction. (Buford Johnson.)

The results of the application of standardised tests to a number of children are given. The most effective use of the tests is the determination of methods of instruction to meet individual needs.

A Brief Group Scale of Intelligence for Use in School Surveys. (Sidney L. Pressey.)

A short scale consisting of four tests dealing with verbal ingenuity, logical judgment, arithmetical ingenuity, and moral judgment, is described. It can be used both for comparison between schools and for individual diagnosis.

The Use of Psychological and Trade Tests in a Scheme for the Vocational Training of Disabled Men. (Ralph S. Roberts.)

Vocational training must be based on psychology. Knowledge of the man to be trained must be gained by intelligence tests, trade tests and experts interviewing. Cooperation with manufacturers and trade unions is necessary.

Communications and Discussions: Some Results of Monroe's Diagnostic Tests in Arithmetic. (Julie E. Wertheimer.)

The results of seven tests showed weak work in the fundamental operations. The results were made the basis of improvements in teaching methods and a re-application of the tests showed an improvement of 100 per cent.

No. 3, March 1920.

Measurement of Ability in Latin. Part III. Vocabulary and Sentence Tests. (V. A. C. Henmon.)

In two previous papers five vocabulary tests and three sentence tests were put forward. In this paper scale values for eighty additional words and 60 additional sentences are given.

The Rational Learning Test Applied to Eighty-One College Students. (Joseph Peterson.)

The test attempts to measure quantitatively the effectiveness of a continued reasoning process. The results of the application of the test to college students are given and correlated with the standing of the students in psychology. Decrease of errors in the test seemed to mean increase in scores in psychology, but the converse was not so generally true.

The Effect of Practice on Two Mental Tests. (J. F. Dashiell.)

The two tests are the 'mental formation' test and the 'free association' test. The former shows no improvement with practice: the latter shows definite effects of practice in the case of boys, while for adults the results are equivocal.

The Adolescent Will-Profile. (June E. Downey.)

A series of tests designed to throw light on a number of character traits is described. The adolescent tends to excel in the tests revealing force of reaction and in those showing ease and fluency of reaction, and to obtain a low score in those requiring precision and carefulness of reaction. Illustrations of individual differences in will-profile are given.

Communications and Discussions: Classification of Junior High School Pupils by the Otis Scale. (W. D. Armentrout.)

An account of the Otis classification of the pupils of a particular school.

Correlation of General Intelligence Tests and School Standing. (E. B. Skaggs.)

A very low correlation was found. The need for a complete summary of previous work on this subject is emphasized.

No. 4, April 1920.

Mental Tests as a Means of Selecting and Classifying College Students. (Agnes L. Rogers.)

The correlation between the standing in the tests and the academic standing was small, but the tests might form a useful supplementation of existing methods of selecting students if the period of testing were lengthened, and more fundamental capacities measured.

Students' Methods of Studying a Certain Subject—Psychology. (L. W. Webb.)

A questionnaire showed that the general tendency of most students is to use poor methods of study. Good students were found to possess better study-habits than poor students. The need for instruction in methods of work is emphasized.

An Educational Survey Test. (Rudolf Pintner and Florence Fitzgerald.)

A short Educational Survey Test is given in detail. This test shows a good correlation with the results of the long scales from which it is derived and represents an economy in time, labour and cost.

Results of the Bell Chemistry Test. (Thomas H. Briggs.)

The test was given to 38 boys and the results show that while the test is a fairly good measure of a class knowledge of chemistry it is unreliable as a measure of the individual. A correction sheet for the test is given.

No. 5, May–June 1920.

Graded Opposites and Analogies Tests. (M. J. Van Wagenen.)

An account is given of the construction of a series of analogies tests and a series of opposites tests of approximately equal difficulty. The tests with their adequate responses are given in detail.

A Study of Twenty High School Seniors of Superior Intelligence. (Dorothy Hazeltine Yates.)

A study of twenty pupils selected by means of intelligence tests showed that the superior pupils came from homes suitable to development and were generally precocious physically as well as mentally. Their interests were more intellectual and their health not below normal.

A Test of Some Standard Tests. (Grace E. Bird.)

The correlations found to obtain between certain standard tests and general ability are given. The tests involving the most language ability were the most reliable indices of general school ability while those which were the least interesting in form to the pupil were of least diagnostic value. The tests need to be more easily administered and scored.

The Reliability and Significance of Tests of Intelligence. (Edward L. Thorndike.)

An examination of twenty to fifty minutes is adequate to make only coarse distractions among individuals. Intelligence as measured by verbal tests is very different from intelligence as measured by non-verbal tests.

Psychological Monographs, Vol. XXVIII.

No. 123. *Quantitative Aspects of the Evolution of Concepts.* (Clark L. Hull.)

Learning of "isolated concepts" gave as good results as the evolution of concepts from concrete instances. The evolution is only about one-sixth as rapid for abnormal as for normal subjects. Moderate familiarity with a large number of cases is more economical than complete familiarity with half as many cases. Trial and error plays an important part in the evolution of concepts.

No. 124. *An Experimental Analysis of a Case of Trial and Error Learning in the Human Subject.* (George S. Snoddy.)

A detailed account of the process of learning mirror-tracing which is shown to be of the true trial-and-error type. In the early stages of the learning the period of rest is the important factor, and attention to accuracy gives the best results. In the later stages continuous practice is more efficient and secures progress in both speed and accuracy. The pleasure following a successful movement has no place in bringing about improvement. A physiological account of the rest period, based on the conception of the Multiple Final Path, is put forward.

No. 125. *Work with Knowledge of Results versus Work without Knowledge of Results.* (George F. Arps.)

Both the absolute amount of ergographic work, and the speed of work, were found to be greater when the subject was allowed to see the record during the work. In the later experiments concrete imagery of results appears in the 'unknown' series and takes the place of the perception of results in the 'known' series, and the difference between the results diminishes.

No. 126. *Individual Differences in Finger Reactions.* (Esther L. Gatewood.)

The speed and accuracy of the reactions of the fingers both singly and in combination are investigated. The fingers of the right hand react more quickly and more accurately than those of the left. Two-finger reactions are faster and more accurate than one-finger reactions. The most efficient combinations are those involving one finger of each hand. Practice increases the speed and accuracy of all fingers and tends to reduce the differences between them.

No. 127. *The Lag of Visual Sensation in its Relation to Wave-Length and Intensity of Light.* (Marion A. Bills.)

Four older methods of investigating the order of speed with which visual sensations reach their maximum are compared with three new methods. The new methods aim also at a comparison of the effects produced at different times before the maximum is reached. Close agreement was found between all the new methods and two of the old as regards the time required to reach the maximum, and between the new methods as regards the rate of the rise. The rate of rise differs for different wave-lengths and the order of the rate of rise varies with the intensities of the lights.

Journal of Experimental Psychology, Vol. III.

No. 1, February 1920.

Conditioned Emotional Reactions. (J. B. Watson and R. Rayner.)

Conditioned emotional reactions, once established, can, when the subject is in infancy, be transferred to other objects which have some characteristics in common;

further, they persist all through life, and emotional disturbances may be traced in adults to responses set up in infancy. All three of the fundamental human emotions, fear, rage and love, cause such responses, consequently Freud is wrong when he traces them only to love or sex.

The Organic Effects of Repeated Bodily Reaction. (C. R. Griffith.)

The organic effects of repeated bodily rotation are highly variable in their appearance and so amenable to practice that they may entirely disappear within a relatively short time, provided rotation is repeated from day to day.

A Series of Form Boards. (G. O. Ferguson, Jr.)

A Description of an Apparatus for Determining Acuity at Low Illuminations. (Ferree and Rand.)

For testing light and colour sense and for detecting small errors in refraction and in their correction.

Reaction-Time Symptoms of Deception. (W. M. Marsten.)

Two distinct types of reaction-time behaviour during deception:

(1) *Positive type* characterized by increased length of reaction-times and increase of mean variations.

(2) *Negative type* giving shortened reaction-times which appeared to have no consistent effect upon mean variation.

A third group of subjects shifted from positive to negative types on different days.

No. 2, April 1920.

An Experimental Study of Dizziness. (C. R. Griffith.)

The organic and mental effects of dizziness are readily affected, in their appearance and duration, by the direction of attention, psycho-physical determination, the character of the visual field, and the mode of ocular fixation.

Interdependence of Judgments within the Series for the Method of Constant Stimuli. (S. W. Fernberger.)

A contrast effect is present between the pairs of stimuli under the conditions specified. This causes a distortion of the curves for the psycho-metric functions for the 'lighter' and 'heavier' judgments, and increases with practice.

Discussion: Note on Mr G. J. Rich's Study of Tonal Attributes. (Henry J. Watt.)

Reply to Dr Watt's 'Note.' (Gilbert J. Rich.)

No. 3, June 1920.

The Influence of the Group upon Association and Thought. (F. H. Allport.)

The presence of a co-working group is favourable to the speed of Free Association. The group produces more ideas of thought but these are of a lower logical value than those produced in solitude.

Attributive v. Cognitive Clearness. (Carl M. Dallenbach.)

Britz's perceptual steps are verified, and extended. The end result of experiments on 'range of attention' is that they involve not 'attention' but 'cognition.'

Curves of Growth of Intelligence. (H. A. Toops and R. Pintner.)

These curves are based upon data derived from a specific set of tests, and though too much increase may be shown in the higher ages, they will serve to indicate the general trend of mental development for all degrees of intelligence at the various chronological ages.

No. 4, August 1920.

The Use of the Illumination Scale for the Detection of Small Errors in Refraction and in their Correction. (C. E. Ferree and Miss Rand.)

This method would seem to have its chief value in determining the exact amount of the correction in cases of high astigmatism, and both the amount and placement of correction in cases of low astigmatism.

The Backward Elimination of Errors in Mental Maze Learning. (Joseph Peterson.)

Two methods of studying maze-learning emerge from this paper: (1) an empirical method which enables the problem to be studied independently of spatial factors peculiar to individual mazes, and (2) a method of comparing the distribution of errors in the maze with the expected distribution based on chance.

Some Volitional Patterns Revealed by the Will-Profile. (Miss Downey.)

The outcome of this investigation indicates that the 'Will-Profile' can be used in determining the specific combination of traits of character of an individual in addition to his general temperamental pattern.

On the "*After-Sensation*" *of Pressure* (R. T. Holland) the following conclusions are arrived at:

A. If the observer is passive towards the experiment after-experiences are few and brief.

B. If he assumes an active attitude, they are numerous and of considerable duration.

C. When true after-sensations of pressure *do* occur, there is also no doubt that pressure-imagery plays a part in the formation of cutaneous after-experiences.

Psychological Bulletin, Vol. XVII.

No. 1, January 1920.

In analysing "the operation of the laws of chance in tests of the two-alternative type," S. C. Kohs finds that "opinion in the past has been inclined to exaggerate the frequency of zero scores, and to under-estimate or overlook the frequency with which significant positive scores might occur."

The following are responsible for reviewing 1918–20 publications under the accompanying subject-groups: (1) W. T. Mason, "Mind and Body,"—the Behaviourist and Dualist standpoints. (2) A. P. Weiss, "Consciousness and the Unconscious." (3) H. S. Langfield, "Textbooks and General Treatises."

C. E. Longshore describes new apparatus invented. H. C. Warren notes movements towards standardisation of terminology.

No. 2, February 1920.

This number reports the 28th Annual Meeting of the American Psychological Association, with abstracts of the fifty-seven papers read.

No. 3, March 1920.

Floyd Allport surveys the publications of 1918–20 in the realm of Social Psychology under three groups, viz.: (1) "social-mind" theories, (2) the social behaviour of the individual, and (3) social behaviour in relation to society. He considers that "in spite of the persistent speculative essays of the old school, the trend is encouragingly towards observation and experiment."

George A. Coe summarises works which have appeared over the same period on the Psychology of Religion, while the more prominent of these are reviewed by J. H. Leuba.

Under the heading "Crime and Social Psychology," Annie L. Kellog outlines some twenty recent articles on crime "which lay the emphasis very largely upon psychiatric problems."

No. 4, April 1920.

In reviewing the work of the past three years on "Cutaneous and Kinaesthetic Senses," John T. Metcalf draws special attention to Lashley's article on "The accuracy of movement in the absence of excitation from the moving organ."

In his survey entitled "Affective Phenomena, Descriptive and Theoretical," H. N. Gardiner outlines recent contributions to the study of the emotions.

Special reviews are devoted to Cannon's "Physiological Basis of Thirst" and Goddard's "Psychology of the Normal and Subnormal."

Controversial discussion is entered into between Ferree and Rand, authors of a paper on "The selectiveness of the achromatic response of the eye to wave-length," and their reviewer Dr L. T. Troland.

The Committee on Terminology specifies certain lines on which it invites the co-operation of psychologists.

No. 5, May 1920. Physiology number.

Edwin B. Holt gives a comprehensive survey of recent experimental work on the Nervous and Muscular Systems. He groups his subjects under the topics of (1) physiology of the autonomic nervous system, and (2) the working of the twofold neuro-muscular process.

F. W. Carpenter deals with some further papers falling under the first of these subject-groups such as: the effects of different kinds of emotional excitement on heart-beat and blood-pressure, on catalase content and the accumulation of hyperglycemia, of the anatomical and histological structure of the autonomic nervous system, of the gastric hunger-mechanism in fasting and fever, and of autonomic functions and the personality.

No. 6, June 1920. Comparative Psychology number.

Wallace Craig reviews the results of recent research into "Tropisms and Instinctive Activities."

In surveying the previous year's work on "The Sensory Physiology of Animals," K. S. Lashley finds that "the bulk of material deals with photic sensitivity" while "labyrinthine functions occupy second place."

John F. Shephard summarises recent papers on "Habit Formation and Higher Mental Capacities in Animals."

Special reviews are devoted to Ritter's "Unity of the Organism," and G. V. Anrep's "Pitch Discrimination in the Dog."

The Training School Bulletin, Vol. XVII.

Nos. 1 and 2, March–April 1920.

S. D. Porteus and Helen F. Hill outline a condensed guide to the Binet-Simon tests.

Part I. Tests and Diagnosis.

A just estimate of the value and limitations of these tests is now being approached. The importance of supplementary tests to counter-balance the 'over-literary-emphasis' in Binet verdicts is urged. Goddard and Stanford revisions are considered and recommendations made as regards: (1) percentile tabulation of results for statistical research purposes, (2) scale modifications for infants and adolescents, (3) modifications in diagnostic methods.

Part II. Tests and Test-Procedure.

A table is drawn up, supplementary to Terman's "Record Blank," with clear directions for giving each test, together with descriptions of material required and conditions of success; explanatory notes and commentary are appended.

No. 3, May 1920.

J. E. Wallin dealing with "The Concept of the Feeble-Minded" concludes that:

(1) Congenital (or early acquired) intellectual deficiency resulting in social and economic incompetency is the sole criterion of feeble-mindedness.

(2) The term 'moron' should be reserved for those persons whose intelligence level at maturity is below 9 years: even then its application is not universal.

(3) Those persons, whose intelligence level is above 9 years requiring custodial care on account of conditional or volitional instability, may be termed 'defective delinquents,' and should be accommodated apart from the feeble-minded.

No. 5, September 1920.

In Part I of a paper on "Congenital Word-Blindness," J. E. Wallin defines this defect as "inability to understand or interpret printed or written characteristics." Its incidence is probably much more general than hitherto supposed, and it appears to be the primary reason for some children's slow school-progress. The defect has been more often found in boys than girls, and its physiological basis is considered. It is due to the interpretation of defective visual word-imagery, not to defective apprehension or retention of ideas once grasped.

The work of the Vineland Research Department is outlined; problems within its general scope are: (1) the recognition, (2) the causation, and (3) the prevention of mental defect.

No. 6, October 1920.

Concluding his paper on "Congenital Word-Blindness" Wallin proves that "word-blind children are not deficient in visual and auditory imagery." The extent of the defect varies in individual degree, not in kind. Special classes are advocated, with more oral instruction and special reading-lessons.

E. R. Johnstone writing on "The management of an Institution for Mentally Defective Children," explains methods of enlisting the confidence and co-operation of parents.

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		The Functioning of the Will: a suggested application of Sherrington's work on the Reflex, by C. A. Claremont.
March	12, 1921.	The Intellectual Respectability of Muscular Skill, by T. H. Pear.
		A Method for the Study of Fluctuations in Mental Output: Types of Oscillations Found, by S. J. Philpott.

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(*b*) Industrial Section.

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		The Dotting Machine as a Fatigue Test in a Factory, by May Smith.

(*c*) Medical Section.

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THE BRITISH JOURNAL OF PSYCHOLOGY

GENERAL SECTION

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GENERAL SECTION

INSTINCTIVE BEHAVIOUR AND ENJOYMENT.

BY C. LLOYD MORGAN.

(1) *Its character as such* (pp. 1–3).
(2) *Its dependence on heredity* (pp. 3–6).
(3) *Concrete instances of behaviour seldom purely instinctive* (pp. 6–8).
(4) *Instinctive enjoyment correlated with instinctive behaviour* (pp. 8–10).
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(8) *Levels of complexity. Matter and form* (pp. 18–20).
(9) *Nature, disposition, inclination, and impulse* (pp. 20–22).
(10) *Disposition as* ad hoc *preparedness* (pp. 22–25).
(11) *Innate tendencies in human life* (pp. 25–28).
(12) *Conclusion* (pp. 28–30).

(1) The word 'instinctive' is one which an author feels free to use in such wise as shall best serve his purpose in hand. There is no established convention in accordance with which its connotation is definitely settled. There is not even agreement as to that to which the word is to be taken as adjectival. Some speak of instinctive impulse; others of instinctive dispositions; yet others of instinctive knowledge, instinctive appreciation, instinctive belief, and so on.

Without prejudice to other applications, the word 'instinctive' may unquestionably, in accordance with good precedent, be applied to behaviour. Let us take this as our point of departure. The advantage of doing so is that behaviour is something that can be observed—the life-history of which can be traced. This serves to indicate that what we are here to start with is that which we *can* thus observe—that which *is* actually given for our consideration and interpretation. If it be asked what it is that behaves, our reply here is that it is an organism, including of course man—not an organ or a part of an organism, though these are included, but the organism as such and as a whole. It is clear therefore that our departure platform is to be frankly biological. This it is meant to be. That does not imply however that it is to be biological only. I assume throughout that instinctive behaviour has an inner aspect of instinctive enjoyment—that, in a broad sense of the word, it is conscious behaviour with which we are to deal. Just as the behaviour we observe is an integral phase in the life-history of the organism, so is enjoyment in so behaving an integral phase in the life-history of the psychical system correlated with functional processes in that organism. For biological treatment, therefore, our departure platform is instinctive behaviour. For psychological treatment it is the instinctive enjoyment which is the psychical correlate of such behaviour. I borrow the word 'enjoyment' from Prof. Alexander; but I use the word 'instinctive' with a different connotation.

Now we may distinguish the ground and the conditions of behaviour. The ground, as I use the word, is the intrinsic nature of the organism concerned. The conditions (extrinsic) are afforded by the situation in which that organism is placed. We speak of the effective influence of the total situation as affording the stimulation under which the responsive behaviour is evoked. But we must remember that the nature of the organism, under its existing poise at the time of stimulation, is the ground of action, in the absence of which there would be no such response. For purposes of interpretation, however, we have also to consider the observable outcome of the behaviour, what it leads to, what it is good for. Thus we have the instinctive behaviour itself; its ground in the nature of the organism; the conditions under which it is evoked; and its utility or prospective value.

Under the instinctive behaviour itself fall multifarious instances of thus behaving. It is the group-name under which they are all comprised. There would be no point, however, in using the distinguishing adjective, if there were no characters which earmark any given instance of thus behaving. What, then, are the criteria of behaviour as instinctive, by which all instances which have them may be distinguished from other instances of behaving? Can we say from simple inspection whether some observed act of some given animal or of man is instinctive or not? We cannot do so. There is nothing on the face of it to enable us to say whether it is or is not. We must broaden our view so as to assign to it a place in the history of the organism. Now here the criterion of instinctive behaviour which has been widely accepted, and is here accepted, is that the act which affords an instance has not been learnt or acquired in the course of individual life. This, however, gives to the criterion a negative form. If we seek to translate it into something positive we can do so, under safeguards, in reference to heredity. An instance of instinctive behaviour is one the 'form' of which is inherited and not acquired by the animal that so behaves. If then a complex act is simply observed we may be quite unable to say whether, taken by itself, it is an instance of instinctive behaviour or not, but taken in the light of its history in the individual life the case is different. One sees for example a boy cycling and a duckling swimming. Each is an instance of behaviour; but as a matter of historical development, interpreted on the basis of observation, the boy has to learn to ride, whereas the duckling has not to learn to swim. The 'form' of cycling in boys is acquired; that of swimming in water-birds is inherited. In the duckling there is inherited ability to swim; in the boy the ability to cycle is acquired. In the duckling the enjoyment which accompanies the behaviour is as new in form to the bird as is the behaviour; in the boy the enjoyment in cycling grows and develops as the art of riding is gradually acquired. No doubt the conclusions we draw with regard to enjoyment or conscious accompaniment are inferential. The doctrine of correlation which I accept seeks to afford a reasoned basis for such inferences.

There are other features on which the biologist may lay stress for his special purpose; for example, that under like circumstances closely similar instances of instinctive behaviour are observable in all the members of the same more or less restricted sub-class of animals, *e.g.* males of the same species. This is a matter of statistical evidence. If this clause of a definition be accepted an instance of instinctive behaviour is the functional analogue of an instance of inherited structure. The plumage of a male bird has certain inherited characters; the bird's instinctive behaviour in 'display' has in like manner an inherited and unlearnt character.

(2) While the specific form of instinctive behaviour is the observable expression of unlearnt ability so to behave, instinctive enjoyment is its psychical correlate. This enjoyment is just the inner aspect of that reality (the psycho-organism) of which the observed behaviour is the outer aspect. Under distinguishing analysis the one has its ground in the inherited constitution of the organism, the other has its ground in the correlated psychical system. In the one aspect, as in the other aspect, there is, in some sense, retention from one generation to the

next. Such retention, which is sometimes called 'mnemic,' must here be taken for granted. Just now what calls for emphasis is that what is retained is not biologically a specific mode of behaviour nor psychologically this or that kind of enjoyment. At present we will fix our attention on the biological aspect of behaviour, on the understanding that the psychological aspect of enjoyment is correlated with it. Now it is pretty clear that the nest-building behaviour of the parent is not retained, as such, in the fertilised ova which give rise to the next generation of nest-builders. Nor is the plumage of the male Peacock Pheasant retained in the egg from which he is developed. What then is retained in that germinal substance which, for biological treatment, affords the continuous thread upon which the beads of successive generations of adults are strung? Now here we must remember that, for such treatment in accordance with the doctrine of germinal continuity, the beads are developed from the thread, but the thread is not developed from the beads. In other words, germ-cells are the parents of the body-cells which are concerned in plumage and in behaviour; but these body-cells are not the parents of germ-cells. The parents of germ-cells are other germ-cells in the thread of living substance. What then is retained in the germinal thread from which the beads of adult organisms are developed? Clearly neither plumage nor behaviour for they are functions of specialised groups of body-cells. What then? Can we say more than that some essential feature of the intrinsic constitution of the germinal substance from which the organism is developed persists? That intrinsic constitution is the ground of all changes which occur under appropriate extrinsic conditions. But since this is so general and comprehensive as to be of little use for the interpretation of particular instances we define it *ad hoc* in terms of some specific item of behaviour. We say that what is retained is not this or that mode of behaviour itself, but the ability so to behave. None the less, we speak elliptically of inherited behaviour (or enjoyment); and people know what we mean. On this understanding it is a plain matter of statistical evidence whether behaviour is inherited or not, and if so in what measure.

It is probably quite unnecessary to insist that heredity does not in any true sense make offspring resemble their parents. The fact, or rather the general truth, is that they *do* resemble them in a measure that can be evaluated. This measure may be expressed in the form of a 'coefficient.' Thus perfect resemblance of offspring and parents in any given respect would have 1 as its coefficient of heredity; no resemblance (if it occurred) would be 0. Actually observed resemblance would be somewhere between these values—say ·75. Now I am not acquainted with definite statistics with regard to instinctive behaviour. But in such cases of what I shall call low-level instinct, as swimming, diving, or flying in birds, the procedure of offspring, near the outset of life, very closely resembles that which their parents exhibited at the same stage of development; and the coefficient would, I suppose, be a near approximation to 1. Still, there would be some variation in the behaviour of the offspring of the same parents, on either side of the most frequent measure or the 'mode,' in the technical sense, which, by a play upon the word, we may regard as the mode of procedure normal to the species. According to the theory of natural selection, those whose behaviour falls seriously below the normal mode of procedure are eliminated. Hence under such elimination the coefficient of hereditary transmission retains a value more or less closely approximating to 1, say, at a guess, ·95, with some slight range of variation above and below this value.

What we have been dealing with is such ability as is, on the evidence, unlearnt; and there are numberless instances of instinctive behaviour concerning which the evidence is quite convincing to an unprejudiced observer. Of these, the swimming of a duckling, a few hours old, is one. But there is also acquired ability of which we have taken a boy's cycling as an instance. The question therefore arises: Does not that also depend on heredity, as the phrase goes? It does, in so far as there is inherited ability to learn to ride. Here, however, the word 'ability' is used to cover both performing and learning to perform. Since this may lead to ambiguity, and since we have two words—'ability' and 'capacity'—may it not be well to differentiate? Let us then speak of *ability* to behave (*a*) without learning, and (*b*) through learning; and *capacity* to learn or acquire. The boy's capacity to learn to ride a bicycle is certainly inherited; otherwise he could never acquire the art of cycling. Wherein, then, does the duckling that swims differ from the boy that cycles? The former has inherited ability; the latter has inherited capacity, but the ability must be acquired. This differentiation of meaning is not, I hope, unduly strained. It is what we have in mind when we say that some pupil has plenty of capacity for, say, mathematics, though he has, from lack of training or want of application, little ability to solve problems. It is convenient to speak of inherited capacity as *innate*, and to reserve the word 'instinctive' for inherited ability to behave in some specific manner without learning. On these terms, intelligence as the capacity of learning through experience is innate; the intelligent *act* which depends on such learning is acquired by the individual; and behaviour thus acquired is to be distinguished from instinctive behaviour a criterion of which is that it is not so acquired.

(3) Thus far I have sought to distinguish (1) instinctive behaviour as unlearnt from that which we may now call intelligent behaviour the form of which is the outcome of learning; and (2) inherited ability to behave in some definite way without having to learn how so to act, from innate capacity to acquire some modification, let us say improvement, in the form of behaviour.

We may here pause to note that by intelligent behaviour we are to understand that which is learnt through profiting by experience. It is the outcome of guidance and control and in that sense is acquired in the course of individual life. Physiologically regarded, it involves a loop-line neuronic path, and the playing down of controlling centres on those which lie below them in the hierarchy of neural arcs. On these terms instinctive behaviour is logically distinguishable from intelligent behaviour; for the former is unlearnt and the latter is the outcome of learning. But it may be said, by Prof. Stout for example[1], that the acquisition of psychical data is part of the process of learning, and is therefore included in the intelligence which learning implies. There is, however, a valid distinction between the *provision* of data and the *acquisition* of data, if this be held to imply some seeking and choosing. The latter involves intelligent process; the former does not. My position is that the provision and psychical registration of data through unlearnt behaviour does not itself imply intelligent process. There may no doubt be, and in many cases is, innate capacity for dealing with such data; but we should distinguish between the intelligent process of so dealing with them, the outcome of which is intelligent behaviour, and the initial provision of data through instinctive behaviour.

The next thing clearly to realise is that the instances of behaviour which we commonly find in animal life are partly unlearnt and partly learnt. In other words there is an instinctive component or factor and an intelligent factor in the blended product which is what, on most occasions, we actually observe. And it is the difficult task of analysis to distinguish these two factors which combine in inseparable union to give the behaviour we see. Take for example the flight of an adult swallow. The young bird flies passably well the very first time it is committed to the wing. It has unlearnt ability so to fly. But it learns to fly far better. To its unlearnt ability there is added much improvement through acquired excellence, in virtue of the bird's innate capacity for profiting by experience. What therefore we commonly call 'an instinct' (which means an instance of instinctive behaviour) in adult life is probably never an instance of what may be spoken of as 'pure instinct.' It is an instance of behaviour in which the instinctive factor is still in evidence. Notwithstanding much acquired grace and ease, the swallow's flight is still traceable back to a stage at which the form of behaviour was unlearnt.

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[1] *Manual of Psychology*, 1913, 350.

Now the task of analysis is admittedly difficult; more difficult in some cases than in others; most difficult of all in that of human behaviour. This is partly because so much of that behaviour, even in the infant, is not traceable back to definite unlearnt forms, but to what seems to be a relatively formless assemblage of random and, in themselves, aimless movements. This affords much of the matter to which form is given in the course of personal development. There is probably less unlearnt ability in man than in any other organism, as there is more innate capacity for learning new forms of behaviour. But there is some; and the aim of scientific research is to discover it. There are, therefore, quite reasonable grounds for saying that the distinction between unlearnt (racial) and individually acquired forms of behaviour is sometimes so difficult to apply that it has then but little practical value[1]. But that does not invalidate the logical distinction; nor does it preclude our seeking instances in which it is susceptible of practical application. Such instances abound in animal life. There we often find serviceable forms of instinctive behaviour which, so far as the evidence afforded by careful observation goes, are unlearnt and common to all like members of the same species.

We have, then, to follow up their history in the individual life. We must ask: Does the performance show signs of improvement under intelligent guidance? Is there something more than smoother running as the organic 'mechanism' 'finds itself'? It is a matter of careful observation supplemented by cautious inference. In some cases there are unmistakable signs of improvement; in other cases there are not. The latter clearly approximate more closely to instances of 'purely' instinctive behaviour than the former. There is little sign of improvement, under intelligent guidance, in many of the instincts of insects. The nest-building of some birds seems often as true to type on the first as on later occasions. But very much of the locomotor behaviour of vertebrates—running, swimming, diving, flying—shows marked signs of improvement. The inherited ability to behave passably well is supplemented by much acquired skill.

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[1] Cf. Rivers, *Instinct and the Unconscious*, 41; cf. this *Journal*, x. 1 ff.

There need not be any knowledge, as we count knowledge, of how the betterment comes in the course of experience. It just comes, here a little and there a little; and it carries the naïve satisfaction which gives savour to active life. Which of us, with our human prejudice in favour of intellectual knowledge, knows much more than that, by some process of selection, unconscious or subconscious, redundancies and awkwardness which at first led to comparative failure are gradually eliminated and that just the essential business part of the skilled behaviour is knit into a nicely co-ordinated whole? Then it looks so easy in another, and in oneself the enjoyment runs so glibly. The youngster who rides his bicycle with nonchalent ease has very little detailed knowledge of how it is done. Ask him which way he turns his handle-bar when he heels over a little to the left; he is puzzled and often gives the wrong answer. But he *does* it, on the cycle, right enough. The swallow with yet more consummate mastery of a yet more difficult art probably has no knowledge whatever (of the intellectual kind) about the hows and whys of aerial flight. His not the why to give: his but to do and live.

(4) Instinctive behaviour is just an integral phase in the current life of the organism. We may distinguish its characteristic features; but we cannot profitably deal with it apart from its context; for it is what it is in relation to that context. Furthermore, to behave in this way or that, under given circumstances, reveals the nature of the animal which so acts. Thus we have, as noted above, (1) the behaviour itself, (2) its ground in the constitution of the organism, (3) the external conditions under which it is evoked, and (4) the outcome which we, on the basis of more extended study, regard as having prospective value or utility. The aim of analysis must not be misunderstood. It is not to separate and assign independent existence to nature's inseparables; it is to distinguish whatsoever may be distinguishable (and helpful to distinguish) within an integrated whole. Even so, however, something more must be added. For even in comparatively simple instances there is a current *process* of behaving within which there are sequent phases. And if we, so to speak, take a snap-shot of it in the midst of its course, the phase we thus isolate for consideration is what it is partly in virtue of precedent phases in a continuously developing process. We must realise, therefore, that behaviour, under (1) above, is an integrated and fluent whole which is susceptible of further analysis into the sequent

phases of its development. When we further bear in mind that under 'instinctive' we are to deal with the unlearnt *factor*, without prejudice to acquired factors which may be, and perhaps in some measure nearly always are, compresent therewith, we are in a position to consider that instinctive enjoyment, or consciousness in the broad sense, which is correlated with instinctive behaviour.

Now if we are to take correlation seriously, and if the foregoing treatment of instinctive behaviour be provisionally accepted and not unwarrantably confused with other applications of the adjective 'instinctive' (*e.g.* to knowledge or to impulse), then the psychical analysis must proceed *pari passu* with that applied to behaviour as objectively studied by the biologist. On these terms instinctive enjoyment is that which is the psychical correlate or accompaniment of the instinctive behaviour itself under (1). It is grounded in the constitution of the psychical system (2); it is evoked by the external conditions of sensory presentation (3); it has prospective value of the psychical order in that it provides data for subsequent use in what we speak of as intelligent profiting by experience (4). But what it *is*, as instinctive enjoyment, is, not what it is grounded in, not what it is evoked by, not its utility in mental development—important as all these are—, but just what it actually *is*, namely, the psychical correlate of instinctive *behaviour*. That it is, and nothing else than that, for my treatment under the method of distinguishing analysis. If this be not grasped, I cherish no hope that the thesis I seek here to develop will be understood.

I shall on another occasion say what I can in support of a suggestion that the word 'consciousness' may be used in a broader and in a narrower sense. In the broader sense it comprises all those instances of being conscious which are said to be 'above the threshold' (however that may be defined). In a more restricted sense it comprises only those instances which carry what James would call a 'fringe' of revival or expectancy. The criteria of being conscious in this narrower sense are (*a*) an enjoyment of againness when *this* which is now presented resembles *that* which was experienced on former occasions, and (*b*) a feeling of expectancy that something is coming like that which came on former occasions. These criteria are, as I hold, distinctively psychical and are what I call 'qualia' of conscious enjoyment, the validity of which Prof. Alexander so strenuously denies. Now that which comes with the quale of againness is that the like of which has already been given in the course of previous psychical life in the individual. And that which comes with the quale of expectancy in enjoyment is that the like of which has already come in past experience. But neither the one nor the other can qualify the enjoyment that accompanies instinctive behaviour as such, *i.e.* the instinctive factor in behaviour which is distinguishable under analysis. For when it first comes it is in form as new in the psychical life of the individual as is the behaviour in the organic life of that animal. The instinctive enjoyment which is correlated with instinctive behaviour is accompaniment and accompaniment only (epiphenomenal if you like the word), without guiding efficacy though it provides the data for conscious guidance in the life of fuller and further development.

The stress then here, for resolutely analytic treatment, is on the locus of origin of integration. Instinctive behaviour, with its correlated enjoyment, is an integrated product. But this integration is not (as it is in the intelligent factors in behaviour) of conscious origin, in the restricted sense of the word 'conscious.' It is integration in that part of the psychical system which is analytically distinguishable as (in this sense) the unconscious. But in the broader sense of the word 'consciousness,' which includes all modes of felt enjoyment, it may be thrillingly conscious. What then distinguishes the *restrictedly* conscious factor in the total enjoyment? Its newness, its freshness; the absence of againness or expectancy. When the duckling first swims—when the moorhen first dives—a quite new 'form' of experience (even if the 'matter' be old) breaks in on the already current stream of enjoyment. There is no feeling of againness; for there is no againness in the individual experience to feel. There is no expectancy of what will be the outcome of so behaving; for within that experience such outcome has never been presented. Only on the hypothesis that what is learnt in one generation is remembered by the next, can there be any feeling of againness or of expectancy. On that hypothesis (in support of which I can find no satisfactory evidence)[1] the duckling would have a feeling of againness and expectancy based upon so-called ancestral memory. It would remember—not indeed its own previous mode of behaving and its outcome, for it has never so behaved—but that of its parents and grand-parents.

(5) We live in an age which is somewhat impatient of analysis. The cry is: Let us deal with concrete reality in all its varied richness, not with bloodless logical abstractions. But does the analyst here disagree? Surely not, if he hold firmly to the sound view that the necessary complement of analysis is synthesis. We start with some complex whole which is the reality presented for our interpretation; by analysis, so far

[1] Cf. Stout, *Manual*, p. 496.

as is possible, we evaluate the factors; then follows synthesis by which an imaginative thought-construct is framed which, if it may be, shall correspond to the reality with which we started, and shall enable us to grasp the nature of that reality more adequately than we did to begin with.

Now, suppose we start with a given instance of behaviour. We want to know all we can about it. We find that as a matter of history it is partly unlearnt and partly learnt. We consider it as evoked under certain external conditions; as leading somewhither in its outcome; as revealing the nature of the organism that behaves; as correlated with enjoyment which contributes in some measure to the manner in which the animal is conscious (in the broad sense) at the time. How can we distinguish these several items within a complex whole save by analysis? How can we grasp the true nature of what actually takes form in real life save through intellectual synthesis or ideal construction?

There is trouble, however, when we have to employ technical names. If we use the adjective 'instinctive' to qualify that which we name 'behaviour' in so far as it is unlearnt, we apply it analytically to that which we distinguish within the complex reality. But if we apply it to the synthetic complex we inevitably use it with a different connotation. For thus synthetically used, it covers much more than it does when it is used analytically. I venture to urge that its value as a qualifying adjective lies in its analytic usage.

But why select this one when others are open to our choice? Of any technical name the connotation (as I use the word)[1] is a matter of convention. The pity of it is that there is so little agreement in this case. We shall have therefore to consider some other applications of the word 'instinctive,' and just now its application to knowledge. We here include in our purview that which evokes behaviour. The kind of knowledge in question is that of immediate sensory acquaintance—that which is not mediately learnt. This it is that is said to warrant the application of the limiting word 'instinctive.' Now, suppose that it be asserted that an animal has instinctive knowledge of *this* 'as good for food,' of *that* 'as distasteful,' or of some other animal 'as a natural enemy.' For this, by the way, apart from any acquired knowledge, there is, I believe, very little evidence; but let that pass, important as it is. Put the thing very generally, as it is often put. The animal has, it is said, 'instinctive knowledge' how to act under these or those circumstances. What is the evidence? We observe that under such conditions the animal does so act. And then we say: How could it possibly so act unless it knew how to act? But I submit that this assumed knowledge is not in the evidence. Here, however, much turns on what we are to understand by knowledge. If it be said that the enjoyment involved in any form of sensory presentation is a kind of knowledge, *cadit quæstio*. For obviously enjoyment in behaving is such a kind of knowledge. But surely this is not what is meant when the phrase 'instinctive knowledge' is used. To know how to act without any prior experience of so acting is if I mistake not something quite distinguishable from enjoying the action in progress. And I contend that *this* is neither given in the evidence nor necessary to the interpretation of that which is so given.

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[1] Cf. Keynes, *Formal Logic*, Part I, Ch. II, § 17 (1).

The assumption of inherited knowledge (not to be confused of course with hereditary capacity for learning to know) may, however, take a different form—that of instinctive prevision—expectancy of what is likely to come although the like has never been experienced. This is clearly connected with what has been called above the outcome, or utility, or prospective value. In this form, what is given at the outset is not only how to act under certain given circumstances but, however dimly foreseen, the purpose or end of the act. Prof. Stout accepts such instinctive prevision as always present, though often in a guise that is inexpressibly vague, but yet not wholly indeterminate[1]. "Animals," he says, "in their instinctive actions do actually behave, from the outset, as if they were continuously interested in the development of what is for them one and the same situation or course of events; they actually behave as if they were continuously attentive, looking forward beyond the immediately present experience in preparation for what is to come[2]." I suppose no one dreams of denying these 'as if's.' The bird builds a nest as if it foresaw the laying of eggs therein. But does it foresee? I confess that I place no confidence whatever in the whole brood of 'as if's.' I must continue therefore to lay stress on instinctive behaviour, leaving instinctive knowledge and prevision to M. Bergson on the one hand and Prof. Stout on the other.

(6) Let us now consider the relation of intelligence to what I call low-level cases of instinctive behaviour, and try to correlate this psychical aspect with the physiological concomitants. It will involve some rather technical treatment. We must bear in mind that the function of intelligence is to control and to guide; and that physiologically this always involves neuronic loop-lines.

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[1] *Manual*, p. 355.

[2] *Loc. cit.* p. 351.

In the accompanying diagram, which is drastically schematic, *D* stands for the receptors of all kinds, including those which receive kinaesthetic stimulation from the organs concerned in behaviour *C*. Under *B* fall the subcortical centres which are concerned in purely instinctive behaviour of low-level kind. *A* is the cortex, with the controlling function of which intelligent consciousness is correlated. Its shaded part is correlated with reflective thought where that is in evidence, *e.g.* in man.

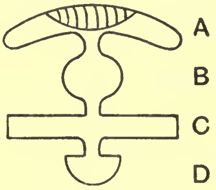

Fig. 1.

A Cortex that controls.
B Subcortical centres.
C Organs of behaviour.
D Sensory and kinaesthetic receptors.

Now, suppose that the connexion between *A* and *B* is severed. In that case we should have instinctive behaviour not merely logically distinguished, but physiologically isolated[1]. What then happens? Something of this sort. The sensory receptors in *D* are stimulated by the external situation; afferent physiological impulses proceed to *B*; and thence a co-ordinated set of efferent impulses pass to *C* and there is some definite form of behaviour. But thereby kinaesthetic receptors in *D* are stimulated; impulses proceed to *B*, and are reflected back on to *C*, modifying further behaviour. The behaviour, however, leads to fresh developments in the situation. Often other sensory receptors are stimulated, as is the case when a chick, in pecking, takes something into the mouth. *B* is further influenced and a new form of behaviour (*e.g.* swallowing or rejecting the food) results; and this new form of behaviour, and its outcome, yet further influences *B*. There is thus a series of stimulations and a chain of behaviour as, in and through the behaviour, the situation develops. The integrative action of *B* at any given moment is in accordance with the whole pattern of a constellation or configuration of neuronic influences. What happens is of the same kind as that which Prof. Sherrington has so brilliantly demonstrated in the 'spinal animal,' especially in its relation to what may be called the 'this or that' principle —where *this* and *that* are incompatible responses. One does not get a nondescript muddle-up of one and the other. This is of quite fundamental importance, and without prejudice to the 'all or none' principle[2] which has also to be reckoned with. It is not yet adequately realised to how large an extent the physiological conditions of 'conflict,' and of the so-called 'barrier,' are laid in the 'this or that' principle. In instinctive procedure this is endorsed by natural selection, as is, on occasion, the 'all or none' reaction. It is tolerably obvious that the highly specialised behaviour-attitude of immobility—so different in little plover and in pheasantling—is biologically incompatible with either fight or flight at the same time, though *this*. *that*, and *the other*, may afford examples of 'alternation'; and that to be of protective value there must be 'all or none' of such immobility, though fight or flight may be, and observably are in normal life, graded according to circumstances.

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[1] Cf. *Instinct and Experience*, Ch. III.

[2] See W. H. R. Rivers, *Instinct and the Unconscious* and cf. C. S. Myers, this *Journal*, 1913, VI. 141 ff.

Even low-level forms of instinctive behaviour reach in themselves a relatively high level of complexity. Furthermore—to add complication to that which is already sufficiently complex—the physiological impulses reflected down from *B* influence not only the organs of behaviour, but the whole organism, altering its physiological balance and, in correlation therewith, the emotional tone of enjoyment. This we must leave on one side, though we must bear in mind that how the organism actually behaves depends in large measure on the existing poise of the whole vital system.

Essential features, then, of low-level instinctive behaviour are (1) that it depends on integrative action in *B*; (2) that it carries into a higher phase of life such physiological principles as 'all or none' and 'this or that'; and (3) that it is, within some range of variation, equal to the biological occasion or has survival value.

Thus far we have supposed the connexion between *A* and *B* to have been severed. We are now to regard that connexion as remaining intact. What then happens? Something of this sort, stated as briefly and in as simplified form as possible. When the sensory receptors in *D* are stimulated under the presentation of the situation, afferent impulses proceed to *B*, are reflected as before on to *C*, but *also* pass up from *B* to *A*. Similarly kinaesthetic receptors send impulses to *B*, which are in like manner handed on to *A*. In general *all* that is sent up to *B* may be transmitted to *A*. From the inherited build of the nervous system, *A* may (to speak picturesquely) receive information through *B* of that which occurs in the body as the result of its integrative intervention. And the information with regard to behaving is the instinctive enjoyment which accompanies this low-level instinctive behaviour in so far as it provides data for control under the functional action of *A*. Under this proviso it is the psychical correlate of the functioning of certain centres in the cortex of the brain or its analogue (*e.g.* in birds).

It must not be supposed that enjoyment is necessarily limited to correlation with the functioning of the cortex or its analogue. More probably it is the psychical correlate of the functioning of *the whole organism.* Consciousness is based on and in touch with the unconscious, which is psychical in its nature, and not with physiological processes as such—*i.e.* only with their inner aspect in enjoyment. One does not deny—nay rather one gladly accepts—'protopathic' enjoyment in the thalamus and perhaps elsewhere. The point is that only when the enjoyment is also 'epicritic' does it enter into the field of discriminative control which characterizes the integrative business of *A*. It is this utilisable information which counts in psychological development.

But the reception of information is one thing; its utilisation in control is another. There must first be information of instinctive behaviour in progress; then follows control in the light of this information. And this control is the mark of intelligent process—not the receipt of information, but its utilisation in action through the playing down of *A* on the underlying centres in *B* or on centres in the spinal cord.

There seem then to have been evolved in close inter-connexion two systems—that of *B* and that of *A*. The functional activity of *A* depends on that of *B*; but initially the functional activity of *B* does not depend on that of *A*. Each is integrative in its action. But the secondary integration due to *A* may modify the primary integration due to *B*. That is what is meant by saying that instinct may be 'plastic' and may be moulded to finer issues. Then, however, it ceases to be purely instinctive.

Only in certain organisms, however, and in certain fields of their behaviour, is there *B*-integration *in instinctive form.* In some animals much of the initial behaviour-stuff consists of relatively random movements comparatively destitute of integrated form as a whole. In such cases, though random movements supply the 'matter,' the form of behaviour as a whole is acquired and due to intelligent guidance under the influence of *A*. This is markedly so in the higher animals, with protected infancy, where parents shield the offspring from the eliminating effects of natural selection. It is conspicuously so in man. There is, therefore, a central core of truth in the old-fashioned antithesis between the instincts of the lower animals and the intelligence of man. No doubt in one large field of behaviour—that connected with reproduction and the relation of the sexes which is subsequent to the period of infancy—there is in man a recognisable legacy of instinctive form, as Freud and his disciples insist. But marking as it does the period of adolescence—coming as it does after so much has been told and read about it, and after manifold warnings of the danger to the moral life which besets its advent—how difficult is the task of distinguishing what are here regarded as the purely instinctive factors! But when all is said and done, we must realise that the human subject is the very last which the investigator of instinctive behaviour should select as a basis for interpretation, or for purposes of illustration.

A word or two of warning may here be necessary in view of a different application of the word 'instinctive.' It is quite commonly used in contradistinction to 'reflective.' If this or that mode of procedure of men in social life has not come to be what it is under the guidance of reflective thought, it is said to be instinctive. This does not mean that the procedure in question is unlearnt; for it is in large measure learnt from cradle-days onwards. It does not even seek to emphasize the presence of such an unlearnt factor as that with which we have been dealing. What it seeks to emphasize is the absence of reflective thought—the absence of fully rational guidance. For such 'instinctive' procedure (in this sense) we often trump up reasons by a process which is nowadays called 'rationalisation.' Perhaps nine times out of ten something of this sort is the meaning of the word 'instinctive' in popular speech. It is, of course, quite a legitimate usage under suitable definition. If I mistake not, Prof. Alexander always uses it thus. It is the usage which Mr Trotter's treatment of the 'herd-instinct,' and Freud's comprehensive treatment of the sexual instinct, have done much to render current. It would be foolish to raise any objection to this usage. Still it should be sufficiently clear that much behaviour that is in large measure the outcome of much personal experience is *in this sense* instinctive, but at the same time not wholly, or even mainly, instinctive in the narrower sense we have here in view. To confuse the one usage with the other can only lead to intellectual smudginess.

To summarise the position, there are three main levels of integration: (1) reflective integration in the sphere of thought-processes; (2) integration in the sphere of naïvely perceptual and unreflective profiting by experience; and (3) integration of the unlearnt order. As I use the word 'instinctive,' so far, the behaviour thus distinguished falls under (3). But the word is often applied to both (2) and (3).

(7) Leaving reflective thought (which is the prerogative of man) on one side, I distinguished above instinctive *B*-integration from intelligent *A*-integration, the former primary, the latter secondary. In some animals, and in some fields of behaviour, the former predominates; in other animals, or in other fields, the latter. The flight of an adult swallow is

a blended product. It retains some measure of the primary integration (in evidence on the first occasion of flight); but this has been modified and much improved by the secondary integration through which it is controlled and rendered more serviceable in the life of the bird. A much debated question here arises: Is the acquired excellence due to *A*-integration in one generation, inherited as *B*-integration in the next? Some say: Yes. Others say: No. It is a matter of statistical evidence into which we cannot here enter. Let us then put it in hypothetical form. If the Noes have it, what then? Then, from the racial point of view, instinctive behaviour is not the offspring of intelligence, though intelligence may play the part of kindly aunt who ministers to its survival in the species. It should be noted that this does not imply that the evolution of instinctive behaviour is necessarily independent of intelligence; for it may be largely dependent on the good offices of the aunt. It does, however, imply that the aunt who sustains it and nurses it, who teaches it and guides its steps, who makes it through training other than it was, is not its mother. Apart from its instinctive mother it would not be born; apart from the aunt it might not be reared; it might perish and have no heirs. But what the behaviour actually becomes in later life is partly derived by direct inheritance from the instinctive mother and partly due to the teaching of the intelligent aunt who dwells in the same household of the organism.

One has to distinguish in what I have called the *B*-system *variations* in nature, around the specific 'mode,' of germinal origin, and *modifications* acquired under the nurture provided by *A*. The survival of the organism depends on both when both are compresent. Let us then tabulate the possible combinations.

Variations and modifications in B-system.

(1) Favourable variation *with* favourable modification due to *A*.
(2) Favourable variation *without* favourable modification due to *A*.
(3) Unfavourable variation *with* favourable modification due to *A*.
(4) Unfavourable variation *without* favourable modification due to *A*.

In this table unfavourable *modifications* are not entered because animals in their free life do not contract many bad habits.

In the first class are those which are strong by nature and stronger still through nurture; in the fourth class are those which are weak by nature and have not been improved through nurture. In the first class are those that inherit good instinctive ability which is supplemented by acquired ability; in the fourth class are those with poor natural ability which remains no better than it was at first. It seems pretty evident that in the struggle for life an animal in the first class will stand the best chance of survival and of propagating his like; while one in the fourth class will run the greatest risk of being eliminated, without begetting heirs to his weakness. The latter will be the first to go; the former will be the last to stay. If that be so, the best all-round animals will survive, not only because they are first-class by instinctive nature, but also because they are first-class too through intelligent nurture. Acquired ability will therefore favour the survival of those who possess unlearnt ability *along the same line of performance.* And, in the absence of very careful analysis, it will look 'as if' acquired ability were inherited as such; which it may not be, and under our hypothesis is not[1].

Of course in concrete fact there is further complication in that the A-system has its innate capacity for guidance, and its hereditary modes of controlling behaviour; while, at any rate in man, naïvely perceptual intelligence comes under the yet higher control of reflective thought. Of all this we need here say nothing more.

It must suffice to add that, if we entertain the hypothesis that in some animals an A-system is differentiated from a B-system, this may mean, as M. Bergson puts it, that neither is intelligence directly evolved from instinct, nor is instinct directly evolved from intelligence, though they may co-exist side by side in the same animal. Instinct and intelligence are on different lines of evolution. But both lines may contribute to the evolution of the same animal, and may co-operate in the furtherance of its life as a whole.

(8) Before passing on, we have next to notice that there are levels of ascending complexity in what we may broadly speak of as instinctive behaviour. At a relatively low level[2] stand such primitive instincts as swimming, diving, flying, showing fight, crouching when startled, and so forth. An example of instinctive behaviour at a much higher level may be given in some little detail. One of the most interesting instincts of bird life is that which Mr Eliot Howard speaks of as 'securing a territory.' Among migrant birds the male, often one in his first year which has never done the like before, arrives here in England a week or two in advance of the females, and forthwith proceeds to 'stake out his claim.' Let us, however, hear Mr Howard himself. If we "watch the

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[1] Cf. *Habit and Instinct,* Ch. XIV.

[2] I insert here a passage from an article in *Scientia,* XXVIII. 272. I there suggest a provisional scheme of progressive integration.

male of one of our common species—a Reed Bunting for instance—and record its movements each day during the first hours of daylight from about the second week in February, what do we find? Not that its behaviour is of a casual description, not that it is here to-day and gone to-morrow, but that all its movements are subject to a routine which becomes increasingly definite as the season advances. There it is in the same plot of ground, in the same bush or clump of bushes, taking short flights in one direction and then in another, attacking other males that come within a certain radius, and exhibiting in all its actions a strong disposition to make its own just that one particular corner of the universe. Those few acres wherein it performs this routine of activities and awaits a female I have termed a breeding territory, the dimensions of which vary according to the species and according to the environment[1]." Now all this has for the naturalist prospective value. It is the normal precursor of mating and all that follows thereon. But the point which I wish here to emphasize is that there is probably not a single one of the many low-level instincts incorporated in the procedure as a whole, which has not undergone much modification—which has not been perfected through a lifetime's profiting by experience. Flying to and fro, perching on a chosen bush, attacking other males—one may say *all* the movements which are subject to that definite routine which Mr Howard so well describes in this and many other passages—are the perfected acts of a bird that has long ago learnt to use them in many and varied situations. In a sense there is nothing new; in another sense the procedure is conspicuously new. It is the 'form' that is new, while the 'matter,' which is incorporated in this form, is old.

This distinction between matter and form is, I suppose, sufficiently familiar. It is, of course, a distinction and not a separation. Following Aristotle, we may say that there is no matter without some form; nor can there be any form without some matter which takes on that relational form. Thus we may pay attention to the 'form' of a hexameter irrespective of the particular words which give the 'matter' of any line in the Homeric poems, in the *Æneid*, or in *Evangeline*. So too, we may grasp the form of a geometrical progression, though the numbers which have relation to each other under this common form are indefinitely variable. But in neither case can one have the form existing in complete independence of *any* matter. Furthermore, just as there may be in a hexameter new matter in a form that is already familiar, so there may be, and often is, the old matter of well-worn significant words set by a poet in a form which is quite new and which one greets with glad surprise.

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[1] *British Warblers*, Pt IX, 2.

The bearing of all this should be obvious without elaboration. In the high-level instinctive act of securing a territory—carried out by a male in his first year who has never before so acted—there is, in a sense, nothing new, while, in another sense, the procedure is, in him, conspicuously new. New too must be the specific character of the instinctive enjoyment. The matter is old; the form is new. All the constituent activities—flying hither and thither, perching, fighting and the rest—are well-nigh as old as the bird himself. But this routine, now for the first time in evidence—the relational form which all this old matter assumes—that is new. We have a typical instance of integration unconscious in origin (in the restricted sense of consciousness). Nor is it only at this higher level of bird-life that old matter takes on new form. The muscles employed in the duckling's swim have perhaps all been used in previous walking; those employed in the moorhen's dive, in the swimming which preceded it. None the less they are used with a difference; the instinctive *form* of swimming is other than that of walking. The instinctive *form* of diving is different from that of swimming. The whole set and poise of the body is different; the limb-movements are observably different; respiration is suspended, and so forth. And correlated with all this, the form of instinctive enjoyment must be different.

(9) Instinctive behaviour has been the pivot on which our whole discussion has turned. In the adult life of vertebrate animals, however, there is only an instinctive factor in behaviour; and we have sought to trace its relation to another co-operant factor in virtue of the presence of which, behaviour is, in part, of intelligent origin. But, as was said above, we have to consider behaviour as evoked under certain external conditions, as leading somewhither in its outcome, and as revealing the nature of the psycho-organism that behaves. It is to this last-named feature that we have now to direct our attention; and in doing so, we have to realise that it is *this*, or some feature therein, and not the behaviour itself, that is regarded by many writers on the subject as the pivotal concept. The stress is then not on behaving in some observable way, but on an instinctive disposition so to behave, and not only to behave in bodily fashion, but more besides. Thus Prof. McDougall defines an instinct as "an inherited or innate psycho-physical disposition which determines its possessor to perceive, and to pay attention to,

objects of a certain class, to experience an emotional excitement of a particular quality upon perceiving such an object, and to act in regard to it in a particular manner, or, at least, to experience an impulse to such action[1]." Here instinct, as disposition, *determines* perception, emotion, and overt behaviour. But in the treatment that follows, it is the behaviour which is specially regarded as instinctive and its name is hyphened with the concurrent emotional state, *e.g.* flight-fear, pugnacity-anger. So too, what Dr Drever understands by instinct is "determinate conscious impulse which is not determined by previous individual experience, but which, nevertheless, enters into and determines individual experience and attitude[2]." The point here to be noticed is the shifting of emphasis from behaviour to disposition or to impulse, the application of the word instinctive to some 'determining' feature in the nature of an independent Mind or Psyche which controls behaviour, and the retention, in some measure, of the distinguishing criterion of innateness as contrasted with acquiredness.

Now if, apart from the animistic hypothesis, this shifting of emphasis conduces to more adequate interpretation of all the facts, it would be foolish to reject it. And where the universe of discourse is mainly restricted to 'instinct in man' it may on these terms be convenient to accept it. But it forms no part of my present aim to argue in favour of one course or the other. From the standpoint of the study of life (and not only of human life), I regard observable behaviour as the best departure platform; but that this depends on, or is grounded in, the nature of the being that behaves, no one is likely to deny.

One has here to take over from common speech words which may be shaped to technical usage with some change of significance. We have, for example, this array of names—nature, temperament, disposition, inclination, and impulse. Let us pause to consider briefly what the plain man understands to be their meaning[3]. I suppose he will regard 'nature' as the most comprehensive and as including both the physical and the psychical constitution. If he has occasion to use the word 'temperament' he understands it to refer to a specific kind of nature—melancholy, sanguine, and so forth—mainly in its mental aspect, but dependent on something 'in the blood.' Without denying a physical aspect to the other names on our list he will take them primarily in their mental aspect. For him, I take it, the disposition at any given time is a *felt* disposition. And, I suppose, he might regard the sequence, given above, as implying a progressive focusing with concentration of activity. And if he be asked to draw some distinction between a disposition to smoke, an inclination to do so, and an impulse to light a cigarette; or (to extend the series) between a choleric temperament, an irritable disposition, an inclination to swear, and an impulse to say 'damn' when he misses his stroke at golf, he will perhaps say that it is a progressive series, which culminates in the expletive which relieves the tension; that he hardly feels the temperament which is an abiding trait of his nature; that the disposition is a more clearly felt manifestation of that trait; that the inclination is yet more insistent; while the impulse is still more strongly felt so that it requires some self-restraint to resist it. And I think he might add that to act under impulse does not imply any intention so to act; nay rather that, whereas when we act with intention we always have some end in view, when we act under impulse we do so without thought of the consequences. Thus I interpret the attitude of the man in the street or in the smoking-room.

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[1] *Social Psychology*, 29.

[2] This *Journal*, x. 29. Cf. his *Instinct in Man.*

[3] Modified from *Scientia, loc. cit.*

(10) Now the word 'instinctive,' though it is used in a general way to qualify 'nature,' has a more specific application to 'disposition.' The first thing to notice is that in the technical sense advocated by some leading writers, a disposition, as such, is not within the field of experience. Thus Prof. Stout says that "dispositions themselves fall outside of experience[1]." Prof. McDougall urges that, *qua* psychical, they belong to the structure of the mind while consciousness arises in the course of its functional activity[2]. And Dr Drever says that "dispositions as such, though determining experience, are never themselves experienced[3]." Here there is a pretty wide departure from popular usage; for on these terms one may not speak of a felt disposition. One may not say that a Reed Bunting has a disposition to secure a territory if one means that it feels an inclination to do so. One might, of course, differentiate between a disposition as an unconscious urge (hormé of some writers) and a conscious inclination. But I question whether this is practicable or desirable. As matters now stand the trouble, in dealing with problems of animal life, is that what is written concerning dispositions in the sense advocated by Prof. Stout and others is pretty sure to be read as implying some mode of conscious enjoyment. In what follows I shall depart from current technical usage and deal with dispositions on the

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[1] *Groundwork of Psychology*, 60.

[2] *Psychology*, Home University Library, Ch. III.

[3] This *Journal*, x. 28.

understanding that some measure of enjoyed experiencing may be included, or at any rate is not excluded. And should I have occasion to use the word 'impulse'—which occurs so frequently in Mr Eliot Howard's work on *Territory in Bird Life*—I should mean, in accordance as I think with popular usage, the focusing of a preparatory disposition on some action at the moment of actual behaviour or just before that moment. One ought perhaps to add that in the usage I suggest it is not the nature, nor the disposition, nor the impulse that acts, but the animal or the psycho-organism. One must be careful not so to hypostatize a disposition as to regard it as having independent existence. It is not, in strictness, a disposition or an impulse that is stimulated and acts, but some being so disposed that responds to appropriate stimulation.

One may first clear the ground for further treatment by noting that we commonly describe a disposition in terms of the prospective value of the behaviour which is its normal expression. We speak of a disposition to secure a territory, to mate, to build a nest, and so on. It is as appropriate to describe an instinctive disposition in terms of its biological utility as it is to describe a reflective act in terms of the intention or end in view. When we speak of a disposition, then, we have a double reference in mind, (*a*) to its outcome in the behaviour itself or its results, and (*b*) to its specific character as the existing poise or attitude of the nature or constitution at the time being. One cannot deal satisfactorily with the facts apart from this double reference, for behaviour is the evidence of the disposition and the disposition is that which is regarded as the ground of the behaviour. But it is with the second reference (*b*), as a matter of emphasis, that we are here chiefly concerned.

Now we place a duckling in water and it swims; we frighten a moorhen and it dives; we put grains of rice before a chick and it pecks; we tweak the down on a pheasantling's neck and it scratches. In such instances of low-level instinctive behaviour there is little point or value in distinguishing the disposition from the nature of the bird. Each does what it does under the given circumstances "for 'tis its nature to." And if we call self-preservation or race-maintenance instinctive (which some would not), here also to speak of a disposition is of little service to further interpretation. It suffices to say that such is the nature of psycho-organisms. But when the nature or constitution of the animal is in some way thrown into a state of preparedness for the further development of some situation—when in the absence of such a state of preparedness as affording an internal factor (*e.g.* sex-hunger) the behaviour under environing circumstances is different—then it seems really helpful to speak of a disposition. It means a state of *ad hoc* attunement of the animal for appropriate behaviour at a coming stage of some normal routine. And I think that we may fairly assume that such attuned preparedness is accompanied by enjoyment and does not necessarily fall outside of consciousness in the broader sense of the word. And this psychical aspect of the disposition is correlated with a state of physiological preparedness in the organism. There is a psycho-physical disposition.

Take an example which is discussed with fuller detail in Mr Eliot Howard's *Territory in Bird Life*[1]. During the winter, before the mating season, Lapwings live together in flocks. The males are seemingly on the most friendly terms with each other. Just such minor squabbles occur as may give some savour to the life of enjoyment. The females are, for the males, just other birds in the flock and are not viewed, so to speak, in the light of a disposition to mate. Then in due season come the changes which constitute *ad hoc* physiological and psychical preparedness. Now one male and now another leaves the flock and occupies a more or less circumscribed area in the fields; and, once there, he is intolerant of other males, fighting with any intruder for all he is worth. With the physiological change (partly due to specific internal secretions contributed to the blood-stream) there is a complete psychical change in the dispositional set of the bird's nature. Substantially the same presentation afforded by another male is now linked with quite different modes of overt behaviour. No longer gregarious he is a solitary occupant of a restricted domain driving all other males away. Presently he is joined by a mate, and the normal routine of reproduction runs its course. But if a cold snap should come on, the separated birds may reunite in a flock, and the male is then no longer intolerant of other males. The physiological state probably reverts to its winter poise, and with this is correlated a reversion to the previous psychical disposition. But it seems that when a male bird (Lapwing or other) has secured a territory and is intolerant therein of all others save a mate, he sometimes returns for a while to the flock which occupies a neutral area. *There* he is no longer intolerant of others, but moves among them on quite friendly terms. Hence under different circumstances (1) in his territory, and (2) with the flock, his disposition is different, so that occupancy of a territory appears to be a determining condition of the behaviour observed therein. If this sort of thing occurred in so marked a form in human life it would perhaps be regarded as a case of 'dissociation,' or even described as an instance

[1] *E.g.* pp. 58–61, 98 and 102.

of 'double personality.' In any case just thus, as it seems, does the male bird in his first year behave though he has never so behaved before nor seen others so behave[1].

Now in dealing above with low-level instinct, I regarded the integration as a function of the system I distinguished as *B*. Is the higher-level integration with which we have more lately been concerned to be regarded as likewise a function of *B*? We do not know. But I think it more probable that it is the hereditary factor in the functioning of *A*[2].

(11) Assume that this is so. Does it not upset the whole treatment of (6)? A distinction was there drawn between *B*-integration, in virtue of which the behaviour was said to be so far instinctive, and *A*-integration, in virtue of which the behaviour was said to be intelligently modified. *B* gives the instinctive factor; *A* the intelligent factor. That, it may be said, is clear enough, whether we agree with it or not. But now it seems to be suggested that there is an instinctive factor in the *A*-business. Surely here there is flagrant inconsistency.

But what was our fundamental criterion of instinctive behaviour? That it is, on the evidence, in form unlearnt in the course of individual life—or at any rate that there is distinguishable, under analysis, such an unlearnt factor. In the case of what I called *low-level* instinct (class 5 of my *Scientia* paper) I regarded the unlearnt factor in integration as having its home in *B*. Is it really inconsistent to suggest that in certain forms of higher-level instinct, there may also be an unlearnt factor in virtue of hereditary modes of the functioning of *A*? May not intelligent integration have its unlearnt 'form' dependent on the inherited synaptic pattern in *A*? I have, however, myself felt the difficulty and have elsewhere spoken of "hereditary dispositions and innate tendencies" in connexion with integration as dependent on *A*. This has been criticized; and I feel the weight of the criticism. Here and now I seek so far as I can to come into line with the views and the nomenclature of others. It does seem to be inconvenient, to say the least of it, to be precluded from speaking of the securing of a territory by male birds as instinctive. So I broaden my use of the word so as to include such higher forms of unlearnt behaviour. What I do *not* include (though others are free to do so under suitable definition) is that component of form which is due to acquisition in the course of individual life.

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[1] I have used here an extract from a review of Mr Howard's work contributed to *Nature*, Jan. 6th, 1921.

[2] Cf. *Instinct and Experience*, Ch. IV.

Now in the behaviour of birds (and there are also plenty of instances in the life of mammals) one can, if one has one's nose really up against observable facts, distinguish the unlearnt part of the form from that which is learnt. And it is pretty definite form—it is, if one may so put it, specific like the form of a hexameter and not generic like the form of verse. It is much more difficult to distinguish definitely specific form of the unlearnt order in human life—a form which is observable in all members of the same more or less restricted group of men. And yet one can, in some measure, distinguish that which comes by nature from that which is acquired through nurture.

Let us take those two conflicting tendencies in human life, recognised for centuries and abundantly illustrated in literature, which Prof. McDougall names self-assertion and self-abasement. It is difficult to hit upon satisfactory designations of antithetical forms of conduct each so very wide in its range of occurrence. Let us briefly consider the part they play in human life, though it has its counterpart, on a much lower plane, in animal life. In the one there is some looking down to (not necessarily looking down *on*) that which is at a lower level on the same line of development; in the other some looking up to that which is above us. Can there be any doubt that both these tendencies are there in the inherited make-up of each one of us? Can there be any question that both are manifested in very varying circumstances? Of course, broadly speaking, we may say that the one appears under these circumstances and the other under those. But how wide is the range of difference in these circumstances! And though fundamentally based on unlearnt foundations and thereby conforming to our definition of instinctive, how largely they are shaped under the teaching of acquired experience! The boy among 'grown-ups' in the drawing-room is (sometimes) delightfully submissive; in the nursery, among 'the kids' he lords it over others. In the company of those who know far more about some subject than we do, we look up to them from our lower level; presently among those who regard us among those who are counted as authorities, we find ourselves laying down the law from our higher standpoint. Some of us are unduly submissive in presence of difficulties; others have extravagant confidence in their power to grapple with and master any difficulty under the sun. In almost everything that we do, now one, and now the other, gets expression. The matter is as varied as life; the form, though it too varies, has features due to nature and not to nurture.

The teacher who knows his business plays upon these complementary traits in the characters of his pupils. If both be present by nature in something like due balance, he seeks to bring it about that each finds its proper place and time in the life of the boy or girl. If, say in the matter of difficulties, there is, in a diffident child, an unduly submissive attitude in their presence, he gives well-graded tasks which can be progressively mastered with increasing confidence. But if the child is by nature too cocksure, the grading of tasks is different. Overweening confidence has to be suppressed; the boy has to learn that there are difficulties he cannot readily overcome. The teacher has to help him to realise this and perhaps rub it in.

It is tolerably obvious, then, that if we speak of these as modes of instinctive behaviour—if with Prof. McDougall we place them on the list of man's 'primary instincts'—we should clearly realise to how large an extent the purely instinctive factor is masked by manifold acquisitions in the life of human folk. We should realise too that we can hardly be said to be dealing with a particular mode of behaviour, or one that involves perceiving and paying attention to objects of a certain class, or one that is linked with a particular emotional affect, unless the word 'particular' and the phrase 'of a certain class' are used in so broad a sense as to lose their usual signification. Still there is present an unlearnt core at the heart of so much that is learnt.

In some cases in human life it is customary to use the word instinctive for what is in a marked degree individual and personal. It is for example in accordance with current and long-established literary usage to speak of the creative intuition of the born artist as instinctive. Of him we say: *Nascitur non fit.* By this we mean that what is most distinctive of his life-work is not only, and not chiefly, the result of laborious acquisition. It is in large measure of unconscious and unlearnt origin. It wells up in him, he knows not how and can only guess whence. How impatient he often is of any psychological interpretation in terms only of what is acquired! It is *not* acquired, he protests; it just comes with glad surprise even to the artist himself whose life is brimful of the gloriously unexpected. That is the joy of it. But (this he will admit) mastery over the *technique* of his art has to be learnt; manipulative ability must, in large measure, be acquired. It is utilised, however, in subservience to the dominant artistic 'instinct.' For the most consummate skill, however consciously and conscientiously won, never yet made an artist. If there be no touch of unconscious genius, of what avail all this mastery over detail? No doubt it may be said that the male bird which secures a territory and acts his part in the drama of reproduction, has *his* touch of unconscious genius, though he too has acquired the necessary mastery of detail in his skilled behaviour. But *it is the genius of the species rather than of a highly endowed individual.* It is perhaps more nearly analogous to the racial traits of the Japanese. And though a comparison of human life and bird-life, from which so many of the foregoing illustrations have been taken, may be instituted on some such lines as these, we must remember that man has above all the capacity of reflecting on the ends to which his inherited tendencies are insensibly leading him on. If the human artist lives along the lines of native endowment, as does also, with a difference, the Blackcap as a consummate master of song, he has also, as man, the beckoning vision of a promised land to which he has not attained—only the outskirts of which he will live to reach. If hereditary racial traits differentiate the Frenchman from the Japanese, each has also his ideal of national development in the future. It is in this prevision of the unattained end to which an innate urge is contributory, and in the rational control of the acts which are expressive thereof, that we find what is most distinctive of human life. Whether the male bird in securing a territory has aught of this anticipatory vision—whether his procedure is subservient to an ideal—is, to say the least of it, open to serious question.

(12) If I were asked: "What are you really driving at in this and so much else that you have written on instinct?" the reply would be: "My aim has been to find its place, as defined, in the evolutionary story of life and of consciousness." Further points may be developed in dialogue form. "Do you mean by this (*a*) the manner in which the mind acts as a directive agency in determining the course of instinctive behaviour (as mere matter of observed fact), or (*b*) the way in which the mind acts as an impulsive agency—as the driving power behind the observed behaviour?" "I mean neither the one nor the other." "But you *must* include both; otherwise you give only a description in terms of observed facts and of truths (so called) which are merely generalisations that formulate the plan of the facts; you leave both the facts and the plan—still more what makes the facts *go* in accordance with plan—utterly unexplained." "Yes, that is so. I base my procedure on science as I conceive it. Science does not pretend to explain anything in the sense in which you use the word." "That may be all very well in physical science which has no immediate concern with mind as the source of all true activity, directive and impulsive. It is an impossible position when one deals with instinct, which can nowise be explained on mechanistic principles." "May I suggest that we drop the use of the word 'mechan-

istic' since it betrays a complete misapprehension of the view which you criticize?" "I confess I do not know how else to characterize that view. In what way would you state it broadly yet succinctly?" "In terms of relatedness. Alike in a field of purely mechanical relatedness, in one of physico-chemical relatedness, or of organic relatedness, or of conscious relatedness (awareness)—*i.e.* in any field of relatedness—given the varying 'density' of that field, and the configuration of terms therein, something happens. Our business is to state clearly what happens, and what is the plan of that happening." "But you are forgetting the fundamental question, what makes it happen and guides the course of its happening?" "All that we can know or surmise about this is derivative from the *de facto* happening. In science (again as I conceive its aim) we deal with facts and seek to formulate their plan." "But is not instinct, as a mental force, a fact at least as worthy of consideration as any of the physical forces?" "Force in this sense is not a scientific concept. It has been almost universally rejected in physical science. I venture to anticipate its rejection in any philosophy which is founded on scientific method." "No doubt it may have been wisely rejected in physical science. Its methodological exclusion there is justifiable because there is no activity—no vital impulsion—which intuitive experience alone can reveal." "You are doubtless aware that philosophers of standing, who are not specially tainted with what you may call materialism, have roundly denied the validity of your interpretation of this intuition." "Has not Bergson conclusively shown that their attitude is untenable?" "That is a matter of opinion. But does M. Bergson regard *l'élan vital* as a concept of science? I had thought that his contention is that science is quite incapable of revealing this vital impulsion[1]." "Be that as it may, at all events modern psychology as a science is not so narrowly pedantic as to disregard such facts as these. And all progressive discussion of instinct defines it in terms of mental activity, impulse, and driving force."

"Do you speak of 'progressive discussion' in England, or in America, or elsewhere?" "No doubt there is in the United States a school of behaviourists, so called,—I half suspect you are an alien member of the school—whose aim apparently is to deal with psychology irrespective of consciousness." "What do you suppose they here mean by consciousness?" "That which plays the part of the Prince in the psychological play—the activity of mind as an agent." "I think you are right. They follow the lead of Wm James in his answer to the question: Does consciousness exist? Most of them say that it exists as a function but not as an agent; and not only they but, if I mistake not, the majority of American psychologists." "I really fail to see the bearing of all this. Are we not wandering from the main issue?" "And that is—?" "Well, I am still perplexed as to what may be your philosophical aim, if you have one." "Here as elsewhere, and perchance more fully in a forthcoming work, to revert to the teaching of Spinoza, and to restate it in modernised form. One corollary of his doctrine is that there is no interaction between instinctive behaviour, as a mode within the attribute of 'extension' and instinctive enjoyment, as a mode within the attribute of 'thought' or of consciousness." "Thus does he, and do you, leave the driving power behind what you are pleased to name instinct, and behind every act properly so called wholly inexplicable." "Had not Spinoza something to say about God?"

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[1] Cf. *Creative Evolution,* Eng. Trans. 171.

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IS A FATIGUE TEST POSSIBLE?

BY B. MUSCIO.

(*A Report to the Industrial Fatigue Research Board.*)

I. *The conditions of search for a fatigue test* (pp. 31–33).
II. *The extent to which these conditions can be realised* (pp. 33–41).
III. *Proposed fatigue tests* (pp. 41–44).
IV. *Conclusions and suggestions* (pp. 45–46).

I. THE CONDITIONS OF SEARCH FOR A FATIGUE TEST.

THE conditions of experimentation with the purpose of finding a fatigue test are two:

(*a*) that we know what we mean by fatigue;

(*b*) that we have some method *other than the use of a suggested fatigue test* by which we can *know* that different degrees of fatigue are present at certain different times.

(*a*) That the first of these conditions is necessary is self-evident: it is obviously absurd to set about finding a test of an undefined entity.

Strictly, however, it would be sufficient if we knew that certain different degrees of fatigue were present at certain different times (without knowing the nature of fatigue); but it is in fact impossible to know this so long as the nature of fatigue is unknown. Nevertheless, it is not necessary that our knowledge of the nature of fatigue be exhaustive; incomplete knowledge, of a particular kind, *about* fatigue would be sufficient to render the search for a fatigue test reasonable. This knowledge would need to be of the form: fatigue, though not known exhaustively, has a specified characteristic expression. It would be necessary to possess this as the minimum of knowledge before we could know in what degree, if any, fatigue were present at certain times.

(*b*) If it be not known *before the application of a proposed fatigue test* in what degree fatigue is present, the result obtained from its application will be destitute of significance. The results of any proposed fatigue test, in so far as it is a fatigue test, must correspond exactly with the presence of different degrees of fatigue; and the determination of such correspondence requires a knowledge of *two series*, namely, *on the one hand*, a series consisting of results obtained from the application of the test at different times, and *on the other hand,* a series consisting of the different degrees of fatigue present when these results were obtained. The knowledge of this second series must be obtained by some other means than by the proposed test; otherwise we should have only one series.

Suppose that we obtained from the application of the skin-reaction test[1] results of the following kind: that the appearance-time of the streak after one hour's muscular work is seven-eighths of the appearance-time before beginning work; that after two hours' muscular work it is seven-eighths of what it was after one; that after three hours' muscular work it is seven-eighths of what it was after two, and so on; and that pauses of certain lengths and at certain times increase the appearance-time by certain definite amounts. What would follow? It would certainly *not* follow that we had a fatigue test *unless we already knew* that fatigue after one hour's work is greater than before work, that after two hours' work it is greater than after one, and so on; and that pauses of certain lengths removed definite amounts of it. If we did not have this knowledge, we should need, after obtaining these results, to discover their significance; and, of course, it *might* then be proved that they indicated the course of fatigue—that the test was a fatigue test. *To prove* this, however, it would be necessary *to know* the degree of fatigue present at any time the test was applied.

Or suppose that some mental task were suggested as a fatigue test. A possible procedure would then be to apply this test to a number of workers at different periods of the day—for convenience, let us say, at 10 a.m. and at 5 p.m. Suppose that 60 per cent. of the subjects were to give a poorer result at 5 p.m. than at 10 a.m., and that the remaining 40 per cent. gave the better result in the afternoon. The experimenter would almost certainly conclude that this mental task is not a test of fatigue. But only if he *knew* that every one of his subjects was more fatigued at 5 p.m. than at 10 a.m., would this be a justifiable conclusion. The chances are considerable that the conclusion would be drawn in the absence of this knowledge, as the degree of fatigue present at any time is frequently assumed. 'Inconsistency of results,' whether obtained from the same or different persons, cannot be used to prove that the method of getting them is, or is not, a test of fatigue, unless it is *known* that different results are obtained when the same degree of fatigue is present, or the same results when different degrees of fatigue are present.

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[1] If a blunt point be drawn across the skin surface, with moderate pressure, a whitish streak appears in a few seconds, which maintains a constant whiteness for a short time and then gradually disappears. Prof. A. H. Ryan, formerly of Tufts Medical School, Boston, has recently attempted to determine whether the duration of this 'skin-reaction' shows characteristic variations with fatigue.

It is now necessary to consider how far the above conditions of experimentation for the finding of a fatigue test are realisable.

II. THE EXTENT TO WHICH THESE CONDITIONS CAN BE REALISED.

(1) In the present state of scientific knowledge, two definitions of the intrinsic nature of fatigue may be offered. When organic matter is 'continuously' active, energy is transformed into heat and mechanical work, and there tend to accumulate chemical products of the activity, of the character of organic poisons. The nature of fatigue may be specified by reference to either of these two facts.

(*a*) Fatigue may be defined as that condition in which a certain percentage of available organic energy has been transformed into heat or work (the more energy transformed after this point has been reached, the greater the fatigue).

Determination of the percentage of energy to be transformed before the fatigue condition is reached would be guided by common usage of the term fatigue. Once this percentage had been determined, the definition would yield an adequate experimental situation. Calorimetry would show when different percentages of energy had been transformed, and consequently when different degrees of fatigue, according to the definition, were present. It would then be possible to search for some unique characteristic of the fatigue condition, and the method of showing the presence or absence of this characteristic at any time would be a fatigue test.

While adequate for the purpose of fatigue test experimentation, this definition does not conform to current scientific opinion concerning the essential feature of fatigue. The current view stresses the accumulation of organic poisons in the active organ. It is based chiefly upon the results of experiments with muscle and nerve-muscle preparation, and is in accord with certain of the facts brought to light by these experiments. We thus come to the second definition.

(*b*) Fatigue may be defined as that condition in which certain chemical products of activity, of the nature of organic poisons, have accumulated in the organ that has been active; 'different degrees of fatigue' meaning 'different amounts of these activity products.' As it is known what some at least of these products are in active muscle, it might seem that this definition would be satisfactory; but this is not the case.

When stimuli are repeatedly applied to nerves, a condition soon occurs at the synapses (at least) that renders the passage of the impulse more difficult. The nature of this blocking is at present speculative; it certainly is not known to result from the accumulation of metabolites. The synapse blocking is nevertheless considered most definitely to be a part of fatigue. Consequently, it is impossible at present to define fatigue by reference to the accumulation of metabolites alone; *the definition adopted must refer both to these and to the phenomena of 'blocking.'*

The question that now arises is: can we (independently of a fatigue test) determine the degree of fatigue present at different times when such a definition is accepted?

It is clear that fatigue in the present sense cannot be directly observed. There is, therefore, only one method by which its presence and its degree at any time can be known, namely, deduction from something else that is observed. Hence, everything depends, so far as our present purpose is concerned, upon whether or not there can be found some phenomenon from which this deduction can be made; and this resolves itself into *the possibility of finding some characteristic expression of fatigue. If such an expression cannot be found, fatigue test experimentation with reference to the intact organism has no sufficiently definite problem.*

(2) The *acceptance* of a characteristic expression of fatigue in the intact organism is included in that *general* definition of fatigue most usually adopted at the present time; according to which *fatigue is a condition caused by work, in which the capacity for work is diminished.* The characteristic expression of fatigue here accepted, and explicitly stated to be such in certain forms of the definition, is *a diminished capacity for work.* We have now to consider whether this is adequate for our purpose.

As usually *conceived,* the general definition just stated requires more precision in certain minor particulars than occurs in this statement of it; and in introducing this, we are clearing the ground for a discussion of the question just raised. Two points arise concerning the term *work*:—

(*a*) The definition might be taken to mean that play, as distinct from work, could not produce fatigue. Such a conclusion would probably be denied by most of those who accept the definition: if capacity for work can be diminished by work, it seems certain that it *can* also be diminished by play. In any case, it is often difficult to determine whether a particular activity is play or work. It may be work in respect of the fact that it yields remuneration, and play so far as the performer's attitude is concerned. This ambiguity in the definition does not occur when the term *activity* is substituted for the term *work.*

(*b*) The definition does not specify the *kind* of work for which capacity is diminished in fatigue. It could be taken to mean either that capacity for *every* kind of work is diminished, or that capacity for the particular kind of work that caused it is diminished. This ambiguity is removed in that statement of the definition which explicitly limits the lowered capacity to the performance of the activity which caused it. And this keeps the definition within the body of known fact. It prevents any premature conclusion as to whether fatigue is general or specific since in both cases it would, according to current conceptions, diminish the capacity for performing the activity which caused it.

(*c*) A statement of the 'diminished capacity' definition of fatigue which would remove the above ambiguities would be as follows: *fatigue is a condition caused by activity, in which the capacity for repeating the activity that caused it is diminished.*

(3) (*a*) The serious difficulty in the 'diminished capacity' definition of fatigue now comes to light. When an attempt is made to determine (independently of a fatigue test) in what degree fatigue is present at any time, so that it can be decided whether a proposed fatigue test is a diagnostic instrument or not, it becomes clear that the expression of fatigue indicated in the definition *is not adequate for our purpose.* The meaning of 'diminished capacity' is not self-evident, though from such a statement as fatigue is *shown and measurable* by it[1], there seems to be a general opinion that it is. Various statements in fatigue literature suggest that 'diminished capacity' can be *observed directly*, and as easily as the shape of a man's nose. But if *any* degree of capacity, small or great, can ever be detected, it is not by directly observing it but by observing something else which is a mark for it—from which it can be inferred. The diminished capacity said to be characteristic of fatigue (in such a statement, for instance, as fatigue is 'shown by' it) can *only* be discovered by discovering some way in which it expresses itself. If diminished capacity is the most characteristic 'expression' of fatigue we know of, then, since it cannot be itself directly observed, we must discover some characteristic way in which *it* expresses *itself* before fatigue test experimentation becomes justifiable.

Briefly, the position is this. Since fatigue, in the present sense, cannot be directly observed, we require (before fatigue test experimentation begins) something observable (an *expression* of fatigue) from which its presence can be inferred. The *accepted* 'expression' (diminished capacity) is itself not directly observable; and, consequently, we require an observable expression of this 'expression' of fatigue, from which the presence of diminished capacity (and hence of fatigue) may be inferred.

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[1] "Real or objective fatigue is shown and is measurable by the diminished capacity for performing the act that caused it." Memo. 7, *Health of Munitions Workers Committee*, "Industrial Fatigue and its Causes," p. 5. Cd. 8213. 1916.

Now, if diminished capacity is an 'expression' of fatigue, any observable expression of diminished capacity can be considered simply as an expression of fatigue. We may therefore speak *as though any expression of diminished capacity* due to prior activity were an *expression of fatigue.*

(*b*) It is perhaps most natural to assume that 'a condition of diminished capacity' is *a condition in which output is relatively poor in quantity or quality, or both*; a definition which may be taken to cover the results of special performance tests as well as industrial output. What is meant by relatively poor output is, or can be made, perfectly clear; and *if* such output is a constant characteristic of a condition of diminished capacity, and *if* diminished capacity is a characteristic 'expression' of fatigue, we have all that is required for our purpose in searching for a fatigue test. *Assuming that these hypothetical propositions are true*, what we need to do is to find some test which yields one type of result when output is poor and another type when output is good, and other but consistent results with intermediate degrees of excellence of output.

(*c*) The supposition that relatively poor output is a characteristic expression of diminished capacity has to meet considerable difficulty. It leads us to assert the absence of fatigue in certain cases when, in common belief, it is most certainly present, and to deny its absence in other cases when, in common belief, it is most certainly not present. If these common beliefs are accepted, it must be admitted that fatigue is sometimes present when output is relatively good and sometimes absent when output is relatively bad; and this admission does not agree with the proposed characteristic expression of diminished capacity. Therefore, *either* that expression *or* these common beliefs about the presence and absence of fatigue at certain times must be given up. While these common beliefs make use of a nebulous conception of fatigue, various considerations suggest that some modification of the above characteristic expression of diminished capacity is necessary. These considerations may be grouped under two heads.

(i) Within certain limits, *output is a function of the intensity of the stimulus to activity.*

This law, called by Dodge "the first law of relative fatigue[1]," may be illustrated from the results of experiments with nerve-muscle preparation. When a stimulus of weak intensity is applied to such a preparation, the muscle contracts and continues to contract for a time with repetition of the stimulus, the excursions presently becoming smaller. Immediately after it has ceased to respond to this stimulus, it will respond with further contractions to stronger stimuli. Moreover, before the point has been reached when the weak stimulus is no longer adequate to evoke a contraction, a stronger stimulus will, within limits, increase the extent of the contractions.

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[1] R. Dodge, "The Laws of Relative Fatigue," *Psychol. Rev.* 1917, XXIV. 102.

A similar phenomenon occurs with regard to the organism as a whole and with respect to those stimuli commonly called *incentives*. The relatively small late-afternoon output of an industrial worker may suddenly become large as a consequence of an appeal to any one or several of a number of 'motives'—to fear, ambition, or patriotism. His relatively large output between 10 a.m. and 12 a.m. may suddenly become small if he be told that his employer is treating him unfairly, profiteering generally, or preparing to cut piece rates. The whole question of systems of wages, so far as they are incentives to work, is here relevant. The phenomena of spurts manifested in continuous work possibly arise from variations in the intensity of incentives from one moment to another, especially if variations in spontaneous *interest* are included, as they should be, among incentives to work[1].

(ii) Within certain limits, *the character of output depends upon the intensity of stimuli to other kinds of activity than that which produces it.*

Illustrations of this fact can be found in the results of experiments upon 'competing' systems of reflex arcs; but it will probably suffice if there are noted here certain experiences of a well-known character.

Even with a superabundance of 'capacity,' it is often difficult to concentrate attention upon some particular work, and at such times output is relatively poor compared with other times when concentration of attention is easy. Irrelevant matters come into the mind, causing distraction; and the occurrence of such distraction is at the expense of both the quantity and the quality of output. It is simply contrary to facts to say that the only time that such distractions occur is when we are fatigued—when 'capacity is diminished.' While they do occur at such times, they occur also, almost universally, at the beginning of a period of work; and their gradual elimination seems what in part is meant by 'warming up,' though doubtless this has a more strictly physiological reference also. To adjust the organism to the task of concentrating attention and activity upon certain 'work' for several hours, necessitates the inhibition of associations which recent experience has created and which involve tendencies to action different from those involved in the particular work. The interference thus occasioned between competing systems may render output relatively poor, and such output may occur *at any time* as a result of such interference. Probably the feelings of weariness, with their positive tendency to cause a general relaxation of effort, are important here; but by no means these only.

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[1] It needs to be emphasized that the operation of mental stimuli is often extremely subtle. The following statement by R. A. Spaeth is of some interest here. Speaking of ergographic work, he says: "If a man has been raising five or eight pounds for some time, he reaches a point where he can't work any longer, but if we remove a pound, the individual finds he can go on. We also find, that when certain individuals are told, 'We are now removing a pound,' but actually we add a pound instead of taking one off, the individual starts merrily on and proceeds to work....The thing I want to bring out is that the suggestion of work and the idea that one is capable of work plays a large part in actual performance. The reverse is quite true as demonstrated by the fact that if I tell the individual that I have added a weight and really remove one, he says he cannot go any further....We have repeatedly carried out this experiment by way of demonstrating that the working capacity is not a fixed base line." *Industrial Education*, p. 204. (The Society of Industrial Engineers, New York, 1920.)

The individual normally experiences impulses of a more or less instinctive nature: and the interference of these with work stimuli will cause output to deteriorate. Up to a certain point, suppressing impulses renders them more insistent. In modern industrial conditions, the worker stands in an environment highly artificial; and after tending a machine requiring constant attention for hours together, he will feel an ever-growing desire to go somewhere and to talk to his fellows. When such desires are strong, they distract attention from work and lower output. The point is, briefly, that such interferences may and sometimes do occur when, according to common belief, fatigue is not present. We know broadly the effects of 'competing stimuli,' and this enables us to say with certainty that relatively poor output is not universally the expression of diminished capacity. It may be due in a specific case to the occurrence of a strong stimulus to a rival form of activity; and such a stimulus may arise from within the organism—it may be an 'impulse.'

(iii) Since, then, output is relative both to its own stimulus and to that of activity incompatible with it, there cannot be an exact correlation between the condition of the organism and the character of output—such as would enable us to say that one particular type of output universally indicates one condition of the organism. The incentives to work and the general organic condition of the worker vary independently of each other: the greatest incentive may be experienced when the organism is least vigorous, and the least incentive when it is most vigorous; and the greatest incentive acting upon diminished capacity may yield an output equal to or better than a very small incentive acting upon great capacity[1]. Output, in such a case as the former, would *not* show fatigue, although fatigue would be present; and, on the other hand, in the absence of fatigue output may be relatively poor because the incentives to some activity incompatible with that required for its production are very strong.

(*d*) The statement that the condition of diminished capacity expresses itself in relatively poor output must therefore be altered by substituting *tends to express* for *expresses* itself. This will mean that it expresses itself in this way *in the absence of interfering factors.*

But a further alteration, indicating that there is a tendency for diminution in capacity and poorness of output to vary directly, must also be made. This is necessary, first, because it is in accord with the results of physiological experiments on fatigue, and, secondly, because we must know that different *degrees* of fatigue are present at certain times before a fatigue test can be established. We shall then have to say: *diminished capacity* is a condition which *tends* to express itself in relatively poor output, the degree of poorness *tending* to vary directly with diminution in capacity. So far as fatigue is concerned, it would be necessary to show in any case that diminished capacity was due to prior activity.

(4) Having seen how the commonly accepted expression of diminished capacity must be modified if from its presence diminished capacity may be inferred, we must consider one other fundamental question. *Is it justifiable to accept diminished capacity as a characteristic expression of fatigue?* So far an affirmative answer to this question has been *assumed.*

There seems no ground for questioning this procedure so long as attention is confined to fatigue in the relatively isolated muscle. A characteristic feature of fatigue in muscle or nerve-muscle preparation is the gradual deterioration of muscle output. A broadly similar result is obtained from ergographic work with a relatively isolated group of muscles, with regard to which, it may be supposed, there occurs a gradual accumulation of metabolites and some blocking of impulse paths. In such cases as these, in which interfering factors can be largely or altogether controlled, diminished capacity (as inferred from its expression) appears to be a constant 'expression' of fatigue. The acceptance of diminished capacity as a characteristic 'expression' of fatigue in the intact organism is an inference from these phenomena. We are unable to say if it is altogether justifiable; but it seems reasonable, provided *its* expression be modified to allow for interfering factors in the manner already outlined.

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[1] The case in which excellent output is obtained in 'serious fatigue' should also be mentioned—whether this be due to the 'failure of higher control' or to other causes. Cf. C. S. Myers, *Mind and Work,* 1921, 46.

It will be observed that no attempt has been made to define the intrinsic nature of *diminished capacity*. The situation is such that no definition of this term is required. In fact, since (1) diminished capacity is itself not directly observable, since (2) it 'expresses' something else (fatigue) also not directly observable, and since (3) in the absence of interfering factors it is itself expressed by something that *is* directly observable, *it may be altogether eliminated from fatigue discussion* which can proceed just as well in terms of *fatigue* and *output*. It would be an aid to clarity of thought if this were done, as, in its present status, 'diminished capacity' is nothing more than an algebraical symbol, and may conceal ambiguity or carry unjustified conclusions.

The definition of *fatigue* would then be as follows: *Fatigue is a condition* (partly[1] specifiable by reference to accumulation of metabolites and blocking in impulse paths) *caused by activity, in which the output produced by that activity tends to be relatively poor; and the degree of fatigue tends to vary directly with poorness of output.*

(5) The experimental situation that now results is far from ideal; but it is possible. We do not know of any absolute constant characteristic expression of fatigue: the most constant expression we know of is evoked sometimes by conditions in which fatigue is not present, while at other times certain factors interfere with its occurrence and thus mask the presence of fatigue.

The possibility of finding a fatigue test thus depends upon the possibility of eliminating or of determining the precise effects of all interfering factors (among which are included factors tending to produce the accepted characteristic expression of fatigue in the absence of the fatigued condition). Among these factors are (*a*) incitement, (*b*) practice, (*c*) spurts, (*d*) illness, (*e*) diurnal rhythm, (*f*) incentives to a given kind of activity, and (*g*) competing incentives. It seems possible to control the first five of these, but impossible to control the two last. Whether it is possible to determine the precise effects upon output of ineliminable interfering factors, is a highly problematical question. The mere discovery that

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[1] A considerable step forward would be made by the exact determination of the *intrinsic* nature of fatigue. "Fatigue is undefined as a physiological entity" (C. K. Drinker, *J. of Industr. Hyg.* 1920, II. 114–115). It is undefined, intrinsically, in *any* terms.

any factor influences output is very important here; and the elimination or determination of the precise effects of each such factor is in itself a considerable experimental problem. This work, however, *must precede* the application of proposed fatigue tests: that is, *the first requirement is a* technique *for determining independently of a proposed test in what degree fatigue is present at certain times.* For the carrying out of this work, the interfering factors must be under the control of the experimenter, and this condition cannot generally be realised in industry. Once this work has been done, the next question concerns the actual fatigue tests to be used.

III. PROPOSED FATIGUE TESTS.

(1) From the points of view of the criticisms which may be urged against them, and the *technique* required for their application, proposed fatigue tests may be conveniently classified under two main heads—*performance* tests and *non-performance* tests.

A *performance test* may be defined as one that requires a subject to do something, the character of what is done being regarded as a possible indicator of fatigue. Thus, an ergographic test is a performance test. A *non-performance test* is one that is directed to the measurement of certain non-voluntary phenomena, the character of which is regarded as a possible indicator of fatigue. Thus, the measurement of blood-pressure is a non-performance test.

Most proposed tests of fatigue are performance tests.

(2) *Performance tests* may be classified, first, as *workshop* tests and *laboratory* tests.

There is one *workshop* performance test, namely the character (quantity and quality) of industrial output.

Of *laboratory* performance tests there is an indefinite number, and these may be classified in various ways. Perhaps the most convenient, if rough, method of classification is by reference to that part of the organism whose performance they seem specially designed to measure. By this method we first have (*a*) *muscular* tests, and (*b*) *mental* tests. These may be again subdivided as follows:

(*a*) *Muscular tests:*

  (i) *Muscular strength tests* (including *endurance* tests); *e.g.*, dynamometer tests, ergograph tests, tapping;

  (ii) *Muscular precision tests, e.g.*, the steadiness test, 'pursuit pendulum[1],' dart-throwing;

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[1] See W. R. Miles, *Psychol. Rev.* 1920, XXVII. 361–376.

(iii) *Muscular rapidity tests, e.g.*, tapping.

(*b*) *Mental tests:*

(i) *Algometer tests;*
(ii) *Tests of sensory acuity;*
(iii) *Tests of immediate memory;*
(iv) *Tests of attention;*
(v) *Tests of apprehension;*
(vi) *Tests of judgment and reasoning.*

In addition, there is one test, at least, which does not readily fall under any of these heads, namely, *the reaction-time test.*

An indefinite number of tests may be devised under each of the above sub-headings.

(3) Proposed *non-performance* tests of fatigue may also be classified as *workshop* and *laboratory* tests.

The only possible *workshop* non-performance tests are two, *industrial accidents* and *consumption of power. Industrial absenteeism, sickness* and *labour turnover* have also been suggested as possible tests; but while these phenomena are almost certainly influenced by fatigue, the other factors which may influence them are so many and potent that it is impossible to consider them seriously as *tests.*

The number of proposed *laboratory* non-performance tests is small. The chief that have been suggested are the following eight:

(*a*) Variations in blood-pressure;
(*b*) ,, ,, the pulse;
(*c*) ,, ,, the 'skin-reaction'[1];
(*d*) ,, ,, physiological reflexes;
(*e*) ,, ,, psycho-galvanic reactions;
(*f*) ,, ,, excretions;
(*g*) ,, ,, muscle-tonus;
(*h*) Calorimetry.

(4) The question now to be considered is whether some of these tests are more promising than others.

(*a*) *Performance tests.* Certain general considerations necessitate the surrender of *all* performance tests as possible tests of fatigue. They are not, as a rule, convenient tests; and none of them can yield results that do not require interpretation and correction for interfering factors. This may be made clear briefly.

All performance tests are affected by the factors already shown to interfere with the accepted characteristic expression of fatigue. So far as these tests are concerned, therefore, these factors are interfering factors, whose influence must be eliminated before a result can indicate fatigue unambiguously. Such an elimination seems possible, though difficult, when the performance test is continuous over a long period, or even if, in itself occupying a short period, it can be given to groups or individuals many times; but it is possible neither to eliminate all these factors nor to determine their influence at any one application of the test, and especially at its first application. A convenient and rapid test (and the idea of a test includes rapidity) would be one which need be applied to an individual or a group on a few occasions only in order to indicate the degree of fatigue present. But it takes time to eliminate the influence of practice or warming up or spurts; and it is impossible to determine the effects of fluctuations in incentives to activity, which may altogether mask the presence of fatigue, in a period of short duration—if indeed, it is possible in such a period to know that they occur. Spontaneous fluctuations in interest, for instance, or in the desire to do one's best, to mention no other factors, are entirely outside the control of the experimenter and most largely remain outside his knowledge: the most that can be hoped is that their influence may be isolated in the results of performance tests carried out for a long period. But this means that such tests cannot be rapid tests. This conclusion gains confirmation from what is commonly called the 'inconsistency' of the experimental results obtained from the application of performance tests.

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[1] See footnote, p. 32.

Performance tests, therefore, must be altogether given up if experimentation with them is undertaken in the hope of finding among them a *rapid and convenient* fatigue test. If used for other purposes, however, they may yield valuable results. For it is only by means of performances that it can be known, in the first instance, in what degree, if any, fatigue is present at certain times. Hence, experimental work with non-performance tests *must* be combined with a consideration of performance results (in order that the worth of the non-performance tests may be rightly judged). For this purpose almost any kind of performance should be valuable. In practice, the reliability of a proposed non-performance test would need to be tested by reference to the results of various types of performance.

(*b*) *Non-performance tests.* Certain of those factors previously designated *interfering* factors in the case of *performance* tests are not interfering factors in the case of *non-performance* tests. The results of a *non-performance* test are practically unaffected by practice, for instance. It might therefore seem that there is considerable probability of obtaining a rapid and convenient fatigue test of a non-performance type. Two points, however, should be noted.

First, there might in any case be no rapid and convenient fatigue test, because no uniquely characteristic mark of the fatigued condition might be brought to light by any non-performance test.

Secondly, it is a mistake to suppose that the results of non-performance tests are unaffected by factors other than fatigue. Sickness may interfere with the result of a non-performance test just as much as with the result of a performance test. Accidents may be caused by factors among which fatigue has no place. All circulatory tests (the first three of the laboratory non-performance tests) are directly influenced by emotions, and it is impossible for the experimenter to control these, or to eliminate their influence except in results covering long periods; which means that circulatory tests cannot be rapid tests unless we possess also a rapid test of the presence of emotions, and a knowledge of their circulatory effects which could then (theoretically) be allowed for. Again, although experimentation with psycho-galvanic reactions is in its infancy, it shows that 'emotive reactions' are influenced by other factors in addition to fatigue, if fatigue *has* a characteristic effect upon them.

Concerning the other proposed non-performance tests, all that need be said here is that such facts as are known concerning the nature of excretions and calorimetry suggest that these will yield no test of fatigue; that at present there is no satisfactory method of measuring muscle tonus; and that the possibilities of physiological reflexes as a test are not clear.

(5) It is worth noting that the mere demonstration of the presence of fatigue at a given time has no practical significance whatever. The practical question asks at what point fatigue becomes inimical to efficiency and to health. Consequently, if a test is to be valuable practically, it must be capable of discriminating nicely between degrees of fatigue. When such a test is obtained, extensive inquiries would need to be undertaken to determine the relation between these different degrees of fatigue, on the one hand, and efficiency and health, on the other hand. It might then be possible to say, first, that such and such a degree of fatigue (or a greater degree) is bad; and, secondly, that we have a means of knowing when any degree of fatigue inimical to health or efficiency is present. If we cannot finally make such statements, our fatigue tests will have no *practical* value whatever.

IV. Conclusions and Suggestions.

(1) An essential pre-condition of experimentation designed to obtain a fatigue test is the knowledge that different degrees of fatigue are present at certain times. This knowledge can probably be obtained *if* it be accepted that fatigue tends to express itself in relatively poor output. It would then be necessary to develop a *technique* such that by it either (*a*) factors which interfere with this characteristic expression of fatigue can be eliminated, or (*b*) the presence of interfering factors can be known and their effects upon output precisely determined.

If this *technique* could be obtained we should possess *in it* a fatigue test[1], that is, a method by which it could be shown in what degree, if any, fatigue was present at given times, but we should not have a rapid and convenient fatigue test.

(2) Given any method by which we can determine in what degree, if any, fatigue is present at certain times, it is justifiable to experiment with the object of finding a rapid and convenient fatigue test. We should proceed largely by the method of trial and error, our object being to find *some* test that would rapidly yield at any time a characteristic result if a given degree of fatigue were present. The only suggested tests that could possibly yield such a result are non-performance tests; and consequently experiment aiming to discover a rapid test of fatigue must work with such tests. If such a test were found, the degree of fatigue present at any time could be determined without a lengthy and difficult analysis of output figures.

(3) In view of the foregoing considerations it is recommended that the whole fatigue test problem be stated in a form the nature of which may be indicated by the following suggestions:

(*a*) That the term *fatigue* be absolutely banished from precise scientific discussion, and consequently that attempts to obtain a fatigue *test* be abandoned.

(*b*) That the problem to be investigated be defined as the determination of the effects of different kinds and amounts of work (activity) upon mental and physiological functions: that is, that the kind and amount of work be correlated *directly* with changes in psycho-physiological functions, and *not* (as at present) *indirectly* by means of 'fatigue.' The various proposed fatigue 'tests' would then be used to determine *effects of activity* and not *the presence or absence of fatigue.* Thus stated, 'fatigue investigation' seems to offer a fertile field for scientific work.

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[1] This is only true *on the assumption*, stated under II. (4), that diminished capacity (expressed in relatively poor output) is a characteristic expression of fatigue. Without this assumption we are left with only one series—that constituted by output of different degrees of excellence. It is only by assuming that such a series runs parallel with fatigue that we obtain the two series, a knowledge of which was stated in I. (*b*) to be essential.

(*c*) That, so far as practical purposes are concerned, attention should be concentrated on methods, such as motion study, by which the amount of *work* required for a given quantity of output can be decreased.

A PRELIMINARY STUDY OF THE REPRODUCTION OF HAND MOVEMENTS.

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(*From the Cambridge Psychological Laboratory.*)

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2. *Apparatus* (p. 47).
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§ 1. THE PROBLEM.

THE general purpose of the experiments, the results of which will here be briefly discussed, was to determine the most favourable conditions for the learning and reproduction of hand movements. The practical bearing of this problem upon various educational and industrial questions is obvious, while it also raises interesting theoretical points, such as the relation between visual and motor perception of space, or between visual and motor imagery.

Six subjects were employed, five men and one woman, all of whom were of adult age. Each subject executed the prescribed movement in a manner which will be described later, and after a lapse of five minutes he attempted his reproduction of the movement. This was the procedure adopted at the first sitting. In subsequent sittings, if further practice in the execution of the movements was required, the same course was adopted. The total time occupied at any given sitting was not more than one hour, this limit being imposed so as to avoid fatigue effects.

I wish at the outset to thank Mr F. C. Bartlett, who proposed the subject of this investigation and helped to prepare this paper for publication.

§ 2. APPARATUS.

The apparatus used consisted of a metal sheet 47 cm. × 39 cm. divided into 20 7½ cm. squares, the sides of these squares being made by lines of sealing-wax let into the metal sheet, and level with its surface. These sealing-wax boundaries were broken at the corners of the squares, so that no portion of the metal sheet was completely isolated. The whole sheet was placed in circuit with a metal pointer, a steel pen, and two electric time signals, and the latter were made to register, in the usual way, upon a travelling paper surface, below a Jacquet chronometer. Simple curved patterns were cut out of sheets of thin wood, which were then placed over the metal background. The subject traced the pattern with the metal pointer, following the path cut in the wooden model. The model was then removed, and after the prescribed interval, the subject attempted, with the steel pen, to reproduce the movement on the same metal background. A copy was taken on tracing paper of his attempt, the whole sponged off, and the process repeated. The passing of the pointer or pen over a line of sealing-wax caused a break in the current flowing through the time-signal, and thus the total time occupied by a complete execution or reproduction could be readily computed, and also the time taken by various sections of the movement.

§ 3. METHOD.

The general plan was to vary, one at a time, different specified conditions of learning and of reproduction. Thus the following conditions were imposed:

Learning		Reproduction		
L. hand or R. hand	eyes closed	eyes closed	L. hand	R. hand
		eyes open	,,	,,
R. hand or L. hand	eyes open	eyes open	,,	,,
		eyes closed	,,	,,

Again the subject learned by watching the experimenter perform the movement, his reproductions being effected in the various modes specified above. Or in some cases the effect of previous practice was observed by instructing the subject to draw any meaningless pattern he pleased. Some attempt also was made to study the effect of eye movement, but this was not very successful, and will not be further discussed. In every case a reproduction was attempted after *each* practice movement.

The general procedure was as follows:—

(*a*) the subject executed the movement by the help of the pattern-board, being allowed always to do so at his own rate of speed;

(*b*) an interval of five minutes was imposed, this interval being filled by some other task, or by conversation;

(*c*) the subject attempted his reproduction in the prescribed manner.

Five separate executions and reproductions completed a single sitting, and the sittings were continued with each subject for each pattern employed until the reproduction was accurate. The total number of sittings required by different subjects varied considerably.

§ 4. RESULTS.

No attempt will be made to discuss exhaustively the results obtained. It is recognised that further subjects and more experimentation are needed. A number of the conclusions suggested however seem to be of general interest, and these will be briefly indicated:

(i) In the early stages of practice all of the subjects attended to the *form* of a movement rather than to its *extent*. Thus one who, after a series of 15 trials, each trial followed by a reproduction, was asked if he clearly recalled the drawing, replied "I know its form well enough, but not its size." The inability to remember extent during the early stages of a reproduced movement is most marked of all, when the first execution is made with the eyes closed. There seems to be no doubt that the normal mode of acquisition of a new 'meaningless' hand movement is first to learn the form, and only afterwards the size. Satisfaction on the part of the subject with his reproduction came only when the size was being learned; and it was often noticeable that, although the subject was very dissatisfied with his attempt, his reproduction as regards the details of form was fairly accurate.

(ii) The points of movement most speedily and accurately learned were the beginning and end (as in the case of a series of nonsense syllables), and wherever sharp changes of direction occurred.

(iii) If the eyes were closed during learning, or reproduction, or both, the acquisition of the movement was retarded. In particular, subjects who reproduced with closed eyes very readily interchanged the dimensions of two curves in sequence, or the directions of symmetrical curves. These errors were especially liable to occur at the beginning of the movement.

(iv) Different forms of hand movement were very readily coalesced, or 'condensed.' Thus when a subject had been practised on a certain movement, and was then set to reproduce a different one, the result was invariably at first a combination of the two. Again when a subject, practised in a certain form of movement, was told to draw haphazard meaningless patterns differing from one another as widely as possible, it was astonishing to observe the general likeness of his attempts to the practised movement.

(v) When, in the original execution, the subject's hand was guided by the experimenter, the process of learning was very distinctly retarded.

(vi) The rate of learning was not affected according as the right or left hand was used. Moreover if practice was given to one hand, nevertheless reproductions could be equally well effected by the other hand; and, as regards their general form at least, reproductions could be correctly effected also by other parts of the body, *e.g.* the head.

(vii) Individuals differed as to their relative abilities in reproduction with eyes open or closed. But the influence of this factor was independent of the method of learning.

(viii) When a number of similar curves followed one another, the reproductions were several times in error concerning the precise number of curves reproduced. Errors of this character proved to be unusually difficult to eradicate, and often persisted even when practice was given with sight.

(ix) The most favourable method for the teaching of movements of the kind here studied, was for the experimenter to make the tracing and for the subject to watch him. The results were quite unequivocal, and would appear to have important general bearings upon practice; but further observations on different types of movement are required. As already mentioned, subjects showed consistent individual differences as to the relative accuracy of reproduction with eyes open or closed. Making allowance for this, the methods of practice may be arranged in the following order, as regards their efficiency:

		Movements reproduced	
	Movements learned by	by some subjects with	by other subjects with
1.	watching them done by another	eyes open	eyes closed
2.	performing them with eyes open	,, open	,, closed
3.	watching them done by another	,, closed	,, open
4.	performing them with eyes open	,, closed	,, open
5.	,, ,, ,, closed	,, open	,, closed
6.	,, ,, ,, closed	,, closed	,, open

(x) As regards the time of the reproduction in relation to the time of a practice movement, the subjects gave consistent results as follows:

Form of practice	Time spent in reproduction
With eyes closed	Less than in practice
Watching movement performed by another	Variable: very little difference
With eyes open	More than in practice

(xi) Movements learned and reproduced with the eyes closed showed a striking diminution in the size of the reproductions. If the eyes were allowed to be open during one only of the two periods of practice and reproduction, the size was still commonly diminished, but less markedly so. When the subject watched the movements as they were performed by another and then reproduced them with his eyes shut, the reproduction was still of a smaller size, but in a slight degree only. Again, when a subject was allowed to learn a movement thoroughly by dint of much practice, and was then asked for two reproductions, one with his eyes closed and the other with his eyes open, the two attempts were alike as to form, but the first was invariably smaller than the second. Further experiments should be carried out with regard to the judgment of distance or size based upon motor as compared with visual experience.

§ 5. GENERAL SUGGESTIONS.

These results appear to open up a large field of study and to justify a few general suggestions.

(*a*) Clearly the adult subjects employed in these experiments brought to their task a variety of preformed habits of observation. This makes it hard to state exactly what were the main bases of their attempts at reproduction of the movements. Not only were there muscle-joint-tendon sensations, and sensations of cutaneous origin, but it is certain that visual and verbal images were often of the greatest use. The very fact, for example, that movements learnt with the left hand appeared equally well traceable with the right suggests that some spatial scheme expressed in terms other than those of local motor sensations must have been used. It would be interesting to know whether equal facility of transference would be found in subjects blind from birth. Similarly a study should be made of subjects who have become anaesthetic as regards motor and cutaneous sensation, and experiments should be specially devised to test the possible effects of eye movement.

(*b*) A matter of the greatest practical interest is that of the basis of the superiority of watching a movement over merely performing it as a method of teaching. Is this equally great for all types of subject, and for all kinds of movement? It may be the case that the superiority is maintained only when the watched movement is performed so slowly that its analysis is easy.

(*c*) The diminution of size which marked all reproductions carried out without the aid of vision is a matter which clearly calls for further study. That the results of the present experiment were not merely accidental would appear to be certain from the fact that in recent observations carried out on children's reproductions of simple forms exactly the same diminution of size was practically invariably secured.

It may be that in teaching any new form of skilled movement the most important factor for the instructor to lay stress upon is the *extent* of the movement.

(*d*) It may be that in all reproductions of movement carried out upon a basis of motor experience only, 'points of reference' are developed which are in the main determined by changes in direction in the complete pattern of movement in question. Thus, in the present case, when several successive attempted repetitions of the same movement were traced on the same background, it was very noticeable how the whole of the attempts tended to come together at certain points. These points seem to have the function of common 'points of reference' between which successive movements may diverge considerably, but at which all attempts join. Such 'points of reference' may be regarded, perhaps, as sections of a complete movement which are unusually readily established. They seem to bear a definite relationship to changes of direction of a curving movement. Thus they may all be regarded in a way as starting-points, and the ready establishment of starting-points for 'muscular memory' seems to yield a perfect analogy with the favourable position of first elements in a series for 'verbal memory.'

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SUGGESTIBILITY WITH AND WITHOUT PRESTIGE IN CHILDREN.

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I.

THE first of the researches upon which the following paper is based was carried out, by the first of the above authors, after some preliminary experiments with the tests, in the autumn term of 1915. The subjects were school children: 27 boys (average age 13) in one school (*A*); and 19 boys (average age 12), and 19 girls (average age 12) in another (*B*).

It was then intended to prolong the experiments with a greater number of subjects, but circumstances made this impracticable at the time; and it was found impossible to continue the research up to the period at which the second investigation was begun. The protocols, however, though few in number, have yielded results which appear to be worth consideration; and have been notably corroborated by the second research. Such evidence as is forthcoming seems to point to a factor of general suggestibility, rather than to specific suggestibilities, as previously believed.

TESTS: Several tests of suggestibility were chosen, of which some had been used in previous work. Seven tests in all were employed. Four only were used with the subjects in school *A*, but all seven with those in school *B*. The following were the tests:

1. Hand Rigidity by Suggestion.
2. Suggestion by Progressive Lines[1].
3. Suggestion by Illusion of Warmth[2].
4. Hand Levitation by Suggestion.
5. Suggestion by Progressive Weights[3].
6. Fidelity of Report Method "to induce a strong degree of suggestion[4]."
7. Suggestion of Line Lengths by Personal Influence; A. Contradictory Suggestion[5].

It will be perceived that these tests fall into two classes: (i) tests without personal prestige, or 'depersonalised' suggestibility tests: and (ii) tests with personal prestige derived from the experimenter. Nos. 2, 3 and 5 would seem to be of the former, Nos. 1, 4, 6 and 7 of the latter class; but results obtained in both researches have led the authors to a modification of this classification. This point will be considered in Section III.

1. *Description of the tests, with frequency distribution tables.*

Test 1. Hand Rigidity by Suggestion.

This test was used with subjects of both schools *A* and *B*.

Method. The subject was seated opposite the experimenter and given the following instruction: "Hold out your hand—so, like mine; and fix your eyes on my hand. Whatever I say, do not take your eyes off my hand. But, while looking at my hand all the time, I want you to pay special attention to the *feelings* you get in your own. Do you understand? You are to look at my hand all the time. Don't look at your own, or at me; but pay attention to the 'feel' of your own hand." During this general instruction the experimenter held out his hand, palm uppermost, easily and loosely, with the fingers very slightly flexed, in a position some four inches above his right knee, so that the subject could easily fixate it. The instruction then continued: "You will probably feel a slight tingling, or tightening, or stiffening in your fingers. I want you to tell me *at once* when you feel this." The moment any such feeling was reported, the stop-watch was started; and the experimenter, at the same time deliberately straightening out his fingers, and very slowly making his hand stiff and rigid, continued: "Now! You will find that tingling and stiffening increase, so that your hand becomes *quite* stiff; so that you can't bend your fingers at all; or close your hand. Do you feel it? It's getting much stiffer now—quite stiff and rigid. Now you *can't* move your fingers. You can't *close* your hand. You can't close it at all. Try!"

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[1] Whipple, *Manual of Mental and Physical Tests*, Test 42.

[2] "Resistance-wire Method," Whipple, *ibid.*, Test 44.

[3] Whipple, *ibid.*, Test 41.

[4] Whipple, *ibid.*, Test 32, Variations of Method (3).

[5] Whipple, *ibid.*, Test 43.

This test was performed once with each child of the two schools *A* and *B*. The results were estimated (i) as to the success in inducing the rigidity, (ii) as to the time interval required for the effect. With regard to the first head, the subjects were graded, in order of suggestibility, in one of the following classes:

(i) Subject tries, but cannot close his hand at all or flex his fingers.
(ii) Subject cannot close his hand, but is able to move his fingers slightly (flexing or lateral movement).
(iii) Subject cannot close his hand, but is able to move his fingers considerably.
(iv) Subject cannot close his hand completely, but can close it partly.
(v) Subject can close his hand, but only slowly and with evident effort.
(vi) Subject reports that his hand is stiff (sometimes that he is unable to close it) but actually does close it without apparent effort.
(vii) Subject closes his hand easily, with little report of stiffness.
(viii) Subject closes his hand easily, with no reported feelings.

The character and extent of the effect of the suggestion would appear in general to be a better criterion of suggestibility, especially in this test (and in No. 4), than the length of the time interval, which would presumably measure rather quickness of accepting the suggestion than suggestibility. A long time interval might be present, for instance, in a subject slow to understand the instruction or to react. The command "Try!" (to close your hand) was given when the experimenter believed, from external signs, that the suggestion had been taken. The first class, however, was divided into two on the ground of length of time interval—under 30 or over 30 seconds. This procedure was justified by the fact that, in all other circumstances, the cases were, as far as could be estimated, entirely similar. We thus get a grading of nine classes for this test. As it was only performed once with each subject, there was on possibility of obtaining a reliability coefficient.

The frequency distribution for the subjects is as follows:

Table 1.

	Class...	1	2	3	4	5	6	7	8	9
B.	Boys	6	3	1	.	2	.	7	.	.
	Girls	2	1	1	.	3	1	9	1	1
A.	Boys	.	7	.	.	6	.	4	.	10
	Total	8	11	2	.	11	1	20	1	11

This table is interesting in showing how the cases tend to be grouped at each end of the scale. If we divide the subjects into (i) those who can, and (ii) those who cannot, close their hand, we get the following proportion: 46 per cent. Boys, 63 per cent. Girls, Total 51 per cent., who can close their hand; and 54 per cent. Boys, 37 per cent. Girls, Total 49 per cent., who cannot.

Test 2. Suggestion by Progressive Lines.

This test was used only with subjects of school *B*. The 'yields' to suggestion—in the shape of a judgment of increase of length of lines after the fifth—were counted as one each. For each line pronounced 'shorter' than the preceding one, one-half was deducted. Judgments of equality were ignored. This test was only applied once; and subjects were graded in rank according to the marks obtained. The total number of possible suggestions (increases) was fifteen. The frequency distribution for the 38 subjects is as follows:

Table 2.

'Longer' judgments = +1[1]
'Shorter' „ = $-\frac{1}{2}$

Marks	15		14		13		12		11		10		9		8		7		6		5		4		3		2		1		0
Boys	.	.	.	.	.	.	.	.	.	1	1	.	.	.	1	1	1	.	1	1	4	3	2	2	.	.	.	.	.	1	.
Girls	.	.	.	.	.	.	.	.	.	.	.	.	.	.	1	1	1	.	3	1	2	3	2	1	.	1	2	.	.	.	1
Total										1	1				2	2	2		4	2	6	6	4	3		1	2			1	1

Test 3. Suggestion by Illusion of Warmth ("Resistance Wire Method").

This test was used with all the subjects of schools *A* and *B*. The apparatus was as described by Whipple ('Special Warmth Tester'), with the exception that the lamps were small ones (6 c.p.) lighted by eight-volt accumulators conspicuously exhibited with prominent wiring upon the table supporting the apparatus. The test was performed ten times with each subject alternately, five times with the right and five times with the left hand. Reaction times were taken with the stop-watch. All subjects were graded in rank for (i) the right hand, and (ii) the left hand, according to the average times taken for warmth to be perceived. (All cases where objective warmth had to be given were reckoned as 60 seconds.) The reliability coefficients were then ascertained[1] (school *A*, right *vs.* left hand $r = \cdot 96$; school *B*, right *vs.* left hand $r = \cdot 96$). The reaction times for both hands were then pooled and averaged; and the subjects were again graded in ranks for correlation with their ranks gained in other tests. A distribution for all amounts of time from 0″ to 60″ (beyond which failure was reckoned) was possible. For present purposes of comparison, set out in 5″ intervals, the frequency for the 65 subjects is given in the following table:

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[1] The subtraction of ½ for 'shorter' judgments is arbitrary. The effect of subtracting 'one' is to leave the form of distribution the same, but to reduce slightly the number of marks obtained, and to give a few subjects 0 or – marks. The same applies to the other tests where ½ has been similarly deducted.

Table 3.

Time (secs.)	0–5	5–10	10–15	15–20	20–25	25–30	30–35	35–40	40–45	45–50	50–55	55–60	60
B. Boys	5	5	4	0	0	0	2	1	0	0	0	1	1
Girls	5	5	2	3	1	1	0	0	0	0	0	0	2
A. Boys	5	3	2	0	3	1	0	0	1	1	0	4	7
Total	15	13	8	3	4	2	2	1	1	1	0	5	10

If it is taken into consideration that the long reaction time averages of this Table (in the neighbourhood of 60″) are calculated from data in which failures predominate, while one or two reactions of nearly 60″ only were obtained, the distribution will be seen to be very similar in character to that shown in Table 1.

Test 4. Hand Levitation by Suggestion.

This test was only applied to the subjects in school *B*. It was of a similar general character to test No. 1. A preliminary instruction was given as follows: "Place your hand on the table so." (Subject, facing experimenter, placed his right hand, palm down, upon the table.) "I am going to lay my hand lightly on top of yours; and I want you to pay very special attention to the feelings you get from your hand. You need not tell me anything about them unless I ask you. I just want you to get the feel of them." The experimenter then laid his hand firmly but lightly upon that of the subject, so that the tips of his fingers rested upon its back near the wrist.*

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[1] Formula: $r' = 1 - \frac{6\Sigma(2d^2)}{4n(n-1)}$. Cf. Spearman, "Footrule for measuring correlation," This *Journal*, II. 89 *sq.*

He continued the instruction: "You will feel your hand growing lighter, as if it was losing weight, and not resting on the table. It gets lighter still now, as if it had no weight at all. Do you feel it?" If the subject answered "No," the instruction was continued in the same sense; if "Yes" the experimenter almost imperceptibly raised his hand, relaxing the very slight pressure and at the same time saying: "Good! It is quite light now. It has no weight at all. It is leaving the table altogether—coming right off it and rising in the air." Reaction times were taken with the stop-watch, from the beginning of the instruction after *.

This test was performed twice, once with the subject standing (in which case it was suspected that a certain amount of pressure by the hand upon the table might have interfered with the success of the suggestion), and once sitting, with the hand and arm to the elbow lying loosely upon the table. The subjects were graded in order of suggestibility for each performance into four classes according to the following criteria:

1. Hand completely off the table (an inch or so), pushing experimenter's hand up: Evaluation = + 2.
2. Hand just off the table (less than $\frac{1}{2}$ inch): Evaluation = + 1.
3. Phalanges much flexed from the table, but finger tips (or some) and/or base of hand at wrist still touching table: Evaluation = − 1.
4. Hand still entirely resting on table: Evaluation = − 2.

Reaction times were not used for purposes of classing the subjects. The reliability of the two divisions of the test was ascertained. It was: for the Boys, $r = \cdot 64$; for the Girls, $r = \cdot 99$ (combined $r = \cdot 81$). The results were then pooled and a fresh grading in rank made for purposes of comparison with the other tests. All the marks gained were added for the final ranking, thus providing a possible nine classes. The suggestibility estimated as above shows the following frequency distribution for the 38 subjects:

Table 4.

Class	1	2	3	4	5	6	7	8	9
Boys	5	.	.	1	1	2	.	6	4
Girls	6	3	1	.	3	.	3	3	.
Total	11	3	1	1	4	2	3	9	4

The distribution here is seen to take a form similar to that in tests Nos. 1 and 2. Dividing the subjects into two groups (i) those whose hands rose from the table, and (ii) those whose hands did not so rise, we obtain:

Table 5.

	Hands off table	Hands not off table
Boys	26 %	74 %
Girls	52 %	48 %
Boys and Girls	40 %	60 %

Test 5. Suggestion by Progressive Weights.

This test was performed with all the subjects of the two schools *A* and *B*, as well as with the two variations of method noted in Whipple. The yields to suggestion ('heavier' judgments, after the first four) were counted one each, and for each 'lighter' judgment one-half was deducted. Judgments of equality were not counted either way. According to the value of his performance, each subject was graded in his rank for each variation of the test, and reliability coefficients were ascertained between the three markings. These are: School *A*, Method 1 with Method 2, $r = \cdot 98$; 1 with 3, $r = \cdot 95$; 2 with 3, $r = \cdot 93$. School *B* (Boys and Girls) 1 with 2, $r = \cdot 67$; 1 with 3, $r = \cdot 48$; 2 with 3, $r = \cdot 52$. The marks for each subject were then pooled, and the subjects graded afresh in rank for purposes of correlation with the yields of other tests. The possible number of suggestions in the pooled tests was 30. The frequency distribution is shown for the 65 subjects in the following table:

Table 6.

'Heavier' judgments, +1. 'Lighter' judgments, $-\frac{1}{2}$.
All cases of $\frac{1}{2}$ are reckoned belonging to the next lower whole number.

Possible suggestions...	30	29	28	27	26	25	24	23	22	21	20	19	18	17	16
B. Boys	.	.	.	1	.	.	.	.	2	.	2	1	2	.	1
Girls	.	.	1	.	.	.	.	1	.	.	.	.	2	1	1
A. Boys	.	.	.	.	.	1	1	.	.	.	.	1	2	.	.
Total	.	.	1	1	.	1	1	1	2	.	2	2	6	1	2
Distributed in 5's			2					5					13		

Possible suggestions...	15	14	13	12	11	10	9	8	7	6	5	4	3	2	1	0
B. Boys	1	2	2	2	1	.	.	.	1	.	.	1	.	.	.	.
Girls	1	2	5	1	1	.	1	1	.	1	.	.	.	.	.	.
A. Boys	2	.	1	1	.	2	2	2	5	1	2	2	1	1	.	.
Total	4	4	8	4	2	2	3	3	6	2	2	3	1	1	.	.
Distributed in 5's			22					16						7		

Test 6. Fidelity of Report.

This test was used with children of school *B* only. Two coloured pictures (postcards—'The Mill' and 'The Village') were used for the test. An interrogatory was drawn up in which a certain number of questions were inserted of an 'expective' and 'implicative' character; as "What kind of animal was walking beside her?" (when there was none); "From which chimney, right or left, was smoke coming?" (when no chimney was seen in the picture). Marks for suggestibility were given as follows: (i) for an answer showing that the subject had taken the suggestion that something was shown in the picture, which *de facto* was not there, (*a*) when such a thing could reasonably be expected to be in the picture (as, *e.g.* chimneys on the house) = 1; (*b*) when there was no reason to suppose such a thing to be in the picture = 3; (ii) when affirmative answers were given to questions such as "Wasn't...there?" "Do you remember...?" = 2; (iii) for further answers determining the colour, position, etc. of something not shown in the picture, but admitted in a previous answer = 1.

The subjects were graded in rank for each picture-test and the reliability coefficient taken ($r = \cdot 50$). The marks were then pooled and the subjects again graded in rank for comparison with the other tests. The possible values of suggestibility, according to the system of marking employed, lay between 0 and 42. The frequency for the 38 subjects is shown in the following table:

Table 7.

Suggestibility	42	41	40	39	38	37	36	35	34	33	32	31	30	29	28	27	26	25	24	23	22	21
B. Boys	.	.	.	.	.	.	.	.	1	.	1	.	.	.	.	.	.	.	1	1	.	.
Girls	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	2	.	.
Total	.	.	.	.	.	.	.	.	1	.	1	.	.	.	.	.	.	.	1	3	.	.
Dist. in 5's	.				.					2					.					4		

Suggestibility	20	19	18	17	16	15	14	13	12	11	10	9	8	7	6	5	4	3	2	1	0
B. Boys	2	.	.	.	1	1	1	1	.	.	1	1	3	1	.	1	.	1	.	1	.
Girls	1	.	1	1	.	.	.	1	.	1	1	2	2	2	3	1	.	1	.	.	.
Total	3	.	1	1	1	1	1	2	.	1	2	3	5	3	3	2	.	2	.	1	.
Dist. in 5's			6					5					16					5			

Test 7. Suggestion of Line Lengths by Personal Influence. (Contradictory Suggestion.)

This test was employed with subjects in both schools *A* and *B*. It does not appear to be a very suitable one for the purpose; or else the method of employing it in the present research may have been at fault.

The possible number of suggestions was six. The frequency distribution is as follows:

Table 8.

Suggestions	6	5	4	3	2	1	0
B	.	1	3	2	5	9	18
A	.	.	.	.	4	5	18
Total	.	1	3	2	9	14	36

2. *Correlations between the various tests.*

The subjects of both schools *A* and *B* having been finally graded in ranks for each test, the correlations between the various tests (with their probable errors) were ascertained. The method of ranks was used throughout. The correlation coefficients are shown in the following tables (P.E. shown in brackets):

Table 9. (School *B*.)

	1	2	3	4	5	6	7
1. Hand rigidity	.	·43	·40	·30	·28	·28	·04
2. Progressive lines	·43 (·093)	.	·33	·24	·32	·23	– ·06
3. Warmth illusion	·40 (·096)	·33 (·101)	.	·17	·10	·05	– ·18
4. Hand levitation	·30 (·104)	·24 (·107)	·17 (·110)	.	·07	·01	·12
5. Progressive weights	·28 (·105)	·32 (·101)	·10 (·112)	·07 (·113)	.	·09	·34
6. Fidelity of report	·28 (·105)	·23 (·107)	·05 (·113)	·01 (·114)	·09 (·113)	.	– ·03
7. Line lengths	·04 (·114)	– ·06 (·113)	– ·18 (·109)	·12 (·111)	·34 (·100)	– ·03 (·114)	.

Table 10. (School *A*.)

	1	2	3	4	5	6	7
1. Hand rigidity	.	.	·66	.	·23	.	·32
2. Progressive lines	.	.	.	.	.	.	.
3. Warmth illusion	·66 (·078)	.	.	.	·18	.	·18
4. Hand levitation	.	.	.	.	.	.	.
5. Progressive weights	·23 (·127)	.	·18 (·130)	.	.	.	·13
6. Fidelity of report	.	.	.	.	.	.	.
7. Line lengths	·32 (·119)	.	·18 (·130)	.	·13 (·133)	.	.

Table 11. (Schools *A* and *B* weighted.)

	1	2	3	4	5	6	7
1. Hand rigidity	.	.	·51	.	·26	.	·16
2. Progressive lines	.	.	.	.	.	.	.
3. Warmth illusion	·51 (·064)	.	.	.	·13	.	– ·03
4. Hand levitation	.	.	.	.	.	.	.
5. Progressive weights	·26 (·081)	.	·13 (·086)	.	.	.	·25
6. Fidelity of report	.	.	.	.	.	.	.
7. Line lengths	·16 (·085)	.	– ·03 (·087)	.	·25 (·081)	.	.

3. *Comparison of the results of the tests with teachers' estimates.*

Separate estimates were made by two or more of their teachers with respect to certain qualities of the pupils who acted as subjects in these tests; and correlations were ascertained between these estimates. For school *B*, r between different teachers' estimates for the same children for G (General Intelligence) $= \cdot 49$. If we pool tests Nos. 1 to 6 for this school in two groups (Nos. 1, 3 and 5, and Nos. 2, 4 and 6, together), we can obtain a coefficient of correlation between them. This works out at $r = \cdot 48$ or (including test No. 7 in the odd-numbered tests) $r = \cdot 44$. What is the meaning of this figure? It signifies that the estimates of suggestibility, as measured by one group of tests, are related to those measured by the other group; and it shows a common factor or factors to have been operating in all the tests. The two groups of tests can be likened to two sets of examination papers by which the general information or ability of those sitting for examination is estimated, or to the estimates of the two or more teachers as to the capability of their pupils under any given head. As a matter of fact, the closeness of the two r's in this case indicates that either group of the tests, as a measure of the suggestibility of the subjects, may be taken as an estimate of it about as good as the formed opinion of any one of their teachers upon the qualities which they evaluated.

Similarly, correlating test No. 1 with the pool of tests Nos. 3 and 5 (school *A*) we get $r = \cdot 29$; or taking the pool of tests Nos. 1 and 3 with that of tests Nos. 5 and 7, $r = \cdot 21$. The estimates of the two teachers for the subjects of these tests under the head of quickness of apprehension was, $r = \cdot 71$. The large discrepancy in this case is probably due to a very considerable extent, at least, to the fact that the estimates were made by the form master and the head master of the school; a pair of estimates which as a rule are not so satisfactory as those of independent form masters.

Correlations were also ascertained between suggestibility (S) as evidenced in the tests and certain estimates of character qualities made by teachers in school *A*. The only significant ones, for our purpose, seemed to be:

Tendency to abandon tasks in face of obstacles and S; $r = - \cdot 36$.
Soundness of common sense and S; $r = \cdot 75$.
Originality of ideas and S; $r = \cdot 21$.

The following correlations of S with other tests are of interest. The same group of subjects (school *B*) was tested for Perseveration (P) by

Dr Wynn Jones, and for General Intelligence (G) by Mr Burt, for Oscillation (O)[1] by Mr Flügel, and for Motor Dexterity (M) by Miss Carey. Pooling the G, P, M and O tests severally and the first six tests of S, we get

S and G; $r = \cdot 15$.
S and P; $r = -\cdot 08$.
S and M; $r = \cdot 10$.
S and O; $r = \cdot 04$[2].

There appears to be no tendency, positive or negative, for S to accompany G, P, M or O.

Prestige and its absence. If we compare the tests 'with prestige' (Nos. 1, 4, 6 and 7) with those 'without prestige' (Nos. 2, 3 and 5) by severally pooling and correlating them, we find: S with, and S without prestige; $r = \cdot 40$. But we have reason to think that this classification of prestige and non-prestige tests is faulty; and that the real prestige tests in these experiments were Nos. 1, 3 and 4. (See Part III.)

II.

During the Summer Term of 1920, the second of the writers of this paper received the results just given (schools *A* and *B*) and carried out a further series of tests on 56 children (33 boys, average age 13·2, and 23 girls, average age 13·6) in another school (*C*).

With one exception (test 6; Fidelity of Report), the tests used were the same as those already described. The methods of carrying out the tests and of marking were also the same as those used in the first research.

1. *Frequency distributions for the various tests.*

The following results were obtained[3].

Test 1. Hand Rigidity by Suggestion.

The test was given twice, once with each hand, and the reliability right hand *vs.* left hand worked out $r = \cdot 99$ for boys and ·94 for girls. Grading as before, but not dividing grade 1 into two grades on a time basis, the frequency distribution for classes 1 to 8 is: 18, 2, 3, 1, 6, 7, 9, 10.

Dividing the subjects into (i) those who could, and (ii) those who could not, close their hand, 73 per cent. Boys, 35 per cent. Girls, Total 57 per cent., could close their hand, while 27 per cent. Boys, 65 per cent. Girls, Total 43 per cent., could not.

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[1] Of attention, as in reversible perspective, etc.

[2] N.B. One subject was omitted in the O tests.

[3] In order to economise space the Boys' and Girls' results, calculated separately, are here pooled, and the distributions given in the briefest possible form.

Test 2. Suggestion by Progressive Lines.

The frequency distribution for this test is shown in

Table 12.

Marks	15	.	14	.	13	.	12	.	11	.	10	.	9	.	8	.	7	.
Subjects	.	.	.	.	.	..	.	1	.	.	.	1	1	2	.	3	1	2

Marks	6	.	5	.	4	.	3	.	2	.	1	.	0	.	−1	.	−2
Subjects	3	.	8	8	3	5	8	1	5	1	.	.	1	.	2	.	.

Test 3. Suggestion by Illusion of Warmth.

The test was performed as before, ten times with each subject. The reliability for right hand *vs.* left hand was: Boys, $r = \cdot 99$; Girls, $r = \cdot 99$. The frequency distribution (both hands pooled and averaged, and set out in 5″ intervals) is given in the following table:

Table 13.

Time (secs.)	0–5	5–10	10–15	15–20	20–25	25–30	30–35
Subjects	11	5	4	1	3	3	1

Time (secs.)	35–40	40–45	45–50	50–55	55–60	60
Subjects	5	4	1	1	1	16

Test 4. Hand Levitation by Suggestion.

The test was performed twice with each subject, once sitting and once standing. The reliability between the two was: Boys, $r = \cdot 81$; Girls, $r = \cdot 93$. The results for the two hands were then pooled and averaged, and the following frequency distribution obtained for classes 1 to 9: 16, 2, 3, 4, 0, 0, 2, 6, 23.

When the subjects were divided into those whose hands rose from the table, and those whose hands did not, it was found that 24 per cent. Boys, 74 per cent. Girls, Total 44 per cent., belonged to the former, and 76 per cent. Boys, 26 per cent. Girls, Total 56 per cent., belonged to the latter class.

Test 5. Suggestion by Progressive Weights.

The same three methods were used as before. The reliability coefficients between them were: Boys and Girls pooled, Method 1 with Method 2, $r = \cdot 35$; 1 with 3, $r = \cdot 56$; 2 with 3, $r = \cdot 55$. The lower reliability in school *C* compared with *A* and *B* may be due in part to the large number of errors in judging the first four weights where there was actual increase. One or more errors were made in method 1 by 42 per cent. Boys and 39 per cent. Girls; and in method 2 by 18 per cent. Boys and 22 per cent. Girls. There were no errors in method 3. Pooling the marks for the three methods the frequency distribution (marks 30 to 0 distributed in fives) is: 1, 6, 8, 15, 19, 7.

Test 6. Fidelity of Report.

This test was only given to the Girls, two of whom were absent; the numbers being thus reduced to 21. The method of administering it differed greatly from that used in Part I. There the questions were asked individually and immediately after the pictures had been seen. In the present instance the subjects had written a description of two pictures (the two given by Terman in the Test Material for use with his *Measurement of Intelligence*) a month previously, in connexion with another research. A questionnaire for each of the two pictures was drawn up; and in it were included questions of a nature similar to those described in Part I. The questionnaire was read to the subjects as a group, and the answers to the questions written down as each was read out. The method of marking used in Part I was adopted, and the reliability coefficient between the two pictures worked out: $r = \cdot 62$. The marks for the two pictures were then pooled and a frequency distribution made. This proved so similar in form to the one obtained in the former research that it is included here. The total of possible marks was 41. The results (marks 40 to 0 distributed in fives) are: 0, 2, 0, 3, 3, 3, 6, 3.

Test 7. Suggestion of Line Lengths by Personal Influence.

In the majority of cases the suggestion seemed to arouse contrary suggestion in the subjects, especially in the case of the Boys, while those who yielded appeared to do so with reluctance. The frequency distribution for marks 6 to 0 was: 0, 0, 0, 2, 2, 6, 46. Owing to these poor results this test was omitted when working out the correlations between the tests.

2. *Correlations between the tests.*

These were worked out between the various tests by the method of ranks and the formula $r = 1 - \frac{6\Sigma d^2}{n(n^2 - 1)}$. They are shown (together with their probable errors) in

Table 14.

Boys—Girls (pooled and weighted).

	1	2	3	4	5
1. Hand rigidity	.	·17	·06	·73	·28
2. Progressive lines	·17 (·091)	.	− ·24	·15	·24
3. Warmth illusion	·06 (·094)	− ·24 (·088)	.	·23	·09
4. Hand levitation	·73 (·043)	·15 (·091)	·23 (·088)	.	·24
5. Progressive weights	·28 (·086)	·24 (·088)	·09 (·094)	·24 (·088)	.

The correlation between the odd numbered tests (1, 3, 5) and the even numbered ones (2 and 4) was also worked out (see Part I, p. 61): $r = \cdot 318$.

3. *Correlation of suggestibility with other factors.*

The subjects were also tested for General Intelligence (G); and Perseveration (P) (by Dr Wynn Jones' tests) and compared with their ranking for Suggestibility. Tests for O and M were not performed. The final order in each case was calculated by pooling the ranking in the individual tests (in the case of Suggestibility, Nos. 1–5). Results: S and G, $r = \cdot 01$; S and P, $r = \cdot 00$.

The correlations between Suggestibility and the following character qualities, as estimated by the class teacher, are: S and Common Sense—Girls, $r = -\cdot 13$; Boys, $r = \cdot 29$; Pooled, $r = \cdot 12$. S and Originality of Ideas—Girls, $r = -\cdot 45$; Boys, $r = -\cdot 03$; Pooled, $r = -\cdot 20$. S and Tendency to Abandon Tasks in Face of Obstacles—Girls, $r = \cdot 48$; Boys, $r = -\cdot 09$; Pooled, $r = \cdot 14$.

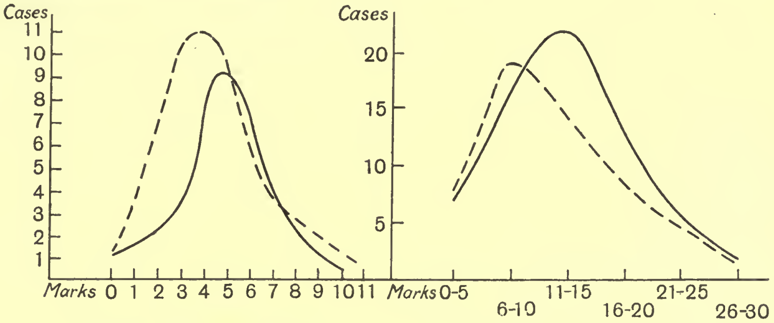
Common Sense as estimated by a teacher is doubtless a quality largely consisting of affective and conative elements, and not purely cognitive. The cognitive aspect may be indicated by two additional tests given to the subjects—namely two "Probable Situations" tests, similar to those described by Dr Webb in *Character and Intelligence*: "If such and such a thing happened, write down (i) all the things you *could* do; (ii) say which of them you *would* do; (iii) say *why*." The reliability between these two tests (marks for sections 1, 2, 3 pooled) was: Boys, $r = \cdot 70$; Girls, $r = \cdot 80$. The marks for the two tests were then added together and a fresh ranking made. The correlations of S with Common Sense, as so measured, are: Boys, $r = \cdot 00$; Girls, $r = -\cdot 45$; Pooled, $r = -\cdot 18$.

III.

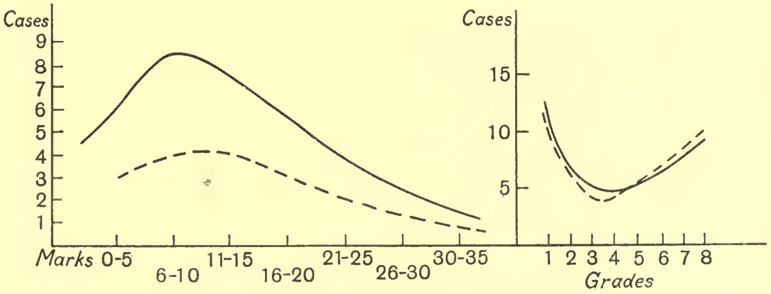
1. *Comparison of the frequency distributions reached in the two researches.*

The first significant feature is the correspondence of the frequency distributions for the same tests in the two researches. It will have been noticed already that in the separate researches there was a marked correspondence between the form of distribution for Boys and Girls. This is also found when the results obtained by two different experimenters, on children differing in age by 1·5 years and living in different surroundings, are compared.

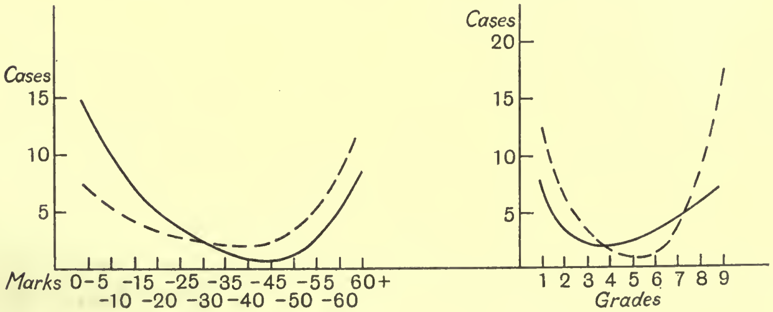
Reference to the tables given will show this; but it is made more evident by exhibiting the results graphically, as is done below. Indicating the results (Boys and Girls pooled) of the first research by ———, and of the second by --------, the following 'smoothed' curves are obtained mathematically[1].



Test 2. Progressive Lines. Test 5. Progressive Weights.



Test 6. Fidelity of Report. Test 1. Hand Rigidity.



Test 3. Warmth Illusion. Test 4. Hand Levitation.

Fig. 1.

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[1] A final slight smoothing was made by eye.

For the same test the form of curve is identical. Taking the tests as a whole, the curves form two distinct types (i) an ordinary, more or less, symmetrical curve, and (ii) a 'U' curve.

The tests producing the latter are the two Hand tests with Personal suggestion, and the Warmth Illusion. The first two are obviously tests in which the factor of prestige, or personal suggestion, is at a maximum; and in the warmth test the same factor, though not so evident, is weighty. Not only is the imposing character of the electrical warmth-tester a strong cause of the suggestion of heat; but the personal attitude of the experimenter seems to be even a stronger one. His manifest general attitude towards the apparatus and the subject of the experiment, his imposing expectancy, his manipulation of the visible switch, all go to form an amount of prestige which is lacking in other tests formerly regarded as prestige tests, such as that based on the Fidelity of Report test. The "Suggestion of Line Lengths by Personal Influence" may be a prestige test; but if so, at any rate in the present research, it has operated mainly in an inverse manner, and, if anything, produced contra-suggestion.

If the form of curve is to be taken as an indication, then these three tests (Hand Rigidity, Hand Levitation and Warmth Illusion) should be classed as the prestige tests[1].

With regard to the 'Report' test, the correspondence of the curves is remarkable, considering the different conditions in which the experiments were conducted. Though it seems natural to regard this test as mainly a prestige test, the form of curve and the fact that it would probably be equally effective with a printed form of questions and instructions appear to indicate that prestige is not a large factor in it.

What is the significance of these two divergent types of distribution?

Can we say that, in the case of suggestion by prestige or personal influence, subjects tend to be either very suggestible or very little so; and that in the case of suggestion with a minimum of prestige, or personal influence, suggestibility tends to have a normal distribution? And if so, are there two distinct types of suggestibility? Or are the different curves due to the nature of the tests given and the method of marking?

From an inspection of the curves the latter seems more probable. In all tests of suggestibility account must be taken of three modes of response—positive, neutral and negative[2], the latter being contra-suggestion. In any given test the distribution may measure any one, two or three of these factors.

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[1] If in the warmth test the subjects are graded, not by time, but in a scale from 1 to 10, according to the number of failures of suggestion recorded, the curve obtained is still the same.

[2] Prideaux, this *Journal*, x. 235 *sq.*

Theoretically then, careful measurement of the subjects' total reactions might produce such a distribution as follows in which the three modes of response are all indicated.

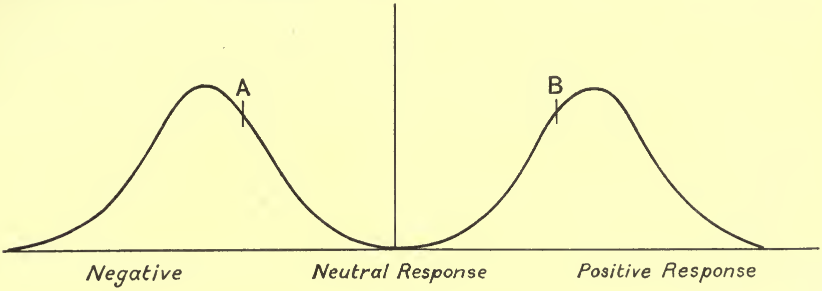

Fig. 2.

Practically the distributions overlap, owing to the difficulty of distinguishing neutral and negative responses.

Now compare the tests. In those with prestige emphasized there is (and this fact is confirmed by observation of the subjects) a much greater liability that contra-suggestion will be aroused. The conditions of the experiment and the method of marking make it impossible to distinguish between neutral and negative responses; so, to a small number of neutral responses, all the negative responses are added, producing a large number of cases at one end of the scale. By the method of marking we also more or less fail to evaluate accurately and to distinguish clearly cases of great and moderate suggestibility. Consequently a large number of cases is also grouped at the other end, and the curve takes the form of a U. In the curve given, what happens is that all the cases of positive responses greater than *B*, and of negative greater than *A* are summed up; and the resultant curve is the portion between *A* and *B* with the parts beyond added towards the two extremes.

In the non-prestige tests the factor of contra-suggestibility is absent, or only slightly present. Also here the marking admits of differences between moderate and extreme suggestibility being noted. Therefore the distribution *tends* towards a normal one. Possibly the skew in the curve is due to a certain number of cases of contra-suggestion. This would yield lower marks and thus account for the steeper side of the curve.

It is suggested that the test (No. 7) which was mostly a failure can be explained on the same assumption. We have in this case only to regard it as a test of contra-suggestion. Then all failures to take the given suggestion are measured in terms of contra-suggestion[1]; and the method of marking allows no recognition of degrees. What is obtained is practically one-half of a normal curve, with all the cases belonging to the second half summed up, *i.e.* distribution curve for pooled results of test No. 7.

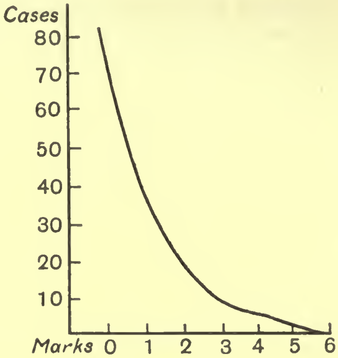

Fig. 3.

In this test, if a system of grading could be devised to mark prompt and decided refusal of the suggestion and the degrees of hesitation, etc., which lead on gradually to acceptance, then satisfactory results should be obtained; and possibly a distribution approximating to that in Fig. 2 would result.

There seem then no grounds for thinking that 'personal' and 'impersonal' suggestibility are of two distinct types. The results obtained from 'personal' and 'impersonal' suggestion will however be different. In the former directly positive or negative responses may be expected; and these are found to be about equal in number. In the latter there will be very few failures and the majority of subjects will be moderately suggestible. This result has a distinct bearing on many educational problems.

2. *Discrepancies in the results.*

Certain discrepancies in the results are interesting. In the Hand Rigidity and Hand Levitation tests the percentage of boys influenced as compared with that of girls is different for the two experimenters. In the second research the results obtained from the boys were low, and from the girls high. One significant cause seems to be that during the war the boys' class had no male teacher, and their discipline consequently deteriorated. Another was that the experimenter was known to most of them, and lacked the additional prestige surrounding a complete stranger. The girls' class however had been in charge of a capable teacher of long standing in the school, and was under complete control and discipline.

Another interesting difference comes out in comparing the Hand Rigidity and Hand Levitation tests. In both researches the latter was comparatively more effective in inducing suggestibility with the girls, and the former with the boys. If as maintained by Dr Prideaux (in the article referred to) suggestibility "depends on the affective processes operating between the two persons," and that the sexual instinct is a distinct factor, then the difference noticed above might be accounted for by the additional effect of personal contact on the girls. In connexion with this and with the psycho-analytic view that suggestion is a special variety of transference, and takes its root in "the masochistic component of the sexual instinct[1]," it is interesting to observe that in both these tests there is no significant correlation with the age of the subjects of this research.

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[1] No doubt many may be included of the kind described above as neutral.

3. *Comparison and summation of the correlations.*

(*a*) Comparing the two researches, considerable fluctuation in the correlations is observed; and there are some the magnitude of which does not exceed three times the P.E., or is less than the P.E.; but in general, though small, they are positive in all the schools. Two discrepancies stand out: the very much larger correlation obtained between the Hand Rigidity and Hand Levitation tests in the second research; and the larger one between Hand Rigidity and Warmth Illusion in the first. This latter difference may well be due to the varying amount of prestige two different experimenters might introduce into these tests. Another difference is that the correlation between groups of tests is smaller in school *C*. Also the correlations between S and character qualities are different: there being a very small correlation with Common Sense in school *C* (·12) instead of a large one in school *A* (·75); one of − ·20 with Originality of Ideas instead of ·21; and ·14 with Tendency to Abandon Tasks in Face of Obstacles instead of − ·36. But since in school *A* only boys were tested we may separate the results yielded by boys and girls in school *C* for purposes of comparison, as these differed considerably. The results (boys only) are:

	School *A*	School *C*
S and common sense	$r = \cdot 75$	$r = \cdot 29$
S and originality of ideas	$r = \cdot 21$	$r = -\cdot 03$
S and tendency to abandon tasks	$r = -\cdot 36$	$r = -\cdot 09$

(*b*) Pooling (and weighting) the correlations for all three schools, we obtain the following table. The total number of subjects for tests Nos. 1, 3 and 5 is 121; for tests Nos. 2 and 4, 94; for test 6, 59. (P.E. is shown in brackets.)

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[1] Prideaux; *op. cit.*

Table 15.

	1	2	3	4	5	6
1. Hand rigidity	.	·27	·30	·55	·28	·16
2. Progressive lines	·27 (·071)	.	– ·01	·18	·27	·32
3. Warmth illusion	·30 (·058)	– ·01 (·077)	.	·20	·11	– ·25
4. Hand levitation	·55 (·053)	·18 (·074)	·20 (·074)	.	·17	– ·06
5. Progressive weights	·28 (·059)	·27 (·071)	·11 (·062)	·17 (·075)	.	·00
6. Fidelity of report	·16 (·089)	·32 (·082)	– ·25 (·085)	– ·06 (·092)	·00 (·092)	.

The correlations between the columns of correlations were also worked out to see if the conditions for a hierarchy obtained. The results are:

Table 16[1].

	1	3	4	2	5	6
1. Hand rigidity	.	·81	·99	– ·31	·42	·05
3. Warmth illusion	·81	.	·93	– ·64	·77	– ·45
4. Hand levitation	·99	·93	.	– ·11	·81	·24
2. Progressive lines	– ·31	– ·64	– ·11	.	– ·01	·92
5. Progressive weights	·42	·77	·81	– ·01	.	·96
6. Fidelity of report	·05	– ·45	·24	·92	·96	.

These, when corrected by the Hart and Spearman formula[2] become

Table 17.

	1	3	4	2	5	6
1. Hand rigidity	.	·94	[·99]	[– ·31]	·62	·06
3. Warmth illusion	·94	.	·99	[– ·64]	·88	– ·46
4. Hand levitation	[·99]	·99	.	– ·25	·94	·24
2. Progressive lines	[– ·31]	[– ·64]	– ·25	.	– ·01	1·09
5. Progressive weights	·62	·88	·94	– ·01	.	[·96]
6. Fidelity of report	·06	– ·46	·24	1·09	[·96]	.

[Coefficients in brackets are those from columns to which the formula could not be applied.]

What does this signify?

Dividing the table so as to mark off the Prestige and Non-prestige tests, it will be noted that the corrected correlations[3] between the former are all high and positive approaching + 1 (av. = ·97). Of the three correlations between the Non-prestige tests, two are practically + 1, the other being approximately 0. The correlations between the two groups vary from + ·94 to − ·64. Those between a Prestige test and tests 2 and 6 being low and in general negative (av. = − ·22), between a Prestige test and test 5 high and positive (av. = + ·78). These facts seem to suggest that the correlations are produced neither by a single general factor, nor by a number of specific factors, nor by two group factors such as Prestige and Non-prestige; but by a combination of a general factor with group and specific factors.

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[1] In this and following tables, the tests are printed in a different order from that previously used; but they are numbered as before.

[2] This *Journal*, 1912, 58.

[3] The conclusions that follow can also be drawn from the uncorrected correlations, and are therefore independent of the validity of the Hart and Spearman formula which has been questioned. This *Journal*, x. 320.

4. *The problem of a 'general factor.'*

It was noticed during the experiments that in a number of cases certain subjects proved very suggestible to all the tests, or nearly all; while others accepted practically no suggestions. This would point to a general factor of suggestibility strongly marked in some subjects. It has also been pointed out that contra-suggestibility was a factor entering especially into the Prestige tests. This may then be regarded as a group factor influencing them. The similarity of method in tests 2 and 5 may also make certain elements operate as a group factor in these two tests.

Assuming the existence of a general factor, and of these group factors, the values obtained in the table of correlations between columns could be partly explained. The effect of the group factors will not affect, to any appreciable extent, the correlations between the Prestige tests (1, 3 and 5) which will be approximately + 1, nor those between the Non-prestige tests which should also approximate to + 1; while those between a Prestige test and tests 2 and 5 will be slightly reduced. This obtains with certain exceptions: namely the greater reduction between the Prestige tests and tests 2 and 6, and the low correlation between tests 2 and 5.

These discrepancies would be explained if there was a largish common element, in addition to Suggestibility, operating as a group factor in tests 2 and 6. Analysis of the tests and methods of procedure has, however, not revealed such a factor. Still it may exist, or possibly experimental errors, augmented by the lower reliability of all correlations with test 6 (due to the fewer number of cases), may have produced spurious correlation. In any case further research seems necessary.

The authors consider that, taking this into consideration, the most probable explanation of their results is that advocated, namely, the existence of a general factor of Suggestibility combined with group factors common to two or more tests.

The following correlations between groups of tests seem to support this opinion. The weighted average of all the correlations between the

Prestige tests (Nos. 1, 3 and 4) = ·34; between the Non-prestige tests (2, 5 and 6) = ·21; between tests 1, 3 and 5 = ·23; between tests 2, 4 and 6 = ·15. The weighted average of all the correlations between each Prestige and each Non-prestige test = ·10; and between tests 1, 3 and 5 and 2, 4 and 6 = ·16.

Assuming the existence of a general factor the tests were examined from the point of view of their Saturation of Suggestibility[1] by the formula $r^2as = \frac{A^2 - A'}{2T - 2A}$; where r^2as is the square of the correlation between any test (a) and Suggestibility; A is the sum of the correlations between this test (a) and every other test; A' is the sum of the squares of these correlations; and T is the sum of all correlations between the different tests. The calculation gives the following results:

Saturation of pooled tests (omitting tests 6 and 7)

r (test 1 and S) = ·88,
r (test 2 and S) = ·31,
r (test 3 and S) = ·26,
r (test 4 and S) = ·61,
r (test 5 and S) = ·40.

The pooled correlations for S and G = ·06; S and P = ·03.

There is no tendency shown for Suggestibility to correlate with General Intelligence, Perseveration, Oscillation or Motor Dexterity. With regard to Common Sense, it is to be observed that the correlation between this (as estimated by teachers) and S is positive in the case of the Boys (school *A*, r = ·75; school *C*, r = ·29; Pooled, r = ·44). In the case of the Girls of school *C* it is negative (r = – ·13). The correlation with Common Sense, as measured by the "Probable Situations" tests and S is Boys, r = ·00; Girls, r = – ·45.

Comparing these figures, it is interesting to note that, however measured, the Boys show a higher correlation between Common Sense and S than the Girls. Also it has already been suggested that affective and conative as well as cognitional elements enter into the data on which the teachers' estimates are based, and that these are absent in the "Probable Situations" tests. There is no room in these for contra-suggestion; and this may be a weighty factor in the case of the estimates. The contra-suggestible child may well appear stupid at times, and be classed as low in Common Sense as well as in Suggestibility.

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[1] See "Mental Tests of Dementia," by B. Hart and C. Spearman, *Journ. of Abnor. Psy.* Oct.–Nov. 1914.

IV. SUMMARY.

1. Suggestion arises out of the total environment and conditions to which a subject is exposed. When personal prestige is the most conspicuous element in a test or situation, the suggestion arising may be called a 'Personal' or 'Prestige' suggestion. When this is at a minimum and the conditions the main factor, the suggestion may be called an 'Impersonal' or 'Non-prestige' suggestion.

2. In cases of 'Personal' suggestion a negative response, owing to the development of contra-suggestion, is frequent. Consequently subjects tend to be classified into two sharply divided groups, the suggestible and the non-suggestible (or contra-suggestible).

3. In cases of 'Impersonal' suggestion, as obtained in our tests, contra-suggestibility is not so frequently aroused, and the distribution is approximately normal, the majority of the subjects being moderately suggestible.

4. There is evidence which points to a general factor of Suggestibility complicated by group factors.

5. General Suggestibility is greatly modified by the specific conditions and elements of the whole situation, which vary in individual cases, according to experience of it and knowledge about it.

6. There is no ascertained tendency for Suggestibility to go with other 'general' factors such as General Intelligence, Perseveration, Oscillation, or Motor Dexterity. There is a small correlation between it and Common Sense regarded as consisting largely of affective and conative elements; but none if Common Sense is considered as a purely cognitive quality.

(*Manuscript received* 1 *January* 1921.)

RECENT WORK IN EXPERIMENTAL AESTHETICS.

By EDWARD BULLOUGH.

To speak at the present moment of 'recent' work in experimental Aesthetics is perhaps a little misleading. No work of an experimental kind has, to my knowledge, been carried out since 1914, and what is meant here by 'recent' are experiments undertaken roughly between 1900 and 1914. It is, however, precisely the lack of experimental work during the last six years which prompts me to attempt a cursory survey of some of the earlier results. For now is the time to take stock of what has been done so that we may build as best we can upon what has been achieved, and survey the unexplored regions lying before us for investigation. This is one of the main objects of this paper.

Its other object is to direct attention to certain problems rather than to others, problems, the examination of which seems to me to promise specially useful results, because the problems themselves appear to me to lie on the main road followed of recent years by aesthetic speculation.

In order to explain, I may perhaps be allowed very shortly to touch upon the earlier beginnings of experimental work in Aesthetics. They are associated with the name of the man whom every student of the subject knows as 'the father of modern Aesthetics' and also as the originator of experimental work in it—Theodor Fechner. In his *Vorschule der Aesthetik*, published in 1867, he gave among other results of his thought the results of his experimental examinations which, by a peculiar irony of fate, had been directed originally to testing the validity of the most rigidly formalistic doctrine of his day, Zeising's 'golden section.' It would be waste of time to recapitulate Fechner's experiments or results. They have been summarised repeatedly, as for instance in C. Lalo's *Esthétique expérimentale contemporaine* (1908). They have in addition been tested all over again, not without interesting results, in a special set of experiments[1] carried out in America and have led within the period here under discussion to a considerable body of work dealing especially with spatial forms such as the divisions and arrangements of lines, balance and symmetry, etc. I do not propose to deal with these experiments here except in so far as they bear upon the problems to which I should like to draw attention. But I take this opportunity to say that there is hardly an experiment carried out in the past which it is not worth while testing again, in order to interpret it in the light of the constantly developing theories of aesthetic experience.

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[1] L. Martin: "An experimental study of Fechner's Principles of Aesthetics," *Psychol. Rev.* XIII. 142 ff.

Fechner's experiments cannot be said to have yielded any very striking or illuminating positive results. The reasons of their failure are well known and are set out in every criticism, such as can be found in the above-mentioned work of Lalo. The purely quantitative measurements, and the system of averages which he employed, were the direct consequences of the hedonistic principles on which he based his theory of Aesthetics in general. The search for the colour which the greatest number of persons prefer, the questions whether squares or oblongs are more generally liked, or what proportions of the arms of a cross meet with the widest approval, seem to us rather crude and naïve. But posterity owes a great debt of gratitude to Fechner for having attacked these kinds of problems, even if the results showed that little progress could be hoped for in that direction.

Now that we can look back upon nearly fifty years of aesthetic speculation "from below," as Fechner said, it is of interest to note that experimental work has followed very closely the trend and evolution of aesthetic theory in general. It would seem as if aesthetic speculation, in the attempt at a better and closer understanding of the aesthetic experience, had proceeded to analyse this experience into its component stages in the reverse sequence to that in which they might be supposed to follow and develop out of each other, and had gradually worked its way up stream, so to speak, from pleasure, the point at which the experience issues out into the sea of practical life, to the still hidden sources of its obscure beginnings.

Fechner will always be remembered in Aesthetics for having opened up Association as a possible aesthetic factor and for having thereby rescued Aesthetics from the narrow formalism into which it had fallen in his day. The introduction of Association led, partly in conjunction with the romantic views of the artist and his functions, to the theory of Expression and of the apprehension of this Expression as the central fact of the aesthetic experience. Its psychological examination produced the various theories of *Einfühlung* or Empathy which reached their flowering period between 1895 and 1905. Association has been much experimented upon, of course, as a fact of general psychology; but from the purely and specifically aesthetic point of view experimental work on

Association as a factor in the aesthetic experience has always presented considerable difficulties, partly by reason of its erratic and uncertain occurrence. Nor is there any doubt that its importance has been greatly exaggerated in the past and is at the present time grossly overrated in popular opinion.—Empathy and all the numerous *Einfühlungs*-theories were exclusively based upon introspection on the part of their authors. Indeed, the great varieties of views of their mechanism and the acrimonious wrangles which took place at the end of last century between the upholders of rival doctrines arose precisely from the generalisation of such purely personal introspective evidence. Experimental work on large numbers would, I believe, have shown that no single one of the explanations championed by different adherents of the theory could claim the monopoly of truth. In justice it must, however, be added that experimental tests of Empathy are extremely difficult to carry out. The relatively systematic observations of Vernon Lee and A. Thompson[1] are very suggestive but, being introspections of the authors themselves, have probably accentuated the idiosyncracies of the experiences. Many of the experiments designed to investigate Empathy either failed altogether, or brought to light points relating not to it, but to the matters with which I propose to deal in the sequel.

Meanwhile aesthetic theory did not stop at Empathy. Formalists and others hastened to point out that Empathy was unable to take satisfactorily into account the incontestable aesthetic qualities of purely formal arrangements, of repetition and symmetry, of composition and finish of workmanship. The adherents of *Einfühlung* tried to meet this attack, as Lipps, for instance, did in his brilliant and suggestive, but not wholly convincing *Raumaesthetik*[2]. The fact, however, remains that Empathy does not invariably occur in aesthetic appreciation. There proved to be an even more insidious line of attack, for it was found that Empathy occurs occasionally with exceptional clearness in what is admittedly bad art, or not an object of aesthetic appreciation at all. ✓ In any case it is fairly clear that Empathy may occur in aesthetic experience, but that it need not, and that it is neither a complete nor wholly satisfactory explanation of it.

The discussions of Empathy and the examination of the conditions under which it occurred had, however, the great merit of leading to attention being directed to what might be called the 'pre-empathic' stages of aesthetic experience. It was in response to the questions, how does Empathy come about, what makes Empathy possible, that these stages rose into prominence. For was it not reasonable to suppose, if Empathy occurred both within and also outside aesthetic experience, that that which in the one case made it into part of the aesthetic experience was something that preceded it and gave it its aesthetic tinge? And was it not equally reasonable to assume that the same thing happened with all the other ingredients of the aesthetic experience, with emotions and associations and pleasure and judgments? If this were really the case, would we not then have discovered in these pre-empathic stages the real *differentia* of the aesthetic from all other kinds of experience?

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[1] Vernon Lee and A. Thompson: "Beauty and Ugliness," *Contemp. Rev.* 1904; reprinted in Vernon Lee and A. Thompson's *Beauty and Ugliness* (1912, John Lane).

[2] Th. Lipps: *Raumaesthetik und geometrisch-optische Täuschungen*, 1897.

These are the problems upon which I venture here to direct attention. Experimental work upon them has been very sporadic. Much of the earlier work has unconsciously touched upon them, most frequently ignoring them, but sometimes throwing out valuable hints. At any rate we cannot be said to know much about them and for that reason they appear to me more worthy of attack and of a sustained effort to bring them to the test of actual experiment.

These pre-empathic stages of aesthetic experience comprise, in the first place, *the apprehension* of the aesthetic object, and secondly, antecedent to it, *the adaptation* initiating the apprehension.

I shall deal with the results of experimental work on the apprehension of the object first.

APPREHENSION.

The study of the apprehension or apperception of objects is a question of General Psychology rather than of Aesthetics, and a great deal of the experimental work done upon it by General Psychology has a direct bearing on the aesthetic problem. Experiments have, however, been tried also from a specifically aesthetic point of view in this connexion, and one of the important means of research has been brought within the scope of aesthetic experiment by the introduction of tests with short-time exposures. It is interesting that this was attempted for the first time in an endeavour to deal with Empathy. Külpe[1] tried to 'cut out' Empathy by offering to subjects a series of slides of architectural, sculptural and pictorial works with a short exposure of 3 seconds. The experiment failed, for even 3 seconds were sufficient to allow empathic states to emerge and associations to be formed. I had the same experience with single colours where even a fraction of a second was insufficient to cut out Empathy and Association[1]. But, besides directing attention to the time factor in the development of the aesthetic state—a point so far very inadequately tested—it gave the negative result that Fechner's 'direct' factor, which these experiments had hoped to isolate, was so intimately bound up with what he supposed to be the 'indirect' (associative) factor as to be practically inseparable. This and the discovery of adaptation raised in a concrete form the problem whether the apprehension of aesthetic objects could be treated as identical with that of non-aesthetic objects, in other words, whether there was such a thing as a definitely *aesthetic* apperception. Speculative Aesthetics had among many other things always tacitly assumed that there was, and the importance of the point is evident. For if there is anything aesthetic in the apprehension itself, it is conceivably this quality of the apprehension which in the first instance imparts the peculiarly aesthetic character to all the subsequent stages of the experience.

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[1] O. Külpe: "Ein Beitrag zur experimentellen Aesthetik," *Amer. J. of Psychol.* 1903, XIV. 215 ff.

Unfortunately there is a great dearth of accurate, experimentally ascertained knowledge on this question. Very little work has been done as yet from the specialised point of view of such a problem. The little information available is derived almost exclusively from experiments in the apprehension of pictorial objects and simple spatial forms and colours. A fair amount of research has indeed been undertaken with temporal forms of a simple kind, especially with rhythm[2], and the first really useful experiments on the apperception of single tones and simple tone-combinations are now available. But the apperceptive processes in the appreciation of poems or dramatic works are, except from a few stray observations[3], so far quite unknown.

Even in the case of spatial forms, our information is derived not so much from aesthetic experiments yielding introspective evidence of the subjects[1], as from tests demanding a written description of a previously apperceived object. Such descriptions—as distinct from the actual apperception—give certainly indications of the manner of the apprehension which they reflect, but are open to generally admitted objections. They usually omit to state all the apprehended details and involve, as written statements, a transfer of visual impressions into language[2]. They require especially as literary productions, however humble, a productive effort, alien to the merely apprehending process and to some extent destructive of appreciation[3]. The former drawback attaches, of course, also to introspective evidence, but is much more easily corrected in the spoken intercourse between experimenter and subject.

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[1] Edward Bullough: "The 'perceptive Problem' in the aesthetic Appreciation of single Colours," this *Journal*, II. 406.

[2] E. Meumann: "Untersuchungen zur Psychologie und Aesthetik des Rhythmus," *Philos. Stud.* x. 249; M. Ettlinger: "Zur Grundlegung einer Aesthetik des Rhythmus," *Ztsch. f. Psychol.* 1900, XXII. 161; R. McDougall: "The Structure of simple Rhythm-forms," *Harvard Psychol. Stud.* 1903, I; R. H. Stetson: "Rhythm and Rhyme," *Harvard Psychol. Stud.* 1903, I: "A Motor Theory of Rhythm and discreet succession," *Psychol. Rev.* 1905, XII.; K. Koffka: "Experimental-Untersuchungen zur Lehre vom Rhythmus," *Ztsch. f. Physiol. d. Sinnesorg.* LII. 1.

[3] M. Dessoir: "Anschauung und Beschreibung," *Arch. f. syst. Philos.* N.F. 1904, x. 20 ff.

In the absence of specifically aesthetic experiments, it is impossible to state any definite conclusions. But the available material of experimental work undertaken from the general psychological standpoint suggests at least certain problems the solution of which would materially advance the question of an 'aesthetic' apperception.

Binet[4] distinguishes in his *Experimental Study of Intelligence* four types of descriptions corresponding to four different types of apprehension: The *type descripteur*, the *type observateur*, *type d'érudit* and *type imaginatif* or *émotionel*. Members of the first type content themselves with a pure and bald description of the details of the object, without even seeking to grasp their significance, much less their relation or the meaning of their collocation. This type yields, as Binet expresses it, a mere "*description de nature morte.*" The second type 'observes,' *i.e.* attempts to seize and gauge the expressive and significant features of an object; it realises that not all the details are of the same importance, but bear a definite relationship of significative values both to each other and to the whole. This type gives, in his words, "*une description de tableau vivant.*" The distinctive mark of a person of the *type d'érudit* is the tendency not to describe the object at all, but to state his *knowledge about* it. The object of his attention is not the impression, but the associated or remembered facts called up by it. The last type, the *type émotionel* or *imaginatif*, attempts to describe—and therefore presumably also to apperceive—not so much the visible details of the object as its emotional meaning or its mood. A person of this class inclines, therefore, first, to grasp this aspect, and then to select only such details as help to reinforce the mood or its emotional appeal. Descriptions by this type are consequently equally centralised as those of the *type observateur*, but usually less rich in accurate detail and much more emotionally coloured. On the other hand, they are fuller of ideas and show more literary finish[1]. It must, of course, be remembered that these tests were carried out with children which detracts from the aesthetic value of the results, and that they were not undertaken from an aesthetic point of view at all. But, if we can trust our knowledge derived from other sources and supported by general considerations, it would seem that of these four types two at least, the *type observateur* and the *type émotionel*, have an aesthetic significance. I am confirmed in this belief by a set of experiments by F. Müller[2]. They were admirably arranged and were preceded by a trial experiment with adults, which enabled him to exercise a criticism upon the different types of his subjects and supplied a kind of aesthetic scale according to which he assessed the apperceptive types. He used Binet's type-distinctions which he slightly elaborated, and places, as it seems to me justly, both the *type descripteur* and the *type d'érudit* low in the aesthetic valuation.

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[1] Except the above-mentioned experiments of Külpe and Dessoir and those of v. Ritook: "Zur Analyse der aesthetischen Wirkung auf Grund der Methode der Zeitvariation," *Ztsch. f. Aesth.* v. 356, 512, and some experiments undertaken from the general psychological point of view which will be laid under contribution in the sequel.

[2] M. Dessoir: "Das Beschreiben von Bildern," *Ztsch. f. Aesth.* VIII. 460.

[3] This is a special case of the non-equivalence of the method of impression and the method of production which I have pointed out elsewhere (this *Journal*, III. 425–7).

[4] A. Binet: *Etude expérimentale de l'Intelligence*, Paris, 1903.

Another classification of direct bearing upon Aesthetics is the distinction between 'analytic' and 'synthetic' apprehension. The difference, rather vague, has long enjoyed general vogue, though it may be questioned whether either analysis or synthesis is ever completely absent from any act of apprehension. The interest lies rather in the different degree of emphasis given to the one or the other, and in the possibility that such differences are perhaps peculiar to the apprehension of particular objects or to the apperceptive habits of certain types of persons. "The process of perception...takes place in two stages. There is first (*a*) an immediate interpretation of the object as a whole and next (*b*) an analysis of this vaguely apprehended whole into its component parts.

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[1] An interesting confirmation of the *type observateur* and the *type émotionel* and of their aesthetic possibilities is furnished by Tolstoy (Сочиненія, IV. 176–8, 1903). A translation of part of this passage is to be found in P. Birukoff, *Tolstoy, his Life and Work*, I. 337, Heinemann, London. The occasion for Tolstoy's observations was a lesson in composition given to the peasant children of Yassnaya Poliana.

[2] F. Müller: *Aesthetisches und ausser-aesthetisches Urteilen des Kindes bei der Betrachtung von Bildwerken*, 1912. Müller expanded Binet's four types into six: (1) descriptive, (2) observing, (3) emotionally-descriptive, (4) emotionally-observing, (5) eruditely-descriptive, and (6) eruditely-observing (pp. 52–3).

Subjects of all ages show a striking uniformity in the first process, whereas in the second there was no less striking variability." These words are taken from a set of experiments[1] on perception undertaken from a general point of view and carried out by means of short exposures of lantern-slide pictures of an average of 1/13″ in the hope of thereby delaying the process of perception and recognition. These exposures were repeated as often as the subject wished, until he felt that he was unable 'to get any more out of it.' The failure to delay recognition or interpretation concords entirely with the earlier experiments of Külpe. The second step again suggests differences between individuals in their mode of apprehension which, unfortunately, do not appear with sufficient clearness in the records, but resemble the distinctions between Binet's *type descripteur* on the one side and his *type observateur* and *émotionel* on the other. The subjects appear to have followed two distinct procedures: "some continued (after the general recognition of the object) to see each picture as a whole except when some outstanding feature happened to fall in the focus of their vision....Other subjects made a more or less systematic search for details[2]." According to the experimenter it was especially the scientists who were distinguished by their systematic and patient analysis of every detail, quite irrespective of its importance for the whole or its intrinsic value, prompted by an impersonal curiosity to solve every obscurity and by an equally impersonal passion for completeness. Some asked for fifty or sixty exposures before they were satisfied, while subjects of the other type contented themselves with a tenth part of this number. Unfortunately the special object of these tests renders them hardly directly applicable to aesthetic purposes. Especially the lack of critical distinctions between the subjects, divided simply into scientists and non-scientists without reference even to their psychological differences in imagery, etc., and the hardly disguised bias of the experimenter in favour of the scientific mind, detract from their usefulness.

A peculiar defect from the aesthetic point of view was the choice of the pictures themselves. It would be of great value to ascertain whether objects of different character would be able to force upon the subjects an analytic or non-analytic apprehension, irrespectively of their usual apprehension and even against their individual apperceptive habits. In particular it would be interesting to discover whether the one or the other mode corresponds to aesthetic and non-aesthetic apprehension, assuming this difference to exist. But none of the slides used lent themselves easily to aesthetic appreciation, which was anyhow rendered almost impossible by the exclusively perceptive adaptation required by the tests. Most of the pictures showed more or less simple objects like animals or complex things without aesthetic interest. The only slides arousing more personal interest were two 'incident pictures' representing 'humorous incidents' of a very mild description and very inferior execution. It is interesting to note that several subjects called attention to the fact that their apprehension of these slides was of a type different from that of the other pictures. "I feel here," says one of them, "a marked difference of perception. In the engine picture it was a question of details. Here it is a question of the sense of a story, for which details are unnecessary"; and another stated that for him "the parts hardly exist in the story picture. Details would be a nuisance, so I keep to the story interest[1]." These differences make it probable that similar tests with different slide material and different adaptations might produce observations of interest for Aesthetics.

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[1] F. Smith: "An Experimental Study of Perception," this *Journal*, VI. 325 ff.

[2] *Ibid.* 329.

This is rendered all the more likely by some tests undertaken by M. Dessoir on the *Description of Pictures*[2]. All his subjects (10) were students of the history of Art. Three exposures of 1″ each were given of slides made from paintings of standing, though unknown to the subjects. After each exposure they were asked to write down a description of what they had seen. The result showed that all started with a general apprehension of the contents and especially with the disposition of the main elements of the pictures. "Even at the first immediate impression the pictures are determined with more or less certainty as belonging to a particular period of Art history (Dutch, Renaissance, 18th century, Modern), and as displaying a particular technique (etching, oil-painting, etc.), and are apprehended in their general appearance[3]." "The picture is apperceived in certain general outlines, so that those elements without which it would be artistically unintelligible, offer themselves for description[4]," and "it is apprehended from the start as a whole, within which some details emerge at once with perfect clearness[5]." "The subjects had the feeling at the first, or at the latest with the second, exposure that they had apprehended all that could be perceived in a momentary exposure[6]." Even the second and third exposures do not seem to have led to so purely analytic an apprehension as occurred among the scientists in the previously mentioned tests. No doubt details were gradually grasped with greater clearness, but—remarks Dessoir—"in spite of all the detailed apprehension as the description proceeds, the whole remains consciously present. Even the smallest detail is apprehended not for its own sake, so to speak, but in connexion with the whole[1]."

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[1] F. Smith: *loc. cit.* 335.

[2] M. Dessoir: "Das Beschreiben von Bildern," *Ztsch. f. Aesth.* VIII. 440.

[3] *Ibid.* 446. This is undoubtedly due to the particular bias of his subjects.

[4] *Ibid.* 447.

[5] *Ibid.* 449.

[6] *Ibid.* 450.

The tendency to apprehend parts or single details of a picture and to base the appreciation upon these, appeared in some experiments of Miss Calkins[2] as a peculiarity of children under nine years of age, and she summarises in the result the principal difference between young children and adults as the apperception of parts and of the whole respectively[3].

One point emerged accidentally from Dessoir's experiments which should certainly be examined more closely: some of his subjects seem to have apprehended first the *formal features* of the picture, the composition, general shapes and outlines, the grouping and the relation of main parts to each other, while others were conscious of the *content represented*, rather than of the formal aspect[4]. This apperception of formal qualities is corroborated by the tests of Külpe[5] and v. Ritook[6]. The last named even inclines to regard the difference as one of apperceptive type[7]. It seems desirable to obtain fuller information on this point by experiments specially designed to test this question, as the short exposures with which all these researchers worked may of themselves have forced the attention to formal characteristics. The difference between subjects may, however, be at the bottom of differences of apperception of simple spatial forms to which Katz[8] has drawn attention. He discovered at first accidentally and then proved by experiments, that persons differ in the manner of apperceiving such simple figures as triangles or two-dimensional representations of cubes, pyramids, cylinders, etc. Some interpret and apprehend suitable line-complexes as faces, or plain figures as plastic bodies, while others take them merely as unmeaning collocations of lines or as two-dimensional figures. Similar differences were found by Karpinska in the apperception of three-dimensional objects[1]. Katz suggests a distinction between 'peripheral' and 'central' types of apprehension, according as the visual impression is apprehended as such, or is modified by subjective additions leading to interpretation.

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[1] M. Dessoir: *loc. cit.* 453.

[2] M. W. Calkins: "An attempted Experiment in psychological Aesthetics," *Psychol. Rev.* VII. 586–7.

[3] *Ibid.* 590–1. The same observation was made by Müller (above).

[4] M. Dessoir: *l.c.* 447.

[5] O. Külpe: *l.c.*

[6] v. Ritook: *l.c.*

[7] *Ibid.* 371–2. v. Ritook observed that one of her subjects habitually based his appreciation on the formal side of pictures. Müller observed that an awareness of formal features and of the artist's intentions and treatment occurred only with subjects having an interest in, and some experience of, Art, but was lacking in aesthetically uneducated subjects.

[8] D. Katz: "Über individuelle Verschiedenheiten bei der Auffassung von Figuren," *Ztsch. f. Psychol.* LXV. 161 ff.

As the result of an extensive series of individual tests of over 100 persons on their appreciation of single colours and simple colour-combinations[2], I found the existence of four clearly distinguishable types of apperception which I called respectively the 'objective,' 'physiological[3],' 'associative' and 'character' types. The peculiarity of the objective type is an impersonal attitude to the colour and a tendency to base appreciation upon its purity, saturation, luminosity, and in general upon its degree of conformity to a preconceived standard. The second type apprehends colour as an influence upon the personal mood and, presumably, the organism of the individual: colours are cheering or depressing, warm or cold, stimulating or soothing. For the third or associative type it is the power of the colour to call up associations or memory images of past experiences which determines the appreciation of the colour. In the eyes of the character-type, finally, colour possesses a mood, emotional character or temperament, even a personality. Colours are cheerful (as distinct from cheering), sad (as distinct from depressing), active, serious, reserved, reflective, brutal, jovial, funny, treacherous, stealthy, etc. Allegiance to one of these types—and everyone belongs to one or other—determines both the range and nature of the colour-preferences and the basis for the use of special criteria in dealing with colour-combinations. The types appear to be not merely momentary attitudes of the subject, but fundamental and permanent modes of apprehending and appreciating colour.

These same types were found to exist in the appreciation of musical tones, as resulted from a set of experiments on "Individual Differences in Attitude towards Tones[1]." It would appear from these tests that the term originally proposed for the second of the above types (the 'physiological') is too narrow and that 'intra-subjective' is a better description of that attitude. In addition, the differences of the material added to the results of the experiments with colours a number of most suggestive observations concerning the range and elaborations of the typical attitudes.

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[1] L. v. Karpinska: "Experimentelle Beiträge zur Analyse der Tiefenwahrnehmung," *Ztsch. f. Psychol.* LVII. 1.

[2] Edward Bullough: "The 'perceptive Problem' in the aesthetic Appreciation of single Colours," this *Journal*, II. 406 ff.; "The 'perceptive Problem' in the aesthetic Appreciation of simple Colour-combinations," this *Journal*, III. 406 ff.

[3] After the experiments of C. S. Myers (see p. 87, note 1), I would adopt the term 'intra-subjective' proposed by him, in place of 'physiological,' which was at best a makeshift and was found by him to be too narrow, when applied to musical experiences.

A last distinction of forms of apprehension has been pointed out by Aveling[2] between what he calls 'symbolic' and 'a-symbolic' apperception. While the 'a-symbolic' type apprehends objects and their meanings as individual, concrete things, the 'symbolic' subjects incline to see in them types or representatives of the species to which they belong. The tendency to see in the picture of a horse, not this individual particular horse, but 'Horse' in the abstract, depends naturally in part upon the kind of picture[3], but appears also and perhaps in the first place to be the outcome of a fundamental symbolising disposition of the subject.

It is clearly a problem of aesthetic research how and to what extent these various apperceptive classifications are to be reconciled. Binet's four types, the apperceptive habits described as analytic and synthetic, active and passive, objective and subjective, peripheral and central, symbolic and a-symbolic, and my four types show sufficiently certain resemblances to each other. In attempting to reduce them to a simple grouping, it has to be remembered that several of these types emerged not in reference to specifically aesthetic tests, but as the results of general psychological experiments. It is very probable, therefore, that several such distinctions may not belong to the aesthetic sphere at all.—Secondly, it is essential that proper criticism should be exercised towards the subjects themselves. While Dessoir's tests, for instance, dealt with subjects to whom the aesthetic attitude was familiar and in face of pictures habitual, Smith's experiments give no indication how far, if at all, his subjects reached an aesthetic appreciation of the slides. In aesthetic experiments it is desirable that not all the subjects should have the same habitual outlook, aesthetic or otherwise, so that the distinction between aesthetic and non-aesthetic behaviour should appear with sufficient clearness. At the same time, like must be compared with like and not with unlike. It is therefore necessary for the experimenter to be in a position to exercise a critical discrimination, based on general psychological, aesthetic and personal knowledge, between his various subjects. This discrimination will enable him to give due weight and value to the evidence supplied by subjects of a known aesthetic receptivity, compared with others to whom the aesthetic attitude is admittedly difficult or non-habitual[1].

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[1] C. S. Myers and C. W. Valentine: "A Study of the Individual Differences in Attitude towards Tones," this *Journal*, VII. 68 ff.

[2] F. Aveling: "The Relation of Thought-process and Percept in Perception," this *Journal*, IV. 227.

[3] *Ibid.* 227.

With such an evaluation of the testimony of subjects it is not improbable that the apparent diversity of types will prove much smaller than seems at first sight. At the same time the complexity of even the mere apprehension of an aesthetic object is evidently much greater than was previously suspected. The differences in aesthetic judgments were well known; varieties of associations were hardly a matter of surprise; the fluctuations in Empathy and the divergencies of its psychological working became evident as soon as analysis concerned itself with these processes. But the existence of apperceptive types has now introduced similar complications into the pre-empathic stages of appreciation. The one guiding principle, as a criterion of the aesthetic quality and value of the different forms of apperception, appears to be the conception of the *unification of the percept and the perceptual processes*. It is true that this is something of an *a priori* principle. But its importance appears repeatedly: in Dessoir's tests[2], in Müller's observations on children[3], on several occasions in v. Ritook's experiments[4], it came out in a peculiarly striking, because quite unexpected, manner in the experiments of v. Karpinska[5] on the perception of (three-dimensional) depth, where the plastic apperception of a really plane figure was said by some of the subjects to be far more unified and therefore aesthetically more effective than the view of the same figure as a plane design, which appeared scattered and disordered. It seemed to Miss Calkins[6] the main difference between an immature and a developed appreciation and it has been used for the evaluation of perceptive types in my colour experiments[7], supported by similar observations of Valentine[1]. The value of unification of the aesthetic experience, subjectively as a certain homogeneity or unity of the processes of appreciation, and objectively as the unification of the object by composition, content, or emotional and associative import, is so extensively attested by experience that it seems difficult to over-rate its importance. The various means by which it is attained, and the range of its application, especially to temporal forms, are urgent problems for further experimental research[2].

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[1] This is a general criticism of employing subjects whose knowledge or interest leads to a uniform mental attitude. It affects to some extent even v. Ritook's experiments. It does not affect Dessoir's tests, since his object presupposed a certain uniformity of attitude on the part of his subjects. On the other hand, it renders experiments such as those of Utitz (*Grundzüge der aesthetischen Farbenlehre*, 1908) practically valueless, as I have pointed out elsewhere (this *Journal*, III. 444, footnote 1). Admirable in the matter of discrimination are Müller's experiments, as already stated.

[2] Dessoir: *l.c.*

[3] F. Müller: *l.c.*

[4] v. Ritook: *l.c.*

[5] v. Karpinska: *l.c.* Cf. also Bühler, *Die Gestaltwahrnehmung*, 1913, 176. Here Prof. Külpe, acting as one of his subjects, contrasted what he called the 'aesthetic' apperception of the figures as synthetic, with the 'geometrical' or analytic apperception.

[6] Calkins: *l.c.*

[7] Bullough: *l.c.*

At this point I may perhaps be permitted a short digression, as its substance is so closely connected with the question of apperception. Though the apperceptive differences are of relatively recent discovery, the general importance of the process of apprehending an aesthetic object was realised much earlier and has occasionally been made the basis for general aesthetic theories. Two groups of such theories may conveniently be mentioned here. We may call those of the one 'intellectualistic,' of the other 'functional' theories.

The intellectualistic theories need not detain us. Their essence is that an object is aesthetically effective in proportion to the ease with

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[1] C. W. Valentine: *The Psychology of Beauty* (The People's Books), rev. ed., 1919, 101.

[2] By way of illustration, the following correspondences might be suggested:

(*a*) My four types in reference to Binet's:

objective	... *descripteur; érudit.*
intra-subjective	*observateur* plus *émotionel.*
associative	... *observateur* plus *émotionel* plus *érudit.*
character	... *émotionel* plus *observateur.*

(*b*) Binet's types in reference to mine:

descripteur objective (1).

$$\left.\begin{array}{ll}\left.\begin{array}{l}\textit{observateur}\\ \textit{émotionel}\end{array}\right\} & \text{character}\\ \textit{érudit} & \text{objective (2)}\end{array}\right\}\text{associative.}$$

A considerably better correspondence is obtainable between my four types and Müller's six, expanded from Binet's four.

Another grouping is the classification under Unification and Dissociation as follows:

Unification		*Dissociation*
synthetic		analytic
central		peripheral
subjective		objective
observateur		*descripteur*
	émotionel	
	érudit	
character		
associative		associative
	intra-subjective	
objective		objective.

which it is grasped. This is at bottom the standpoint of all theories resting upon formal features, ratios, proportions, the golden section, etc. The recent experiments of Bühler[1] on the perception of spatial forms have indeed shown the importance of proportion for the apprehension of linear complexes. But what is open to question is whether such a facility of apprehension and the consequent satisfaction is necessarily aesthetic, as it clearly occurs not merely in the aesthetic sphere, but just as constantly outside it. That ease of apperception—even the purely intellectual facility in apprehending meanings and historical and technical significance—may be a source of satisfaction and of pleasure, can hardly be contested, but the pleasure is by no means generally aesthetic. When it is, the fact can presumably be accounted for by the existence of an aesthetic adaptation which colours all the subsequent processes[2].—The once prevalent idea that familiarity is a source of aesthetic satisfaction, is of the same category, but manifestly untrue. For familiarity, as we know, more frequently breeds contempt.

The functional theories assert that certain objects are aesthetically pleasing, because they offer the best conditions for the easy and natural functioning of the physiological processes required for their apperception. The most prominent theory of this kind is the so-called 'eye-movement' theory which goes back to the eighteenth century and secured a fresh lease of life in the second half of the nineteenth through the 'Wundt-Lamansky law' of eye-movements. The eye-movement theory maintains that owing to the disposition of the muscles of the eye-ball certain movements are more easily carried out than others; and that therefore lines which 'guide' the eye in the direction of these naturally easy movements are more pleasing than lines which cause the eye to execute more unnatural and difficult movements. The directions of easy movements are, according to the Wundt-Lamansky law, straight vertical and horizontal, and curved oblique lines. Sudden changes of direction, or straight oblique lines conflict with the muscular apparatus of the eye-ball and tend to produce torsions and uncertainties in the eye-movements. The consequence for Aesthetics would appear to be that linear forms should employ *straight* verticals and horizontals, but *curved* obliques; undulating lines, but not patterns involving constant changes of direction, like zig-zags; and that such figures would be pleasing as a result of the facility with which 'the eye follows' their lines. For the same reason symmetrical forms are said to have the advantage of demanding a balance or correspondence of eye-movements for the apprehension of the right and left halves[1].

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[1] Bühler: *l.c.* (see p. 88, n. 5).

[2] Cf. v. Ritook: *l.c.* 365.

The theory rests, of course, on the assumption that the eye follows the lines and contours, as do all those unfortunate metaphorical expressions speaking of 'the eye being guided or running along certain lines.' The eye does nothing of the kind. The whole assumption is false. On the contrary; the extremely painstaking and elaborate experiments carried out by Stratton[2] and Judd[3] have shown that the eye does not follow the lines, not even when the subject is told to follow them with his glance and is quite convinced that he is doing so. "In examining the negatives (of photographs taken of the positions of the eye)—says Stratton—one is struck by the almost grotesque unlikeness between the outline observed and the action of the eye in observing it. For the most part the eye moves irregularly over the figure, seeking certain points of vantage from which the best view of the important features may be obtained. And these positions are marked by the eye momentarily resting there...But even these points of rest are not so arranged as to supply of themselves a rough sense of the form perceived[4]...." The same results were obtained from experiments with symmetrical forms: "As regards the question of symmetry of the movements of the eye, in some instances the negative (photographic record) displays a rude balance...But it must be said that even the most symmetrical of these records can be called so only by courtesy, for they signally lack that exactness of correspondence of part to part which we demand in the objective form[5]."

With these observations the eye-movement theory may be regarded as definitely disposed of. The same fate may be anticipated for various other theories which make the easy functioning of muscular or organic action in the apprehension of beautiful objects the basis of aesthetic values, though, as Stratton remarks[6], some of these may be more difficult to disprove by actual observations.

A much more elaborate theory, combining in a sense the functional and intellectualistic conceptions, is the 'apperceptive principle' of Lipps[7].

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[1] See, for instance, Baldwin Brown: *The Fine Arts*, 212–4, London, Murray, 1902.

[2] C. M. Stratton. "Eye-Movements and the Aesthetics of visual Form," *Philos. Stud.* XX. 336 ff.; "Symmetry, linear illusions and the Movements of the Eye," *Psychol. Rev.* XIII. 82.

[3] C. H. Judd: "Studies in Eye-Movements," *Psychol. Rev. Monograph Supplement*, VII. 1; *Yale Psychol. Stud.* I. 1.

[4] Stratton: *Psychol. Rev.* XIII. 94.

[5] *Ibid.* 94.

[6] *Ibid.* 95.

[7] Th. Lipps: *Aesthetik*, I. 1903, 10 ff.

He tries to establish a connexion between pleasure, value and apperception by treating the last named as the principle of pleasure (in the widest sense). "Pleasure arises to the extent to which a psychical process (or its object) finds favourable conditions for its apperception (using 'apperception' in the somewhat specialised sense of 'appropriation' or incorporation into the general psychical life of a person)[1]." Apperception is, so to speak, the mutual fitting into each other of the object and the conditions of its apprehension, and is in so far a source of pleasure. Since "there is the tendency of the soul to comprise all data, all multiplicity into a unity or uniform whole in the act of apprehending or attending or apperceiving[2]"—a law which he calls the 'principle of unity[3]'—objects which meet this tendency halfway, because they offer objectively a unity, are apperceived with greater facility and possess therefore a higher aesthetic value than others which do not offer the same favourable conditions. Hence, Lipps contends, the aesthetic superiority of formal features, rhythms, symmetry, balance, contrast, or proportion, which he with great ingenuity and wealth of illustration brings under the common heading of 'unity in variety'[4].

In spite of the emphasis laid upon the pleasure-value of the aesthetic experience, Lipps' account, apart from the peculiarities of his terminology at times, seems not unattractive. Unfortunately he presses the apperceptive foundations of his system into the exclusive service of his empathy doctrine which of all the empathy theories is perhaps the least happy in formulation.

ADAPTATION.

If relatively so little is known of apperception, we are almost entirely in the region of hypothesis concerning adaptation. There is no doubt that adaptation actually occurs, of course. Our ignorance refers mainly to the special conditions under which it takes place, its character and composition and the question to what extent different types of it can be distinguished. This lack of information is largely due to the fact that adaptation is by its very nature difficult to deal with experimentally and that it seems to involve some of the obscurest factors of 'temperament' and 'personality'; chiefly, however, to the accident that it has never yet been studied as closely as the other stages and factors of aesthetic appreciation. Yet there is a growing sense that here, if anywhere, we may find that bedrock upon which the whole superstructure of aesthetic experience—of appreciation as well as artistic production—ultimately rests.

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[1] Th. Lipps: *l.c.* 11. [2] *Ibid.* 12. [3] *Ibid.* 19.

[4] In accordance with his bias against all 'empirical' factors, Lipps excludes expressly the *intellectuale Lust*, arising from an intellectual knowledge of unity (*Aesthetik*, I. 22 ff.). The obvious difficulty presented by simple objects (colours, tones, etc.), he tries to meet by the desperate expedient of grouping even these under the principle of 'unity in variety' by reference to the vibrations constituting colours and tones (*Aesthetik*, I. 27 ff.).

The interest in adaptation is of relatively recent date. In 1905 Segal[1] observed in carrying out a set of experiments with simple linear forms that the same subject found oblique straight lines sometimes pleasant and sometimes unpleasant, occasionally on one and the same day. The explanation of this response was found to be that the subject regarded these oblique lines, sometimes as imperfect horizontals or verticals (and found them unpleasant as failures to be either one or the other), sometimes as a flying arrow. This latter apperception always produced a pleasant effect. The question arose, what prompted the subject to adopt now this, now the other interpretation? Apart from the natural ambiguity of simple forms, Segal saw the reason, first "in the general, hardly definable, fundamental character of the individual, that which we call his individuality or personality," and secondly in "the momentary mood and adaptation (*Bewusstseinslage*), immediately preceding the apperception of the object[2]."

This is to my knowledge the first explicit reference in aesthetic literature to any such fact as adaptation. However imperfect and contestable in details, Segal's observation was a discovery of the greatest value. It is true that he believed this adaptation as well as the interpretation based upon it to lie outside the sphere of the aesthetic experience, to be a 'pre-aesthetic' process common to all apprehension. Segal's view would, I think, hardly be shared at the present time, since Empathy, the real object of his experiments, has lost considerably in vogue, and the pre-empathic stages of appreciation have attracted more attention. Adaptation, in the sense I venture to give to it, is undoubtedly connected with the general temperament and personality of an individual. The personality as such is, of course, as Segal said, 'pre-aesthetic.' But the adaptation, though a reflexion or manifestation of the personality, belongs unquestionably to the aesthetic sphere. As the result of my colour-experiments and the discovery of the perceptive types, I believe that adaptation as well as apperception are not merely a colourless basis for the later stages of appreciation; adaptation is not simply a gateway through which one walks into the garden of aesthetic delight. It is rather a coloured glass through which one sees all objects steeped in a special colour, and this colour is precisely the aesthetic quality which the objects seem to display. It is, irrespectively of the objects themselves, an *aesthetic* adaptation and represents the whole groundwork on which the aesthetic experience rests. It is needless to say that this is a view so different from that of Segal that it would be unfair to use it as a criticism of his original conception.

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[1] J. Segal: "Über die Wohlgefälligkeit einfacher räumlicher Formen," *Arch. f. d. ges. Psychol.* VII. 53 ff.

[2] *Ibid.* 108.

Before explaining more in detail the significance of adaptation for aesthetic theory thus hinted at, a few words are needed to indicate its nature. In a general way, adaptation implies, positively, the attending to the various aspects presented by the object (either spontaneously or by an act of voluntary attention) and the opening of all the channels of reminiscence, associations, historical and technical knowledge, inferences, emotional resonances, organic sensations, etc., in so far as they may serve to impart an adequate meaning to the object and to lead to an adequate interpretation of it on the lines predetermined by the artist. Negatively, it involves the cutting out of all irrelevant trends of thought, extra-aesthetic points of view, the selection of valuable associations and the suppression of useless or disturbing dispositions, especially of such as by their intensity or nature would lead to deviating associations, purely personal interests, or reminiscences splitting the interest in the object or destroying the unity of attention and appreciation.

It is one of the tasks of experimental research to investigate the chief characteristics of aesthetic adaptation, and by way of suggestion, I venture to put forward four features which I believe to be peculiar to it. In so doing I hope to make clearer the nature of aesthetic adaptation as it presents itself to me, and the function which I would attribute to it in aesthetic theory.

1. Like all adaptation, it involves a large element of expectancy. But this expectancy is curiously vague and undefined. It is true that an adaptation to a special art or even type of work to which the prospective aesthetic experience belongs sets certain limits to this vagueness. We know whether to expect a comedy or a tragedy when we go to the theatre, a sonata or an oratorio when we attend a concert. But it is not—and this seems to me an important characteristic, complicating this indefiniteness—the *typical* experience, but the *individual*, concrete experience which is the object of expectancy[1]. It is not Tragedy that

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[1] This would run counter, as stated immediately below, to *normal* adaptation which is for typical, generic experience rather than for individual. Cf. the general rule (stated by Külpe, *Grundriss der Psychologie* (1893)), that generic experiences are better remembered than individual. This also affects reproduction.

we go to see, nor even 'Shakespearean Tragedy,' but this or that particular tragedy, on this particular occasion—whether we have ever seen it before or not.

2. In fact, our adaptation to the individual and concrete experience is the more effective and adequate, the less it operates with preconceptions and typical standards, with the possible affinities or resemblances of the work, or with its concordance with habit or tradition. While the value of ordinary experience lies largely in its confirmation of our expectations, it is characteristic of aesthetic experiences to surprise us by their originality. This seems to be the result of the combination of the marked individualistic expectancy of our prospective experience with —however paradoxical it sounds—the indefiniteness of our adaptation. This vagueness, the very reverse of dogmatism, but an open-mindedness, even a kind of trustful humility is described somewhere by Schopenhauer, by likening our attitude to that of a humble subject approaching his king and waiting in silence until he should deign to speak to him.

A failure in these two respects is well known as the cause of many a miscarried effect. A pronounced typical expectancy either fails to see the concrete novelty or misses the—wrongly expected—correspondence with traditional or habitual forms. Dogmatic standards and demands are only another form of the same fault of adaptation, the result of prejudice, narrow-mindedness or merely of set habits and immobility of mind.

3. In the third place, the general feeling-tone of our adaptation is markedly pleasant. But it is important to note that this pleasantness is centred not upon the expectation of a *personally pleasant* experience, but upon the *value* of the prospective experience.

4. Closely connected with this last point, I must mention the perhaps most important feature of the aesthetic adaptation, namely its impersonal or rather non-personal character.

There is a fairly general agreement about this characteristic, although there is as yet no concensus on its accurate description or its full significance. Personally I incline to regard this as the most important and ✓ fundamental aspect of the aesthetic, as distinct from other, adaptation. Its main feature is a divorce from all directly practical needs and ✓ functions and from the personal relevance of the object: it is neither agreeable (as a personal affection), nor useful (as fulfilling a practical function), nor directly ethically valuable (as serving a socially sanctioned, remotely personal purpose). At the same time, our adaptation is not *im*personal, in the sense of scientific impartiality or mere intellectual curiosity. It is rather a non-personal relation to the object; its significance does not affect me in my everyday experience, yet it does not lose touch with either the emotional sphere or a generally human interest; and though the experience has a certain unreality or, if you like, fictitiousness about it, it yet does not fail to obtain a full response of the personality to which it appeals. This curious dualism of attitude, a paradoxical combination of impersonal and intensely personal reaction, I have tried elsewhere to describe as ‘Distance[1].” Others have tried to formulate these puzzling facts by reference to ‘assumptions[2]’ in place of existential judgments, or an attitude of ‘supposal’ or make-believe, as the basis of aesthetic response. This view lays successful stress upon the imaginative element involved in the aesthetic attitude, but is held to find difficulties in the occurrence of an aesthetic appreciation of Nature and of actual events in real life. The theory of ‘imaginative emotions’ (*Phantasiegefühle*)[3] attempts to meet this objection and to obviate some obscurities in the ‘assumption theory.’ Another view treats the aesthetic attitude as the result of a ‘play-adaptation.’ This attempt has certain drawbacks, inherent in the correlation of Art and Play, a theory of Art which goes back to the eighteenth century but was revived in the last decade of the nineteenth century. The ‘play-adaptation’ has possibly, as the opposite to a ‘reality-adaptation’ (*Wirklichkeits-Einstellung*), to be distinguished from a ‘non-serious-adaptation’ (*Nicht-ernst-Einstellung*) as has recently been pointed out[4].

All these attempts to define this characteristic feature of aesthetic adaptation is a great step in advance over the old ‘illusion’ theories based on deception or probability or on the opposition between the work and some ‘copied’ reality. Whatever the ultimate formulation will be, I feel sure that we shall find here one of the marks, perhaps *the* mark, distinguishing aesthetic adaptation. It may be this feature which will prove to be the condition of all the subsequent elaborations and colourings of aesthetic, as distinguished from other types of, experience. It is certainly this non-practical, non-personal character of adaptation which is responsible for the seemingly unreal quality of the emotions of Empathy and for the 'semblance' of the whole experience. It is the 'filtering' process exercised by it which permits the associations, freed from their purely personal significance, to enter as ingredients into aesthetic appreciation and to enrich meaning with a quasi-personal interest. It establishes the distinction between the beautiful and the agreeable, marks off the tragic from the sad and the humorous from the merely ludicrous. It is, in short, that unifying element which renders aesthetic appreciation homogeneous and imparts to it its singleness of point of view, quality and appeal.

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[1] Edward Bullough: “Psychical Distance as a Factor in Art and as an aesthetic Principle,” this *Journal*, v. 87–118.

[2] A. Meinong: “Über Annahmen,” *Ztsch. f. Psychol. u. Physiol. d. Sinnesorg., Ergänzungsband*, 2, 1902; S. Witasek: “Zur psychologischen Analyse der Einfühlung,” *Ztsch. f. Psychol. u. Physiol. d. Sinnesorg.* xxv. 4 ff.; *Grundzüge der Aesthetik*, 1904.

[3] R. Saxinger: “Über die Natur der Phantasiegefühle u. Phantasiebegehrungen” (in Meinong’s *Gegenstandstheorie*, 1904). Saxinger tries to show that *Phantasiegefühle* are emotional states *sui generis*, not as Witasek maintained and Meinong implied—wrongly and unintentionally, according to Saxinger—always dependent on or equivalent to *Annahmegefühle.*

[4] M. Geiger: “Das Problem der aesthetischen Scheingefühle,” *Ber. d. ersten Kongresses für Aesthetik*, Berlin, 1913, 191.

It is important to distinguish from the question of the nature and constitution of aesthetic adaptation, the problem of its typical behaviour and distribution.

As research found the different types of apperception to occur with varying frequency among different types of persons, so it is clear—even from our superficial every-day experience—that aesthetic adaptation occurs in various degrees of frequency, stability and permanence in different individuals. And as different types of persons have come to be distinguished by their habitual mode of apperception, nothing would seem more probable than that persons would be found to fall into different classes according to the type of adaptation habitual to them. The perceptive types seemed to all appearance to be permanent. The same persons showed after a lapse of years the same characteristics of apperception and, allowing for the difference of another medium, the peculiar features of their type also in musical experiences. Even the possibility that these types are inherited is not to be altogether dismissed. All this goes to show that a special apperceptive habit is a definite and possibly fundamental asset of its owner. Without suggesting a necessary correlation between perceptive types and adaptation types, I cannot but think that similar divisions into types of adaptation would be possible, once sufficient experimental data were available.

At the same time the questions of spontaneity and stability of adaptation, both in concrete instances and as typical characters of different groups of persons, might find their answer. That adaptation can be attained voluntarily is well known; but in many people it is presumably a spontaneous process, in proportion as the aesthetic adaptation is natural or habitual with them. It is a special object of research, how far disturbing factors can be either voluntarily overcome or are spontaneously set aside. This leads to the question of stability of adaptation, *i.e.* of the greater or lesser facility of different persons to maintain an aesthetic adaptation for any length of time. In reference to both these questions it must be borne in mind that adaptation may be secured or assisted by the object, by its power to *compel* aesthetic adaptation. This is, of course, what makes an object into a work of Art or a source of aesthetic appreciation. The temporal arts in particular dispose of special means to initiate an adaptation, by 'introductions,' opening chapters, the 'exposition' of a play, an overture, etc. This advantage over the spatial arts which cannot prepare the spectator is, however, balanced by the disadvantage of requiring the maintenance of an adequate adaptation for a considerable length of time. It is here that the adaptation is exposed to fluctuations, deviations and apparently especially to oscillations between the object and the subject's self and his reactive feelings.

If aesthetic adaptation proves to be as permanent and as fundamental a form of behaviour as I believe it to be, a new prospect opens out for aesthetic theory. In that case aesthetic adaptation, with all its profound importance for the subsequent stages of the aesthetic state, comes to represent a whole system of dispositions and tendencies orientated towards aesthetic experiences, which is more or less habitual to certain persons, and less frequently realised by others for whom it is attainable only in response to stronger, more explicit stimulation. This disposition or 'polarisation' towards aesthetic experiences constitutes the 'aesthetic consciousness'—in the same sense as we speak of a religious 'consciousness'—within which and through which objects and experiences are aesthetically appreciated. That this kind of special outlook exists, admits of no doubt as a matter of broad, every-day experience. Older Aesthetics used a stock-illustration to show the 'subjective' nature of beauty, by describing the view taken of a landscape by various hypothetical visitors, a shop-keeper, an engineer, an estate-agent, a medical man, a prospector, a farmer, and contrasting their attitude with that of the painter. The difference of outlook between the painter and all the rest is the difference between the 'aesthetic' and the 'practical consciousness.' It is a difference, if I may use my own terminology, guaranteed by the 'distance' of the aesthetic adaptation and attitude[1].

The same guarantee maintains the distinction between the ethical and scientific attitudes on the one side, and the aesthetic attitude on the other. Both the practical value of moral conduct and the impersonal curiosity of science stand opposed to the balance of distance of the aesthetic consciousness. It is time at last to discard the time-honoured confusion of goodness, truth and beauty.

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[1] Cf. for an indication of the differences of attitude, the opening chapter of *L'Intelligence sympathique* of G. Finnbogason, Paris, 1913.

These and similar distinctions belong really to the domain of a Philosophy of Art. The important point to seize at the moment is that until the conceptions with which Philosophies of Art are wont to operate are illuminated by actually and accurately observed experiences of many persons, instead of being vaguely apprehended and rashly generalised personal introspections of their authors, little good will be done by interminable discussions of such topics. All I desire to point out here is that we have reason to believe that there *is* a distinction which imparts to the aesthetic consciousness a character and significance of its own, irreducible to the value of the other attitudes. It is the concern of aesthetic theory and experiment to define and analyse this consciousness, to ascertain the conditions of its realisation and the range and typical features of its occurrence. If this attempt proves successful, theory will have attained to a fundamental generalisation in Aesthetics. For this consciousness, as a mental state *sui generis*, is the level of psychic life which must be reached and maintained, if the aesthetic state is to be realised. Appreciation will then not be dependent upon the presence of this or that mental process, upon pleasure or emotion, upon empathy or association or imagery or organic sensations. No; any mental state, from the obscurest organic feeling to the most abstract intellectual operation, may form the object of aesthetic appreciation—*provided it occurs within and on the level of the sphere of the aesthetic consciousness*[1]. Given the aesthetic point of view, nothing in life or nature or Art need be aesthetically ineffective, as in point of fact, nothing is entirely barren as a possible source of aesthetic delight.

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[1] Objects and experiences differ, of course, considerably in the ease with which they can be maintained on the aesthetic level. The statement holds only in theory. In practice there will always be instances in which it proves impossible. It is in practice for this reason that Art is superior to 'Nature' in aesthetic value and effectiveness.

CRITICAL NOTICE.

BY G. UDNY YULE.

The Essentials of Mental Measurement. By WILLIAM BROWN and GODFREY H. THOMSON. Cambridge, at the University Press. 1921. pp. 216. Price 21*s.* net.

This is a revised and expanded edition of Dr Brown's well-known little work, bearing the same title, which was published in 1911 and has now been for some time out of print. The format has been increased from demy to royal 8vo; the number of pages from 154 to 216, and the price (alas!) from 3*s.* 6*d.* to 21*s.* net. Dr Brown, owing to his time being taken up with Army medical work during the war, was unable to take any further part in the development of applications of statistics to psychology after 1914 and the revision of the work, we gather, is almost wholly due to Professor Thomson.

The additions and revisions are very considerable, and for the benefit of those who know the old edition it may be convenient to describe them. In *Part I. Psychophysics* two new statistical chapters have been interpolated. Chapter I on Mental Measurement is now followed by a chapter on "The Elementary Theory of Probability" which covers a very wide field—frequency distributions, averages and measures of dispersion, the standard error of the mean, the binomial distribution, and the normal curve. The former Chapter II on the psychophysical methods succeeds this, and then comes the second new chapter on "Skewness and Heterogeneity in Psychophysical Data," in which Professor Pearson's test of goodness of fit is described, his system of skew curves, and the analysis of a compound distribution into two normal curves. The former *Part II. Correlation* has been even more largely expanded. The Introduction and the chapter that followed on the Mathematical Theory of Correlation remain the same in title though the matter has been revised. Chapter VII, on the Influence of Selection, covers not only the effect of selection on standard deviations and correlations, but also multiple correlation, 'spurious' correlation ('spurious' is an unfortunate term which is better avoided) and a short account of the variate-difference method. In Chapter VIII the authors consider the correction of raw correlation coefficients for irrelevant factors and for observational errors, and in the two final chapters the theory of general ability and the hierarchy. There are some useful tables at the end of the book and excellent bibliographies.

The exposition, it may be said at once, is admirably lucid, as one would expect from both authors, though it is at times extremely compressed; the student will often have to refer to other volumes or to the original memoirs if he wishes adequately to study some of the methods of which hardly more than an outline is given. Certainly the volume is one which should be in the hands of every investigator who is using the methods considered. And yet—if I were asked whether the volume was entirely a satisfactory one I should feel bound to answer, No. My objection is not to the manner of exposition, but to the matter chosen. This may seem an unfair objection: an objection merely to the authors (for I take it that both authors must be held responsible for the matter included) having given us one sort of book when I would rather that they had given us another. But I do not think that my particular objection is unfair. A book, a good book, should be written with one aim, one outlook. The present book is heterogeneous: it is in part a text-book and in part a polemical pamphlet. If the reader skips the prefaces, to avoid the initial note of controversy, he will find himself reading an ordinary peaceful treatise until he gets to Chapter VI or perhaps Chapter VIII. In Chapter VI the trumpets begin to sound, though gently and musically, about the method of ranks. But with Chapter VIII, and the correction of correlation coefficients for errors of observation, the *mêlée* is fairly started and Chapters IX and X continue the fight on the field of the hierarchy. It should not have been impossible to write an exposition in a less controversial spirit. It would have been perfectly possible, for example, to exhibit the methods of correction of a correlation coefficient for uncorrelated errors without criticism of Professor Spearman's use of the formulae, to emphasise the assumptions made, to show what happened when errors were not uncorrelated, and to illustrate the inapplicability of the assumptions (or of certain of them) in the few cases that have been tested. It would have been perfectly possible, in the same way, to have shown how different systems of correlations arise from specific assumptions as to the nature of the causation—variables which are linear functions of general, specific and group factors; the dice-throwing cases on which Dr Thomson has made himself a specialist; and so on—and to have shown the student how difficult an aggregate of correlations might be to interpret, without entering into controversy at all. References at the end of the discussion to show the reader how questions of interpretation had arisen would have been quite sufficient. In spite of the controversy it is pleasant to note the statement by Dr Brown in his preface that as an opponent he has learnt to admire the work of Professor Spearman to a very great degree, and regards his work in correlational psychology as epoch-making in its significance.

As briefly as possible, a few comments may be made on the points of controversy. First as regards the method of ranks, that is of ranging individuals in order as regards any quality, numbering them off, and taking the number assigned to each individual as the measure of his quality for purposes of correlation. The alternative is to assume that if the two qualities correlated could be measured the correlation between them would be normal, and to use an approximate formula based on this assumption. Dr Thomson summarises his views on this point most clearly in his preface. He makes the, possibly very valuable, suggestion that it may be practicable to approximate to the true distribution by estimating not only ranks, but the relative order of differences between the ranked individuals, of the differences between these and so on. Failing this, "the question is whether it is more probable that the assumed or the measured distribution corresponds to the actual unknown mental distribution" (p. vii). On this I would make two comments. In the first place, as it seems to me, the mental distribution is not merely unknown but totally indeterminate and it is impossible to speak of an 'actual' distribution, or of two intervals on the scale being equal or unequal, until the scale has been agreed on. Secondly, it is always possible to choose such a scale that the distribution of any variable taken singly shall be normal—though of course a scale so fixed is arbitrary and may be unnatural: but the assumption made in using Professor Pearson's formula for obtaining the 'true' correlation from the correlation for ranks is one that it is *not* necessarily possible to secure by any choice of scale. It is assumed not only that the distribution for each variable taken singly is normal, but also that *the correlation is normal.* A choice of scales such that the distribution for both variables is normal will only make the correlation normal, if it was originally of the type that I have elsewhere termed 'strained normal[1].' Given sufficient observations the existence of this type of distribution can perfectly well be tested in either of two ways. (1) A contingency table can be drawn up for the ranks, using at least three rows and three columns, and by the formulae of pp. 124–5 it can be seen whether this can be regarded as a normal distribution with the assigned correlation. (2) Each rank can be replaced by the equivalent normal deviation and the correlation then worked out between these deviations. As the validity of the assumption can be tested, there is no justification for using it without the test. It may be added that an objection may be brought against the use of any approximation which assumes normality, precisely of the same kind as is brought against one of Professor Spearman's methods by Dr Thomson—namely that, even if the assumption may be nearly true for a large aggregate of observations, it cannot be true for small samples. The method (2) suggested above is always applicable and may, perhaps, be of service—but it is not so brief as the pure ranks method. Given, however, the same number of observations, the standard deviations can be determined once for all.

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[1] Cf. my paper on Association in the *Journal of the Royal Statistical Society*, May 1912, Sections 39 et seq.

On the question of correction of correlations for errors of observation, I would like to point out that the formulae for correction only assume that errors and quantities measured are uncorrelated—that the correlation coefficient is zero—not that they are strictly independent, and the fact that the *correlation-ratio* in any given case is large is of no importance at all: the correlation-ratio on p. 160 ought not to be cited in this connection. In that case the correlation *coefficient* between errors and variables is practically zero: that is ali that is required, and surely, in spite of the authors' scepticism, what ought to be expected? No experimenting would be of any value at all if there were a liability to biased errors, which could not be eliminated by averaging, at different points of the scale. The correlation between simultaneous errors in the observed magnitudes of two variables is another matter; but here again to allege that such correlation cannot be eliminated by proper methods of experimenting seems to me to be taking a very hopeless attitude. Moreover I cannot help thinking that some blunder has exaggerated the evidence as to the existence of the latter type of correlation. I must decline to believe that with 43 observations the probable error of a product-sum can be of the order of one-half of one per cent. of the quantity dealt with, as alleged by the figures on p. 159: and I note that the formula given, which I do not recognise, is of the wrong order in n (the number of observations). On the general principle I agree, however, with the authors that no formulae assuming lack of correlation can be trusted for small samples and that "it is an essential of good work to use such samples that corrections to the raw values obtained are unimportant" (p. 161).

Now as regards the 'hierarchy' controversy, let me first take the points with which I find myself in agreement with the authors. I agree that the Hart-Spearman criterion is unsatisfactory, and I agree that the use of the 'correction' formulae has been dangerous and possibly misleading. But both these conclusions leave me rather cold, for in the very little work that I have personally done on the hierarchy question I used Burt's uncorrected coefficients and did not employ the criterion, and the results left no doubt at all in my own mind that the assumption of a general and of specific factors gave an extraordinarily good fit over the part of the table that I tested. Where Dr Thomson's work seems to me most important is in his proof that quite other assumptions may lead to true hierarchies, and yet other assumptions again to distributions which may in some degree ape the form of the hierarchy and, even with a sufficient number of observations, be mistaken for that form under careless or incorrect methods of examination. But I part company altogether with him in his view that hierarchical order "is the natural relationship among these coefficients, on any theory whatever of the cause of the correlations, excepting only theories specially designed to prevent its occurrence. It is the *absence* of hierarchical order which would be a remarkable phenomenon requiring special explanation; its presence requires none beyond what is termed chance" (pp. 183–4). Let us be clear, in the first place, as to what is meant by hierarchical arrangement: its accepted meaning—I think I am right in saying—is that

$$\frac{r_{km}}{r_{kn}} = \frac{r_{sm}}{r_{sn}}$$

for all values of k, m, n, and s. Dr Thomson seems at times to degrade the meaning of the phrase to imply no more than positive correlation between r_{km} and r_{sm} in any pair of columns. I use the phrase in the first sense, and cannot regard any table as hierarchical unless the rule holds—strictly for theoretical coefficients, or within the limits of probable error for observed coefficients. Now it is not necessarily true that "coefficients of correlation are themselves correlated" if by this is meant that r_{km} and r_{sm} are positively correlated for any pair of columns k and s. Fluctuations of sampling in the r's are so correlated and this may give rise to some trouble in interpreting observed tables, but not necessarily to a hierarchy in the strict sense. Nor—so far as I can see—

can 'chance' give rise to a hierarchy. Dr Thomson's mechanism of pp. 175–7 in the first place does not give rise to a hierarchy at all, but only to positive correlation between columns, and in the second place surely cannot be termed a 'chance' mechanism—all the important conditions were predetermined by Dr Thomson himself[1]. The result can no more be termed a 'chance' result than a cubical parabola can be termed a chance law if the values of the coefficients are determined by drawing cards from a pack. As a matter of experience, I may say, no one of the few cases that I have examined from non-psychological fields yields a hierarchical arrangement.

It seems to me impossible to agree then that the hierarchical arrangement found by some observers is a 'chance' result or does not call for explanation. The difficulty is that there may be more than one explanation. From the statistical standpoint Dr Spearman's explanation seems to me to be by far the simplest, but the judgment as to its validity must be based on other grounds.

It is not, or certainly not wholly, the fault of the authors, but the reading of this volume has produced in my own mind a feeling of extreme depression. "The Essentials of Mental Measurement": a fine, rhythmic, magniloquent title! A heartening picture it calls up too: *Essentials*... the real root of the matter here! *Mental*... we are really going to get at the *mind* now, look you! *Measurement*... absence of personal bias... precision...splendid! But what do we find? *Essentials*—a few experimental methods, rather difficult to interpret: a mass of statistical methods, which ought to be termed ancillary, not essential: and a great deal of controversy. *Mental*? well it is no good attempting to pretend that many of the data (or *any* of the data?) can be termed mental. Even the authors, with their title before them, can only hope that their arguments have sufficed to "show that purely psychical measurement is a *conceivable possibility*" (p. 9, my italics) and that their data are "indirect psychical measurements" (p. 10). And *measurement*! O dear! isn't it almost an insult to the word to term some of these numerical data *measurements*? They are of the nature of estimates, most of them, and outrageously bad estimates often at that. I have begun to visualise the title of this book as "The Essentials (?) of Mental (?) Measurement (?)."

Statistical methods, I say, should be regarded as ancillary, not essential. They are only essential where the subject of investigation is itself an aggregate, as a swarm of atoms, or a crowd. But here the subject is the individual, not the aggregate of individuals as such. This being the case, statistical methods are only necessary in so far as experiment fails to attain its ideal, the ideal of only permitting one causal circumstance to vary at a time. And it should always be the aim of the experimenter not to revel in statistical methods (when he does revel and not swear) but steadily to diminish, by continual improvement of his experimental methods, the necessity for their use and the influence they have on his conclusions. Statistical methods are not only ancillary; they are, to the experimenter, a warning of failure.

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[1] Compare Dr Garnett's article in this *Journal*, x. 242.

During the last year or two I have made it a practice to begin a short course of lectures to agricultural students, on some elements of statistical methods, by a lecture devoted entirely to showing how exceedingly bad agricultural experiments are, how this introduces the necessity for statistical methods in discussing the results, but does not obviate the primary necessity for improving the experimental methods. Is the latter warning unnecessary in the case of experimental psychology? I can only speak as a statistician with an exceedingly limited and sporadic knowledge of what is being done, but it seems to me that it is urgently necessary. The experiment (if it can be called an experiment at all) is often not only bad, but its badness seems to be accepted merely as a rather interesting fact, calling perhaps for the exercise of more elaborate statistical methods—a few partial coefficients of correlation—a few rather elaborate probable errors—and that is all. It does not seem to be recognised as primarily a condemnation of the experimenter, perhaps because the experimenter has never been exercised in the sciences where really accurate experiment is possible, so that he has no high ideal before him. Let me cite again from the volume before me one instance that I have mentioned already. On p. 160 it is seriously suggested that we cannot safely assume the error of measurement to be uncorrelated with the quantity measured. It does not seem to have occurred to the authors that if the suggestion were true (it was made perhaps under the domination of the Spearman-complex) it would be an utter condemnation of the science on which they were writing. Unless it were possible to assume a great deal more, namely that the average error was zero for all values of the quantity measured, it would hardly be legitimate to speak of the data as measurements at all: their value for any useful purpose would be nil.

The New Psychology that *was* the new psychology some five-and-twenty years ago has had to surrender the title to a yet newer successor. What has happened in the interval? Measurement does not necessarily mean progress. Failing the possibility of your measuring that which you desire, the lust for measurement may, for example, merely result in your measuring something else—and perhaps forgetting the difference—or in your ignoring some things merely because they cannot be measured. *Has* measurement in this field led to progress? The impression left by "The Essentials of Mental Measurement" is not cheering on the point.

PUBLICATIONS RECENTLY RECEIVED

The Letters of William James. Edited by his son, HENRY JAMES. London: Longmans Green and Co. 1920. Vol. I, pp. xx + 348. Vol. II, pp. xiii + 382. 42*s.* net.

These letters, admirably edited, are all of an intimate nature, revealing with wonderful clearness the attractive character of their writer—his general enthusiasm, his readiness to praise, his love of philosophy, his devotion to work and duty despite numerous interruptions owing to eye-strain, insomnia and fatigue. In them will be found little concerning the development and nature of his views on philosophical or psychological problems. When a medical student, only 21 years old, he wrote that of all departments of Medicine, that to which Dr Prince devotes himself [Psychiatry] is, he would think, the most interesting (I. 44). Writing in 1867 from Berlin, aged 25, he says, "it seems to me that perhaps the time has come for psychology to begin to be a science" (I. 118). Nine year's later he "organised the beginnings of the psychological laboratory" (I. 179) at Harvard, and two years after that—during his honeymoon—he began on his *Principles of Psychology.* Of this he wrote, before its publication in 1890, "I have to forge every sentence in the teeth of irreducible and stubborn facts. It is like walking through the densest brush-wood" (I. 225). "I have written every page four or five times....If there is aught of good in the style, it is the result of ceaseless toil in re-writing." After the publication of the *Principles,* James's main interests veered to philosophy. "My experimental aptitude," he had confessed to Stumpf (I. 249), "is but small"; and now (in 1896, aet. 54) he writes to Flourney, "*I* have got rid of the laboratory for ever....The results that come from all this laboratory work seem to me to grow more and more disappointing and trivial. What is most needed is new ideas. For every man who has one of them one may find a hundred who are willing to drudge patiently at some unimportant experiment" (II. 54).

Psychology and the Day's Work. By Professor E. J. SWIFT. New York: Charles Scribner's Sons. 1918. Pp. ix + 388. $2.00 net.

A review of this ably written and interesting book is impossible, so wide is the field which it covers. The expert psychologist will find in it little to criticize and little that is new beyond the illustrations, abundantly quoted by the author from relatively unfamiliar sources. The ten chapters deal respectively with organization for mental efficiency, thinking and acting, habit in preparation for efficiency, the psychology of learning, fatigue and its psychology, curiosities of memory, memory and its improvement, the psychology of testimony and rumour, our varying selves, and the psychology of digestion. Thus, as he explains, "the writer has tried to select types of conduct, as well as phases and causes of behavior, that are fundamental to thinking and acting, whether in the life of social intercourse or in the business and professional world." To the general public engaged in these spheres of intercourse the book may be confidently recommended.

A Young Girl's Diary. Prefaced with a letter by SIGMUND FREUD. Translated from the German by EDEN and CEDAR PAUL. London: Allen and Unwin. 1921. Pp. 272. 12*s.* 6*d.* net.

This book, for obvious reasons specifically addressed to doctors, parents, teachers and students of psycho-analysis, purports (without adequate evidence) to be a diary written by a young Austrian girl of the upper middle class between her twelfth and fifteenth year. It will appeal strongly to those convinced of the importance of sexual thoughts in early life.

In Search of the Soul and the Mechanism of Thought, Emotion and Conduct. By Dr BERNARD HOLLANDER. London: Kegan Paul. N.D. Vol. I, pp. x + 516. Vol. II, pp. vii + 361. 42*s.* net.

The first of these two massive volumes is historical, and finally leads to a vigorous defence of Gall and of phrenology. The second deals with 'ethology' (the science of character), the mental functions of the brain, genius, insanity and crime, and the "unexplored powers of the mind." The following quotations serve to illustrate the writer's qualifications and standpoint: "The greater the strength and activity of a mental quality, the greater the pleasure attending the exercise" (II. 5). "It has been shown in the previous chapter that the pleasurable passions are connected with the front part of the brain. In this chapter it will be shown that the depressing emotions are connected with the posterior part of the brain" (II. 123). Dr Hollander seeks to relate melancholia to the functions of the parietal lobe, mania to those of the base of the temporal lobe, "active sexual propensity" to "a large nape of the neck," etc. He gives an elaborate account of the now completely discredited N rays, and he "refers to the probable existence of a human aura" (II. 301), and to the "undoubted communication...both in the waking state and the hypnotic state between mind and mind otherwise than through the known channels of sense" (II. 312).

A Study of Women Delinquents in New York State. By M. R. FERNALD, M. H. S. HAYES, A. DAWLEY and others. New York: The Century Co. 1920. Pp. xviii + 542.

"The purpose of the present study," published by the Bureau of Social Hygiene, "may be stated as an attempt to furnish a scientific basis for the conceptions regarding women offenders, through an investigation concerning the distinguishing characteristics of women convicted of either serious crimes or minor offenses in New York State" (p. 5). The first comparison attempted was between "one group of women delinquents and women in general"; the second was between the women offenders and "similar groups of men offenders." Unfortunately, reliable data were often lacking with which those obtained from the women offenders could be compared. Consequently, as the authors state, they are compelled to "offer data on one side of the comparison without [always] being able to secure the complementary information on the group to be compared" (p. 8). Thirdly, they divided the women offenders of the group studied into various classes, according to duration of delinquency, age at first conviction, drug habits, physical data, birth-place, colour, family life, educational opportunities, mental capacity, occupational history, sex irregularity, etc.; and they attempted to study the distinguishing characteristics with varying methods of classification. The statistical treatment appears to have been well carried out. The psychological examination was conducted on about 500 women, based on the Yerkes-Bridges Point Scale, the Stanford-Binet, the Woolley and other tests. The social investigation was so planned as "to obtain from each woman delinquent studied an oral statement giving the details of her personal history and her background" (p. 61), the information thus given being verified by other data and inquiries carefully recorded, and the tendency, if any, being noted towards misrepresentation or reticence. The authors' general conclusion is that far too little importance has been of late attached to 'nurture,' excessive stress being laid on 'nature.' They believe that 'environment' is shown by their data to be a more potent cause of delinquency than 'intellectual ability.' But they do not consider whether other defects, of affective or conative character, are correlated with crime.

Religion and the New Psychology: a psycho-analytic Study of Religion. By W. S. SWISHER. London: Routledge and Sons. N.D. Pp. xv + 261. 10*s*. 6*d*. net.

"There is an increasing body of literature," the author announces in his foreword, "which deals with the new psychology and the psycho-analytic principle." This 'body' is increasing at such a rate as to lack substance and significance. A less superficial and more judicious examination of the psychological bases of religion, with a really critical application thereto of the psycho-analytical standpoint, is greatly needed. But Mr Swisher's book is far from meeting this need.

Mysticism, Freudianism and Scientific Psychology. By Professor KNIGHT DUNLAP. St Louis: L. V. Mosby Co. 1920. Pp. 173. $1.50.

Prof. Dunlap argues that there is at the present day a dangerous increase of mysticism, especially of what he calls 'quasi-' and 'pseudo-mysticism,' which includes "telepathy, clairvoyance and other supernatural methods of acquiring information or knowledge." Then he points out (p. 40) that mystical expressions are closely related "to physiological conditions of a distinctly sexual nature" and "that the description of ecstasy in many cases would pass equally well as a description of the sexual organism." Finally, he describes psycho-analysis as "one of the most important if not the most important mystical movement of the nineteenth century" (p. 44). He holds that "...psycho-analysis is essentially antagonistic to scientific psychology," that "in the Freudian system appears the fundamental anti-scientific postulate of mysticism: a form of knowledge—consciousness—which yet is not consciousness....To this mystic knowledge...is ascribed an importance far above that of consciousness itself" (p. 89). The unconscious mind, then, is, for Prof. Dunlap, 'mystical' and 'anti-scientific.' He holds that it "is almost the exact correspondent of the philosophical mystic's third kind of knowledge" (p. 46), additional to the knowledge obtained by perception and inference. "The whole poisonous vine [of psycho-analysis], with its tendrils threatening to grasp and choke all forms of learning...is rooted in the assumption of an 'unconscious' which is not merely the nervous system, and not merely something which isn't conscious at all, but is truly a mystical *third kind of knowledge*" (pp. 162, 163). Comment is superfluous. Although such hostile prejudice and misunderstanding make just, reasonable criticism on the part of the author impossible, nevertheless illustrations of various absurdities and rash conclusions adopted by psycho-analysts occur scattered throughout the book.

Standard Method of Testing Juvenile Mentality. By N. J. MELVILLE. Second enlarged edition. Philadelphia and London: J. B. Lippincott Co. 1920. Pp. vii + 162. 12*s.* 6*d.* net.

This book describes the general procedure to be adopted in gathering and analysing mental tests, and gives a standard method of applying the Binet-Simon scale, with note of the various modifications adopted by Goddard, Yerkes, Stanford, etc. The appendix includes tests for persons above fifteen, instructions for giving the Porteus maze tests, etc.

Die Intelligenz der Kinder und Jugendlichen und die Methoden ihrer Untersuchung. Von Dr WILLIAM STERN. Leipzig: J. A. Barth. 1920. S. xi + 335. M. 48.

This is issued in place of a third edition of the author's *Intelligenzprüfungen an Kindern und Jugendlichen.* It is divided into four sections, of which the first deals with the nature, varieties and development of intelligence, the second with the tests of the separate functions of intelligence and with its general measurement, the third and fourth with the applications of such tests to defective children and for the selection of those best fitted to proceed to higher education.

Einführung in die Metaphysik auf Grundlage der Erfahrung. Von Professor G. HEYMANS. Leipzig: J. A. Barth. 1921. S. vi + 364. M. 56; geb. M. 61.

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AFFECT IN THE DREAM

BY W. H. R. RIVERS

Affect in the dream and the concept of censorship. The nightmare as a failure of wish-fulfilment; character of affect due to this failure; partial failure and degrees of unpleasant affect. Infantile character of nightmare. Influence on affect of mental level action in the dream

IN an earlier paper[1] I have suggested an alternative explanation of certain features of the dream which Freud explains by means of his concept of the censorship. I have supposed that the fantastic character of the dream is not due to any process of distortion designed to elude the vigilance of some anthropomorphic agency watching at the threshold of consciousness, but is the natural result of the infantile character of the dream. I regard this character as the direct consequence of the coming into activity of modes of behaviour which in the waking state are held in check by levels of mental activity embodying the experience of later life.

It is the aim of this paper to show that the affective character of the dream which Freud has referred to the activity of the censorship is also the natural result of the character of the dream as an expression of infantile mentality. According to Freud one character of the dream is that its manifest content is as a rule poorer in affect than the dream-thoughts. Where there is an affect in the manifest dream it will also be found in the latent content, but the converse is not true. There may be no appreciable affective disturbance in the manifest dream when the presence of affect in the deeper content is evident. In other words, one of the results of the transformation of the latent into the manifest content of the dream is not only to disguise the nature of the dream-thoughts from the sleeper, but also to lessen or inhibit its affective character; and just as Freud ascribes the fact of disguise to the censorship, so does he ascribe to this agency the lessening or inhibition of affect. I will begin by saying that my own experience definitely confirms Freud's statement that affect may be absent or at least inappreciable in the manifest dream when it is evidently present in the deeper dream-thoughts of which the manifest dream is the transformed expression. This is a definite fact which has to be explained by any theory which endeavours to account for the relation of the dream to the affective aspect of experience. I propose, however, to begin the consideration of this subject by dealing with a variety of the dream in which affect is not merely present in the manifest content, but is present in an extreme degree. In the nightmare there is painful affect of the most intense kind, and any theory of the dream must take account of this character. This variety of dream, in the form of so-called night-terror, is especially liable to occur in childhood, but examples of one kind or another often occur in adult life, especially under abnormal circumstances. Everyone who had to do with war-neurosis became very familiar with this form of dream as a characteristic example of the nightmare.

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[1] "Freud's Concept of the 'Censorship,'" *Psychoanalytic Rev.* 1920, VII, 213; republished in *Instinct and the Unconscious*, 1920, p. 228.

The nightmare of war-neurosis generally occurred at first as a faithful reproduction of some scene of warfare, usually some experience of a particularly horrible kind or some dangerous event, such as a crash from an aeroplane. A characteristic feature of this variety of dream is that it is accompanied by an affect of a peculiarly intense kind, often with a special quality described as different from any known in waking life. The dream ends suddenly by the patient waking in a state of acute terror directly continuous with the terror of the dream and with all the physical accompaniments of extreme fear, such as profuse sweating, shaking, and violent beating of the heart. Often the dream recurs in exactly the same form night after night, and even several times in one night, and a sufferer will often keep himself from sleeping again after one experience from dread of its repetition.

Two of these features are of special interest in relation to the place of affect in the dream. One is that the affective disturbance is extreme in amount; the other, that the dream is often the repetition of an actual experience without transformation of any kind. There is absolutely nothing of the grotesque or fantastic, but the dream follows the grim reality faithfully. Moreover, it is often in my experience one of the first signs of improvement that some amount of transformation appears; the events of the actual experience are replaced in the dream by incidents of other kinds, such as the appearance of terrifying animals, which stand in no direct relation to the actual war-experience of the dreamer. Though in these cases the dream continues to be accompanied by fear, this is less intense and accompanied by less severe physical manifestations, and in many cases this transformed character serves as a stage towards the disappearance of the 'nightmare' character of the dreams. The course of many of these cases suggests that there is a definite relation between the amount of transformation and the intensity of the affect. They suggest that the intensity of affect is inversely proportional to the amount of transformation, a suggestion in harmony with the view of Freud that one of the results of the transformation of the latent into the manifest content of the dream is to lessen or inhibit its affective character.

When, however, we examine these dreams more closely we find that they show features which can hardly be reconciled with the general Freudian position. Above all is the difficulty of reconciliation with the view that every dream is a wish-fulfilment. It is difficult to see how such awful and terrifying experiences as those of dreams of this kind can be the result of wishes of the dreamer. Even if there were no other facts to lead us to regard Freud's view that the dream is a wish-fulfilment as unduly simple, and in my opinion there are many such facts, the nightmare and the battle-dream would themselves be sufficient to lead us to revise the Freudian view. In its place I am accustomed to look upon the dream as the attempted solution of a conflict, an attempt to solve a conflict of the waking life by such means as still remain open when the higher levels of mental activity have been put out of action by the inhibition of sleep.

Following this line of thought let us inquire how far the nightmare and the battle-dream are capable of explanation as infantile attempts to solve a conflict. In considering this matter I will begin with a feature of dreams of this kind which I have not so far mentioned.

In most, if not all, battle-dreams it is found that in the waking state the dreamer has been striving to keep out of his consciousness the experience which is reproduced in the dream. He has been repressing this experience. It has been found over and over again that when this process of repression is given up, the dreams no longer occur, or, if they continue, lose their terrifying character. A large body of evidence accumulated during the later years of the war showing that these terrifying dreams are due to repression of the experience which forms the content of the dream. So far as desire enters into causation, the dream is the direct negation of a wish, the wish not to be subjected to the repetition of a painful experience, the wish leading to a process of repression in the waking life which in its turn produces the dream in sleep.

Taking the process as a whole it seems to have the following character. One who has dreams of this kind has been the subject of a painful experience which tends to obtrude itself upon his attention, a tendency which is counteracted by a process of repression, a process of keeping the experience out of attention whenever it tends to appear. The conflict is one between a process in which an experience tends to recur to memory and a desire that the experience shall not recur. So long as the subject of this conflict is awake, the process of repression has the upper hand, but as soon as sleep occurs and the process of witting repression is removed, the repressed experience meets with no obstacle and makes its presence felt with full force. From this point of view the dream, instead of being the fulfilment of a wish, conscious or unconscious, is the complete failure of a wish which is only effectual so long as the subject of the experience remains awake. The experience of the nightmare not only fails wholly to fit into the category of wish-fulfilment; it suggests that this form of dream is essentially an expression of the complete negation of a wish.

If now we turn to consider how far a dream of this kind can be regarded as the solution of a conflict, we find that a conflict is undoubtedly present, viz. a conflict between the tendency of an experience to recur and the wish that it shall not do so. But if the dream is regarded as a solution of this conflict, it is a solution of a quite unsuccessful kind, so far as the health and happiness of the dreamer is concerned. From this point of view a dream of this kind must be regarded as a failure of solution, and, if I am right in my view concerning the dependence of this form of dream upon repression, a failure directly due to a wrong method of meeting the situation. The whole process belongs to a category of a kind very different from that in which we place dreams of the more usual kind.

In the case, on the other hand, in which the dream begins to show a certain amount of transformation with the accompaniment of less intense affect, we find ourselves in the presence of a process of the same order as that with which we are acquainted in dreams of the customary kind. It is only when the process of transformation begins that it becomes at all possible to consider the nightmare as the solution of a conflict, and since in this case the chief result at first is to lessen the intensity of the painful affect, it can only be regarded as a solution of an imperfect kind, the effect of the transformation being to lessen to some extent the painfulness of an experience which the dreamer wishes to keep altogether out of his consciousness. If we include the nightmare in the dream-category, it becomes no longer possible simply to regard the dream as the solution of a conflict, though it may still be possible to look upon it as an attempted solution. It is to the process of transformation that we must look as the instrument whereby the dream-consciousness reaches a symbolic solution of a conflict which is present both in the sleeping and the waking mind.

The nightmare, especially in the pure form taken by the battle-dream, has such special characters that it might perhaps seem desirable to exclude it from the category of dreams and put it in a class by itself. If, however, such a suggestion were seriously entertained, it would be necessary to point out that the same need arises in the case of dreams of a very different kind. There is a large class of dreams, that with a manifest sexual content, in which there may be little or no transformation and the affective accompaniment is of a pleasant kind. Moreover, the affect may be present in an intense degree and accompanied by physical manifestations just as appropriate to the content as sweating and tremor are appropriate to the dream dependent on fear. Another point of similarity between the two kinds of dream is that the manifest sexual dream is almost certainly connected with repression and can be lessened in frequency and intensity, or altogether inhibited, if the repression ceases. Here again there is reason to believe that when transformation occurs in this class of dream, it is associated with a lessening or disappearance of affect. If the absence of transformation is to exclude from the dream-category, the process of exclusion will have to go far beyond the nightmare and will apply to frequent experiences of sleep which are universally recognised as dreams.

The study of the nightmare with its excess of affective disturbance has thus far led us to connect affect with the degree of transformation and to regard lessening or total inhibition of affect as a function of the process of transformation rather than of the dream process in general. Moreover, we are led to regard the nightmare as a phenomenon of sleep in which the affective disturbance is of an extremely painful kind because the dream is the expression of complete failure in a conflict, a conflict between the tendency for a painful experience to recur and an intense desire that it shall not do so. We are tempted to connect the extremely painful character of the affective disturbance with the complete failure of the dream as the solution of a conflict. It is suggested that whether a dream is accompanied by a painful affect, or has an indifferent character from the affective aspect, depends on the degree of success in the solution of the conflict which is finding expression. The general view is suggested that where a conflict is solved in a dream in a symbolic manner there is no affect; that where the solution is completely unsuccessful there is affect of a highly painful kind; and that different degrees of painfulness of the affect depend on different degrees in which the dream forms a successful solution.

According to this point of view a dream is accompanied by a painful affect because it fails to provide a solution of the conflict upon which the dream depends, and when there is transformation in the dream the degree of painfulness of the accompanying affect is correlated with the extent to which the dream provides a solution of this conflict. I have considered the typical battle-dream as an example of the case in which there is a complete failure of solution, and it remains to give examples of cases in which, though the dream is accompanied by transformation, this fails to provide a solution of the conflict upon which the dream depends.

One such example is the highly painful dream of a patient which was found to arise out of a situation of a very difficult and unpleasant kind. In the final incident the dreamer was about to shoot a dream-personage who was certainly a surrogate of himself when he was stopped by the voice of his child saying: "Don't do it, Daddie, you'll hurt me too." The dreamer was at the time involved in a very severe conflict between a desire on the one hand for suicide and for the peace which that act would bring, and on the other, motives of a social kind which acted in the opposite direction, prominent among these being the knowledge that every suicide inflicts a social stigma upon those he leaves behind him. The dream ended with the victory of these latter motives as expressed in the voice of his son, and the consequent frustration of the wish for the solution of the conflict which would bring rest and peace.

In another dream of the same patient, which was so unpleasant that he awoke vomiting, the dream ended by his turning away into a wilderness from a river which was shown by the analysis to symbolize myself. The dream expressed a conflict between his tendency to undue reliance upon my help and his desire to stand on his own feet, and the ending of the dream expressed a frustration of his wish to continue to rely on me for help and comfort. The dream had its source in a situation arising out of the process of 'transference' in the course of psycho-therapy.

Both these dreams support the conclusion reached by the study of the battle-dream that the affective accompaniment of the dream is determined by the degree in which the dream reaches a satisfactory solution of the conflict upon which it depends. The extreme affect of the nightmare, according to this view, will be the result of the complete failure to effect a solution, while the highly painful character of the two dreams of my patient was due to the dream-consciousness having reached a solution contrary to the desires most prominent and most potent in the sleeper's consciousness when awake.

Having reached this solution of our problem, I can now consider another aspect of the dream so far as its affective nature is concerned. According to the view here put forward, dreams are not only regarded as solutions of conflicts, but as solutions of a kind differing from those adopted in healthy waking life in that the solution has an infantile character[1], or at least a character belonging to a period of life earlier than that of the occurrence of the dream. Let us now inquire how far this infantile character is true of the nightmare, again taking the war-dream as our instance. The first point to notice is that under the ordinary circumstances of our modern civilised life, dreams of this character are exceptional in the adult. It is in childhood that they are best known, forming the basis of the night-terrors from which so many children suffer. When they occur in the adult, they may be regarded as examples of regression to a state characteristic of childhood.

A feature of the war-dream, and of other adult nightmares, is the exaggerated character of the fear experienced, this exaggeration being again a character of infancy and childhood. There is little doubt that infancy and childhood form a period of life in which the human being is liable to affective disturbances of a very intense kind with the crude explosive nature which is characteristic of the affect of the nightmare or war-dream.

Still another feature of the war-dream, and probably also of the adult nightmare in general, is of special significance. The affective disturbance is described as having a peculiar quality unlike any experience of adult waking life, or only paralleled by the gusts of unreasoning terror which are also liable occasionally to overwhelm sufferers from certain forms of psycho-neurosis. It is not, of course, possible to say that this quality which seems so peculiar and strange to the adult is a regression to a quality of the fears of childhood, but this is at least possible.

I suggest, therefore, that the nightmare and the war-dream are themselves examples of infantile states, that they are occurrences of the sleep of adults which appear in a form characteristic of infancy.

If now we pass to the stage of the war-dream in which transformation appears, one of the most frequent forms in which this transformation shows itself in my experience is that terrifying animals take the place of the incidents of warfare which have hitherto formed the exclusive content of the dream. There is little question that animals are prominent in the terrifying dreams of childhood, and their occurrence in the transformed war-dream may thus be regarded as another example of regression. Some of my patients remembered having had similar dreams in childhood, while in one case in which the terrifying image was a Chinaman, the dreamer distinctly recollected its occurrence in the dreams of his childhood. The images utilised in these examples of transformation were characteristic of an early period of life.

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[1] See *Instinct and the Unconscious*, 230.

The examination of the nightmare and war-dream thus shows that they possess, though in a different way, a character which I have ascribed to dreams of the more ordinary kind. The nightmare and the war-dream share with dreams of other kinds the feature that they are occurrences in which experience finds expression in sleep in a form characteristic of infancy or of periods of life earlier than that of the occurrence of the dream. While it is not possible to regard all dreams either as wish-fulfilments or as successful solutions of conflicts, it is possible to bring them all into the category of regression, of throwing back in sleep to modes of mental activity and expression characteristic of earlier periods of life.

While the pure battle-dream thus differs from the general run of dreams in respect of transformation, it falls into line with the rest with regard to the infantile form in which the affect finds expression. The infantile character gives a broader basis for classification than is afforded by the process of transformation. This being so, let us inquire how far this infantile character is capable of explaining, or at least of illustrating, the relation of the affect to the dream. For this purpose I shall use a dream which Freud has cited[1] in order to illustrate this topic. In this dream Freud cleansed the seat of a closet loaded with excrement by the same means as that adopted by Gulliver in order to put out the fire of the Lilliputian palace. In the dream Freud was wholly free from the disgust which such a scene would have aroused if it had been a real incident of his waking life, and he uses this dream to illustrate the absence of affect in the manifest dream when this is present in the deeper content. In a very interesting and instructive analysis of this dream Freud shows that it was the expression of a desire to be clear of unpleasant aspects of his professional work, and was a symbolic expression of a wish to clean out the Augean stables with which in a fit of despondency he tended to compare these surroundings. On the other hand, his own rôle as the Hercules of the occasion seemed to Freud to correspond with certain grandiose traits he detected in his own attitude towards the situation. Freud refers the absence of disgust in the dream to the fact revealed by his analysis that the thoughts of cleansing an Augean stable were combined with others of a pleasant kind referring to his own powers as a cleanser. The indifferent character of the dream in its affective aspect is referred to the combination of the two opposed kinds of affect present in the dream-thoughts.

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[1] *Die Traumdeutung*, 5te Aufl., 1919, 318: *The Interpretation of Dreams*, 1916, 372.

On the view that the dream is an expression of infantile modes of mental activity, another and a simpler explanation of the absence of disgust becomes possible. From this point of view Freud's dream is an expression of a conflict between his desire to get away from certain disgusting aspects of his professional work and the desire for the beauties and pleasures of a holiday. The dream produces a symbolic solution of the conflict by getting rid of the disgusting aspects of his work by means of a procedure which would make a direct appeal to the mind of a child and is in consonance with a child's modes of activity. The absence of disgust in the dream seems to receive an explanation more natural and at the same time more simple than that given by Freud if in his dream he had for the time regressed to an infantile attitude and was seeking to purify his surroundings symbolically by a procedure characteristic of childhood. In the child such a procedure would not arouse the emotion of disgust and therefore no such emotion was aroused in the dream. The indifferent affective character of this dream becomes natural and needs no elaborate explanation if the dream be an infantile mode of expression of a wish-fulfilment or the solution of a conflict.

This dream of Freud's is one in which the absence of affect in a situation where it might be expected to occur is readily accounted for on the view that the dream has an infantile character. Let us now turn to examples of dreams accompanied by manifest affect and inquire whether these also become explicable on the view that the dream expresses infantile mentality. For this purpose let us turn to the dreams of my patient.

The central situation of the suicide dream (see p. 118) was one in which the dreamer was about to shoot himself symbolically when he was stopped by the voice of his son calling attention to the injury he would inflict upon the boy if he carried out his design. There can be no question that the homicidal situation presented by the dream would be at least as terrifying as, and probably far more terrifying, to the child than the suicide of which the dream-homicide was a symbolic expression. In this case the difference lies not so much in the relation between affect and the incident which called it forth, as in the nature of the affect itself. The infantile character shows itself in the intense and explosive, or as I have expressed it elsewhere[1], in the 'all-or-none' character of the affect, while in the real situation this would have had the more restrained form which emotion normally assumes in adult life.

Again, if the transference dream was an expression of infantile modes of feeling and acting, we have a case in which we may suppose that the child of the dream was being forced to turn away from one upon whom he had come to rely, and if current views concerning transference have any value, from one whom he had put in the position of his father. The intense affect of the dream, inappropriate perhaps to the actual situation of the dream, certainly inappropriate to the situation of having to turn from the help given to him by his physician, becomes wholly appropriate if in the dream the man had become again the child and was turning away from his father and ceasing to depend upon him for help and comfort[2]. The intensity of the affect becomes intelligible if the dream be a regression to infancy, just as the absence of affect becomes intelligible if the dream-situation is one which the infantile mind would regard with indifference.

I have so far dealt almost entirely with painful or unpleasant affect in its relation to the dream. It is now necessary to say something about the affects of a pleasant kind which so often accompany the dream. We have been led to connect the degree of intensity of painful affect present in the dream with the extent of the process of transformation, the absence of affect being correlated with the completeness of this process. I have supposed that the presence, nature and amount of affect present in the dream stand in no definite relation to the presence, nature and amount of affect accompanying the conflict to which the dream is due, but are determined by the attitude which would have been proper to the dreamer at the level of mental development active in the dream. If the level of mental activity which is active in the dream is that of childhood, and if the guise in which a conflict finds expression in a dream is one which would be terrifying to the child, the dream will be accompanied by affect in the form of terror. If, on the other hand, the imagery and symbolism in which a conflict is finding expression are of a kind so natural to the child that they would not be accompanied by affect, there will be no affect in the dream. The situation as portrayed in the dream may be one of adult life, but the dream-reaction is that of the child so far as affect is concerned.

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[1] See *Instinct and the Unconscious*, 117.

[2] I may point out here that the physical reaction of this dream has a clearly infantile character. The dreamer awoke vomiting, a mode of reaction to unpleasant situations far more frequent in infancy than in adult life.

If we now apply this point of view to the case in which the affect of a dream is of a pleasant kind, it will again be necessary to distinguish between dreams with varying degrees of transformation. We should expect pleasurable, as painful, affect to be the more definite, the less the degree of transformation. Where there is no transformation, we should expect the affective accompaniment to be considerable in amount. As an example of this may be cited the dreams of the kind, to which I have already referred incidentally, where the manifest content has a sexual nature. Here the pleasant nature of the affect is just as natural to the situation as the fear or terror accompanying a dream of which the manifest content has reference to danger.

Another variety of dream accompanied by affect of a pleasant kind is that in which the manifest content has the nature of a day-dream. In this case again there is little if any transformation, but there is carried over into the dream an experience of waking life which in that waking life is definitely accompanied by pleasant affect. It may be suggested that the occurrence of the experience in sleep gives it that enhanced vividness and apparent reality which is characteristic of the day-dreams of childhood, and that the pleasantness of the affect is associated with this approach to an infantile character.

At the beginning of this paper I mentioned a view of Freud to which I may now return for a moment. According to this writer, the lessening or disappearance of affect which accompanies the transformation of the latent into the manifest content of the dream is an effect ascribed to the censorship. As already mentioned, I have dealt elsewhere with the value of the concept of the censorship as a means of explaining or expressing the facts of transformation and disguise. I have referred these processes to the infantile character of the dream and have supposed that transformation and disguise are necessary consequences of this infantile character and would have occurred even if there had not been present the resistance for which Freud has adopted the simile of the censorship. It is evident that this point of view is equally applicable to the problem considered in this paper. The absence of affect in Freud's "Augean stable dream," and its intense character in the dreams of my patient follow with equal readiness from the infantile character of the dream, and need no agency watching at the threshold of consciousness for their explanation. Freud's concept of the censorship was especially devised to meet the cases where affect is lessened or abolished and fails wholly to account for such dreams as those of my patient. The explanation of the nature of the affect in the dream as the necessary result of its infantile character not only renders the concept of the censorship unnecessary when it might seem to be appropriate, but it covers a wider field and is able to bring within its scope features of the dream with which this concept is wholly inadequate to deal.

I will conclude this paper by stating the conclusions reached in it. I start from the position that dreams are attempts to solve in sleep conflicts of the waking life, and that these attempts are necessarily, from the nature of sleep, of a more or less infantile kind; since in sleep only the earlier levels of mental functioning are active. Next, I suppose, and here I follow Freud or at least agree with him, that the effect of transformation is to diminish or abolish the affective aspect of the conflict. Consequently, when there is no transformation, there is affect in the dream. This affect is painful when the conflict fails to satisfy the most prominent wishes of the dreamer. On the other hand, it is pleasant when these wishes are gratified. But in the majority of dreams the affective aspect is slight or absent because the struggle is transformed and the solution of the conflict is only of a symbolic kind.

Moreover, just as I have previously argued that the transformation, ascribed by Freud to the need to evade the agency he calls the censorship, is a natural and indeed inevitable result of the expression of the conflict in the mental imagery and modes of mental functioning of early life, so do I now suppose that the affect has a similar infantile quality, and that it is equally unnecessary to have recourse to the agency of the censorship to explain the relation between the manifest and latent contents of the dream so far as affect is concerned.

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THE LIMIT OF THE GROWTH OF INTELLIGENCE

By P. B. BALLARD.

1. *The problem* (p. 125).
2. *Difficulties in the way of a complete solution of the problem* (p. 126).
3. *The detection of absurdities as a test of intelligence* (pp. 126–128).
4. *The tests used* (pp. 128–131).
5. *Method of marking* (pp. 131–134).
6. *Results* (pp. 134–136).
7. *The curve of growth of intelligence* (pp. 136–138).
8. *The further use of the tests* (p. 138).
9. *The nature of intelligence* (pp. 138–141).

1. The Problem.

I begin by assuming that the term 'Intelligence' has an accepted meaning. Deferring a closer definition to the last section I meanwhile use the term as meaning that form of mental ability, whether simple or composite, which is measured by modern mental tests. To the plain man the most incredible part of this modern concept of intelligence is the early age at which intelligence is supposed to stop growing. Binet's scale of mental ages did not extend beyond fifteen. It is true that he added a group of five tests for adults, but these tests have since been found to be too difficult for the average adult. They fail to fit in with the rest of Binet's scale, which is strictly based on age-performance experimentally determined. Instead of indicating a norm they fix an arbitrary standard. As, however, Binet was primarily concerned with the detection of feeble-minded children it was but natural that he should have standardised the lower part of his scale more carefully than the upper part.

In making the Stanford Revision of the Binet Scale Terman[1] was influenced by no such restriction of purpose, and yet he could carry his age scale no further than fourteen. To represent the higher ages he has two sets of tests, one for the "average adult" and the other for the "superior adult." In calculating the Intelligence Quotient (the ratio the mental age bears to the chronological age) he assumes the chronological age of all persons over sixteen to be sixteen; which is equivalent to assuming that after sixteen years of age there is no improvement in native intelligence.

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[1] *The Measurement of Intelligence*, 56–61.

Burt[1] does not carry his scale of reasoning tests beyond fourteen.

The question to be determined is: At what age does intelligence cease to grow? As there are grounds for believing that this terminal age varies with the individual—that there is not one terminal age but many terminal ages—it is desirable to know both the average and the range of these terminal ages.

2. DIFFICULTIES IN THE WAY OF A COMPLETE SOLUTION OF THE PROBLEM.

The main obstacle in the way of research is the difficulty of securing unselected subjects over fourteen years of age. Up to fourteen years of age there is but little difficulty; for all children between five and fourteen are compelled by law to attend school, where they can be found and tested. But after fourteen in almost every type of school the duller pupils begin to leave; and as the ages increase the age-groups get increasingly smaller, and the quality of intellect increasingly higher. Such improvement therefore as one would find in the intelligence of secondary school children as they proceed up the school is not necessarily due to the growth of intelligence with age: it may be due to the gradual elimination of the less intelligent.

It is almost impossible to feel certain that one has secured a good representative sample of any age-group beyond that which falls between 13 and 14; and the further the group is removed from this the more difficult becomes the task.

3. THE DETECTION OF ABSURDITIES AS A TEST OF INTELLIGENCE.

Binet seems to have been the first to employ absurd statements as a test of intelligence. He used the following five absurdities, which be allotted to the eleventh year in the 1908 scale, and to the tenth in the revised scale of 1911:

(1) One day a man fell on his head off a bicycle and was instantly killed. He was taken to the hospital and they say he will never get better.

(2) I have three brothers—Paul, Ernest, and myself.

(3) Once the body of a poor girl was found in a wood, cut into eighteen pieces. They say that she killed herself.

(4) Yesterday there was a railway accident; but the newspaper says it is not a serious one as only forty-eight people were killed.

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[1] *J. of Exp. Pedag.* 1919, v. 73–77.

(5) A man once said, "If I should ever grow desperate and kill myself I shall not choose a Friday to do it on, for Friday is an unlucky day and would bring me bad luck."

In order to pass the test the child must detect three out of the five absurdities.

By way of criticism it may be remarked that four out of the five deal with murder or sudden death; that three depend on the same basal fact (the fact that a dead man is dead and can neither act nor suffer); that they are not equally easy; that they are not arranged in precise order of difficulty; and that there is some doubt as to the year to which they should be allocated. Burt considers No. 3 harder than No. 4, and No. 5 so hard that it is more suitable for children of eleven or twelve. Terman keeps the test in the tenth year, but Goddard and Kuhlmann place it in the eleventh year. Terman, however, makes two substitutions. Omitting No. 2 because in some languages the expression is idiomatically correct, and No. 5 because it is too hard, he substitutes the following:

(1) A man said: "I know a road from my house to the city which is downhill all the way to the city and downhill all the way back home."

(2) An engineer said that the more cars he had in his train the faster he could go.

To compensate for the easier items Terman requires four to be correct instead of three.

Of the other absurdities which have from time to time been suggested as alternatives to Binet's the best are these:

(1) This morning I met a smart young man. He was walking down the street with his hands in his pockets, and twirling a brand-new walking-stick. (Whipple.)

(2) When there is a collision the last carriage of a train is usually damaged most. So the guard thinks it would be best if the last carriage was always taken off before the train starts. (Yerkes.)

(3) An Irishman called one day at the post office and asked if there was a letter waiting for him. "What is your name?" asked the postmaster. "Sure," said the man, "you will find my name on the envelope."

(4) A boy wrote in his composition: "Soap smells nice but tastes horrid. It tastes worst of all when you get it in your eye."

The value of this type of test is beyond question. Those who have extensively used it agree with Terman, who says: "The detection of absurdities is one of the most ingenious and serviceable tests of the entire scale. It is little influenced by schooling, and it comes nearer than any other to being a test of that species of mother-wit which we call common sense." And again: "It is an invaluable test for the higher grades of mental deficiency."

Valuable as these particular tests are for detecting the dull-witted and for marking a certain stage of mental development, they are of little use in differentiating ordinary adults. Terman observes that by twelve years of age critical ability is so far developed that the test is nearly always passed. This may be so if it is given in the form prescribed by Binet; but if we alter the mode of administering the test we get quite different results. Binet's absurdity tests are oral and individual; the child is told beforehand that each sentence contains something that is silly, and he is asked to discover it. But if, on the other hand, the child is left in doubt whether the statement is sensible or silly; if he is told that it may be either the one or the other, and is asked to say which and give his reasons, and especially if he is asked to write those reasons down, the difficulty of the task is greatly increased. Binet's plan gives two possibilities—to find the absurdity or not to find it. The other plan gives three—to judge the sentence sensible, or to judge it absurd without being able to explain the absurdity, or to judge it absurd and to explain the absurdity.

The difference is exemplified by a test which I have frequently used: Captain Cook made three voyages round the world. In one of these voyages he was killed by savages. Which voyage was it—the first, the second, or the third? Although not precisely conforming to the type described above this is clearly an absurdity test admitting of three possible answers; and if, when the third voyage is given a reason is demanded the number of possibilities is increased to four. It is difficult to conceive that a lad of thirteen years of age with his wits about him could fail to give the right answer and the right explanation. Yet as a matter of fact about 40 per cent. of them do fail; at least they do so when the test is written and not oral. Few give the wrong voyage (although about five per cent. do even this), but those who select the third voyage cannot always show that they decide on logical grounds.

I therefore set about devising a series of absurdity tests which would be free from the main objections urged against the Binet absurdities, which would vary in difficulty and in the type of incongruity, and which could suitably be applied to older pupils.

4. THE TESTS USED.

The tests I used in my investigation comprise 34 absurdities mixed with a few quasi-absurdities—statements which at first blush seem absurd but are not really so. I give below the 34 real absurdities arranged in increasing order of difficulty. They are derived from various sources.

Some I have remembered, some (not many) I have invented, and some have been suggested to me by friends.

Instructions.—On the top of the paper write the name of your school, your name, and your age in years and months.

Some of the following sayings are sensible and some are foolish, and you must say which of them are sensible and which are foolish. Do not write out the saying, but simply put its number on your paper and write after it the word "sensible" or "foolish." If you write the word "sensible" you need say nothing more; but if you write the word "foolish" you must give a reason for your opinion.

Section A.

(1) A soldier writing home to his mother said: "I am writing this letter with a sword in one hand and a pistol in the other."

(2) It is said that a certain town in Greece contains two relics of St Paul; one his skull when he was a boy, and the other his skull when he was a man.

(3) An old gentleman complained that he could no longer walk round the park as he used to: he could now only go half way round and back again.

(4) In the year 1915 many more women got married than men.

(5) A hunter who had used up all his ammunition was chased by a bear. A bright idea struck him: he would climb a tree. When he got to the top he remembered that the bear could also climb a tree; but he got out of his difficulty by pulling the tree up after him.

(6) Light comes from the sun; feathers are light; therefore feathers come from the sun.

Section B.

(7) A boy who wanted to go to a cinema but had no money, thought it would be a good plan to walk in backwards, for the man at the door would think he was going out, and would not ask for his ticket.

(8) A teacher said to his boys: "Tomorrow we hold a scripture examination, attendance at which is voluntary; so if any boy is absent he had better look out."

(9) A soldier in the march complained that every man in the regiment was out of step except himself.

(10) A sailor who was put to haul in a rope from the sea found it so long that he gave it up in disgust saying that somebody must have cut the end off.

(11) The moon is more useful than the sun, for it gives us light in the night when we really need it, while the sun gives us light in the day when we don't need it.

(12) This is a sad and bitter world: we never strew flowers on a man's grave until he is dead.

(13) A stammerer who came from Birmingham to live in London was asked why he stammered more in London. He replied: "B-b-bigger town."

(14) A man said to his shoemaker: "You blockhead; I told you to make one of the shoes larger than the other, and instead of that you have made one of them smaller than the other."

(15) A speaker at a war economy meeting said: "We must all economise. The man who generally buys four suits of clothes a year should be satisfied with three, the man who buys three should be satisfied with two, and so on."

Section C.

(16) There is a tree in America which is so tall that it takes two men and a boy to see the top.

(17) A householder saw an advertisement: "Buy one of Simkin's stoves and save half your coal." He bought two in order to save all of it.

(18) A forgetful man once tied a knot in his hankerchief to remind him of something; but he suddenly remembered that the last time he did this he could not recall what it was he had to remember. So this time he tied two knots to make sure. The first knot was to remind him that he had to remember something; the second to remind him of what that something was.

(19) I am not conceited, for I don't think I am half as clever as I really am.

(20) A country man came up to London during the war and saw on a hoarding the words: "Eat less bread: do it now." He immediately went into a tea-shop to do it now.

(21) "I don't like onions," said the boy, "and I'm glad I don't; for if I did I should be eating them all day, and I hate the beastly things."

(22) An Irishman was charged at the police-court with having stolen a pig. Five witnesses swore that they had seen him do it; but the prisoner said that he could prove his innocence by bringing 50 witnesses who would swear that they had not seen him steal the pig.

Section D.

(23) "I have always noticed," remarked the old man, "that if I do not die in March I am all right for the rest of the year."

(24) Some years ago it was proposed to shift Sunday from the end of the week to the middle so as to divide the week into two parts.

(25) John James, who had married his widow's sister, used to say that if a man had a bad sister it was his misfortune, but if he had a bad wife it was his fault.

(26) You are thin, and I am thin; but he is thinner than both of us put together.

(27) One should never use long words when short ones will do just as well, for when the words are long the meaning is obscured by the unusualness of the terminology, and intelligibility is consequently endangered.

(28) The three men laughed; then stopped suddenly as the eyes of each met those of the others across the table.

(29) Watching pigs wallowing in the mire, he muttered: "No wonder they are called pigs."

Section E.

(30) A man who bought a dog that had been advertised complained to the seller that the dog's legs were too short. The seller replied: "They are long enough to reach the ground aren't they? what more do you want?"

(31) A showman advertised for a giant and a dwarf. A man of ordinary height presented himself and offered to fill both parts. He claimed to be the smallest giant in the world and the biggest dwarf.

(32) While standing near a clock tower just before the clock struck twelve two boys tried to find out which of them could hold his breath the longer. Neither of them won; for one was able to hold his breath from the first stroke of the clock to the sixth, and the other from the sixth to the twelfth.

(33) Every rule, even this one, has an exception.

(34) The horse obeys his master because his eyes magnify, so that his master seems to the horse to be much larger than the horse himself.

The plan I adopted in setting the test was to give each child a cyclostyled copy of 38 statements comprising the 34 real absurdities interspersed with four spurious absurdities. The four spurious ones, arranged in increasing order of difficulty, were:

(1) A man may live many years on a crust—the crust of the earth.

(2) An old woman said that she was so old that in a few years time she would be twice as old as she was forty years ago.

(3) I bought my little boy a watch which was said to give the right time twice a day; it turned out to be a dummy watch with fixed hands.

(4) The commonest word in the English language that is spelt the same way backwards and forwards is "a."

Any others of the same kind would have served just as well, for whatever answer was given it was not scored. Only the real absurdities were considered in estimating the results.

The time allowed was unlimited. The subjects were asked to hand in their papers as soon as they were finished.

The sectional classification of the absurdities is based on the percentage of adults who succeeded in detecting them. Each question in Section A was correctly answered by over 80 per cent. of adults, each in Section B by between 60 and 80 per cent., each in Section C by between 40 and 60 per cent., each in Section D by between 20 and 40 per cent., and each in Section E by less than 20 per cent. Only 6 per cent. dealt successfully with the last question.

The test was given to all children over eleven in two elementary schools, two central schools, three secondary schools, one day continuation school, one evening school, and one training college. Through the kind help of Dr R. R. Rusk I was able to secure results from a few schools and a training college in Scotland. Altogether about 2000 subjects were tested[1].

5. METHOD OF MARKING.

The papers were all marked by myself, or under my supervision, in accordance with the following key[2]:

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[1] I wish here to record my obligations to the following principals and head-teachers for their valuable co-operation in this research: Miss W. Mercier, M.A., Miss M. A. Tucker, M.A., Rev. C. J. Smith, M.A., Mr E. B. Cumberland, B.A., B.Sc., Mr J. G. Robson, Mr T. Lea, Mr T. G. Tibbey, M.A., Mr F. E. Rogers, B.A., Mr E. G. Dixon, and the Principal of Harrods' Continuation School.

[2] My thanks are due to my colleague Mr F. P. B. Shipham, M.A., for his help in the marking and revision of the papers.

KEY.

1. *Right:* "He had no hand free for writing." "Both hands occupied," etc.
 Wrong: "He couldn't write with a sword."
2. *Right:* "No man has two skulls." "A man has the same skull as he had as a boy," etc.
3. *Right:* "The distance is the same."
 Wrong: "He was a silly old man."
4. Some may contend that there is nothing inherently absurd in this, since a widow, widower, bigamist, or a divorced person may disturb the balance. Such an answer is to be considered correct.
5. Two points: (i) "The impossibility of pulling up the tree if he was on it," (ii) "The futility of doing it." If one point is given the test is passed.
 Right: "He could not pull up the tree if he was on it."
 "He had nowhere to put his foot except the tree."
 "He had no fulcrum."
 "If he pulled the tree up he would fall down with it."
 Wrong: "He could not pull the tree up."
6. The logical fallacy must be detected.
 Right: "The word light is used in two senses."
 "When we say feathers are light we mean weight."
 Wrong: "Feathers do not come from the Sun."
7. The answer must imply that the boy would still be moving inwards.
 Right: "The man at the door would see that the boy was going in not out."
 Wrong: "The man at the door would stop him."
8. *Right:* "Voluntary means, if you like."
 Wrong: "The teacher should not punish a boy for being absent."
9. *Right:* "He was out of step himself."
10. *Right:* "By cutting the end off the rope is made shorter."
 "The rope still has an end."
 Wrong: "You can't cut the end off."
11. *Right:* "It is the sun that makes the day."
 "Without the sun there would be no day."
 Wrong: "The moon gets its light from the sun."
12. *Right:* "A man has no grave until he is dead."
13. *Right:* "The size of the town makes no difference."
14. *Right:* "You cannot have one larger without having the other shorter."
 "Both statements mean the same."
15. *Right:* "The man who had one suit a year would have to be satisfied with none."
16. *Right:* "Two men and a boy cannot see further than one man or one boy."
 "We cannot add sights together."
 Wrong: "Anybody could see the top."
17. *Right:* "Two stoves would burn more than one."
 "To save all his coal he must burn none at all."
18. *Right:* "Two knots would not make him remember better than one."
 "If the first knot would not remind him the second would not either."
 Wrong: "Knots can't make you remember."

19. *Right:* "He contradicts himself."

"He first says that he is not conceited and then says that he is."

"He says he does not think himself clever and then says that he does."

Any answer will do that points out a contradiction, (*a*) between the two clauses or (*b*) between the two parts of the second clause.

20. *Right:* "He could not eat less by going into a tea-shop: he could only eat more."

"The advertisement asked you to eat less at meal times: the 'now' did not mean at this moment."

21. *Right:* "The boy assumes that he likes onions and that he does not like onions at the same time."

"He contradicts himself. He says, If I like them I should hate them."

22. *Right:* "The 50 witnesses were not real witnesses: they did not see anything."

"Only those who were present could give real evidence."

Any answer will do if it points out that negative evidence is in this case of no value.

23. *Right:* "He might have said the same of any other month in the year."

The fallacy consists in asserting that to be true of a particular month which is equally true of any other month.

24. *Right:* "There would still be a week from Sunday to Sunday."

"It would not split the week."

Wrong: "You can't shift Sunday."

"Sunday is the beginning of the week, not the end."

25. *Right:* "If a man has a widow he is dead."

26. *Right:* "Putting men together makes them fatter not thinner."

27. *Right:* "The writer does not follow his own advice."

"He tells us to use short words, and he himself uses long ones."

28. *Right:* "One man's eyes cannot meet two other men's eyes at once."

29. *Right:* "Pigs are not so called because they are dirty, but it is the other way about."

"Pigs is their proper name, their dirtiness has nothing to do with it."

Any answer will do if it suggests that the name is prior.

30. *Right:* "Every dog's legs reach the ground" (implying that the buyer's objection to this particular dog is not met).

"Legs half an inch long would reach the ground, but they would be of no use."

"The buyer meant that the legs were not long enough to be useful or beautiful."

"What the buyer meant was that the legs were not long enough to keep the body some distance from the ground."

31. *Right:* "A giant must be above the ordinary height, and a dwarf below the ordinary height."

"Smallness is a bad quality in a giant, and largeness in a dwarf."

32. *Right:* "The second boy won for he held his breath for six intervals, while the first held it for five."

33. *Right:* "Self-contradictory."

"This itself is a rule and the exception to it is that there is a rule which has no exception. Therefore if the rule is true it is also false."

34. *Right:* "If his eyes magnify they magnify everything in the same proportion." "If he sees a man big he also sees another horse big, and sees his own body big."

6. Results.

The first school tested was a Central School, a school to which selected children are annually drafted from a group of contributory schools. The selection is based on proficiency in arithmetic and composition as tested by a general competitive examination. There are no grounds for thinking that one year's draft is any better or any worse than another year's draft. The results are given in Table I.

Table I.

Scores gained at a Central School.

Ages	11–12	12–13	13–14	14–15
No. of children ...	80	108	124	40
Average score ...	14·9	17·4	16·6	16·8
Median score ...	15·5	17·5	16·7	17·0
Semi-interquartile range	3·1	2·2	2·7	2·5

The number of children over 15 was so small that no importance is to be attached to the results obtained from them. It is to be noted that the twelve-year-old group did better than any other.

At another Central School, however, to which the test was applied the scores improved with increasing age, for the average marks (with the semi-interquartile range (s.i.r.) in brackets) obtained by the age-groups beginning at 11–12 were 12·9 (1·9), 14·3 (3·0), 14·4 (2·5) and 16·5 (2·4), respectively.

The three age-groups beginning with 11–12 were tested at an ordinary elementary school, and the average scores made were 8·5 (s.i.r. = 3·2), 10·7 (s.i.r. = 2·2), 10·2 (s.i.r. = 3·0). The twelve-year-old's did better than the thirteen-year-old's. At another elementary school, however,—a school in a better neighbourhood—the corresponding scores were 10 (s.i.r. = 4·2), 12·6 (s.i.r. = 2·2) and 13·6 (s.i.r. = 2·7).

In a large and good secondary school the averages for the years 11 to 17 respectively were: 18, 18·8, 19·7, 22·6, 22·8, 22·5, 22·3. The semi-interquartile range of all the scores was 2·8. The maximum score was reached at 15. At another secondary school the maximum was again reached at 15, though there were pupils up to 18.

Interesting results were secured at a continuation school held on the premises of a large business firm. There were 34 pupils in the fourteen-year-old group, 164 in the fifteen-year-old group, and 112 in the sixteen-year-old group. The average marks were 19·1 (s.i.r. = 3·3), 15·6 (S.I.R. = 2·7) and 16·7 (S.I.R. = 3·3) for the three groups respectively. The superiority of the youngest group is explained by the fact that they were the only group selected by the firm after peace had been signed. During the war the selection was necessarily less stringent.

At a training college for women 141 students were tested. The average score was 21·1, the median was 22, and the semi-interquartile range was 3·5. The results indicate a degree of intelligence well above the average, but a little below the average at a good secondary school; thus confirming an opinion, frequently expressed, that the best scholars from secondary schools do not enter the teaching profession.

The number of papers received from Scotland was too small for the results to be significant. Such as they were, however, they indicate that Scottish children have keener eyes for absurdities than English children. The popular belief that a Scotsman cannot see the point of a joke is probably ill-founded: he sees the point but he does not see its fun. It is an emotional, not an intellectual disability.

The validity of pooling results from such disparate sources is very doubtful; and too much weight must not be attached to the following scores which were obtained by taking all the papers together from all the schools tested in England and Scotland, and finding the average:

Age	11	12	13	14	15	16	17
Averages	13·1	14·4	15·1	17·4	18·5	18·9	18·9

These norms support the conclusions reached by other investigators. Remembering what has already been said about the tendency of the duller children to fall out of the ranks after fourteen, we feel justified in concluding that the rate of growth of intelligence gradually slows down and that after sixteen years of age it virtually stops. Indeed there is not much improvement after twelve. It is true that my figures show a fairly large step from 13 to 14, but this is easily accounted for by the fact that the thirteen-year-old's tested were a fairly representative group; while after the fourteenth birthday social and intellectual selection began to operate.

An attempt was made to compare the abilities of secondary school boys with those of older people. A test for this purpose was prepared by selecting ten of the absurdities given above (the first and last in each of the five sections), and one quasi-absurdity—the third. The results of this test, which was given at a secondary school for boys and an evening school for men and women, and set forth in Table II.

The marks gained by the evening school students were amazingly low. These students, however, were not of an intellectual type; and there is a possibility that some of them did not take the tests quite seriously. The mistakes they made did not as a rule consist in the wrong interpretation of an absurdity, but in a sheer inability to see that there was an absurdity there at all. The item was simply labelled 'sensible.' There was no attempt at irony, sarcasm, or 'smartness' of any kind, such as was occasionally made by the secondary school boys.

Table II.

Marks obtained out of ten.

Secondary School.

Age	16–17	17–18	18–19
No. of pupils	45	15	13
Average score	7	5·8	6·9
Semi-interquartile range	1·4	1·7	·9

Evening School.

Age	15–17	17–19	19–22	over 22
No. of students	17	39	18	19
Average score	3·8	4·2	3·8	3·1
Semi-interquartile range	·8	1·2	1·0	1·5

The evidence on which to base a judgment of adult intelligence is regrettably scanty. The number of adults tested by me is small, and there is no guarantee that the sample is good—good as a sample. Yet scanty as the evidence is, it strongly forces on us the conviction that intelligence ceases to grow about the age of sixteen; for even when there are *a priori* grounds for believing the samples beyond sixteen to be weighted on the intellectual side we often find no sign of improvement in intelligence.

7. THE CURVE OF GROWTH OF INTELLIGENCE.

The validity of the above results rests on the assumption that the successive age-groups tested are on the average of the same intelligence quotient—that the fifteen-year-old subjects, say, will be on the whole exactly as intelligent a year hence as the sixteen-year-old subjects are to-day. Only thus can we regard the curve determined by the aggregate results as indicating the normal growth of intelligence. That these conditions were in my investigation fulfilled continuously from the ages of eleven to eighteen cannot be asserted. All that can be asserted is that in the individual schools tested there were no grounds for thinking that the quality of the minds varied from year to year up to, and including, the thirteen-year-old group; and as for the higher age-groups there were no grounds for thinking that the quality deteriorated, though, as I have already pointed out, there were good grounds for thinking that it improved.

Assuming that the tests I have used really measure intelligence, and combining my conclusions with those reached by other investigators, I may state generally that the growth of intelligence, which is rapid for the first few years, gradually slows down as the child gets older, that after twelve years of age the slowing-down is very marked, and that after sixteen further growth is inappreciable. It is this retardation of growth that accounts for the fact that Binet failed to interpolate in his scale a set of tests either between the ages of ten and twelve or between the ages of twelve and fifteen: omitting the doubtful tests for adults he could only mark out two steps after the age of ten.

It follows that a year of mental age is not a fixed unit: it gradually diminishes towards the higher end of the scale.

If after sixteen years of age the curve of growth is virtually a straight line, as it seems to be, we are forced to conclude that sixteen is not the average age at which growth ceases, but the maximum age. For the general curve is the resultant of a large number of individual curves; and if the resultant curve is straight after sixteen, then either all the individual curves are straight after sixteen, or if some rise others must fall. In other words we must choose between two alternative suppositions, one that nobody improves after sixteen, the other that some people in sound health begin at that age to suffer from senile decay. There is evidence for thinking that different races and different individuals vary in the age at which mental maturity is reached. S. D. Porteus[1] found that Australian aborigines complete their growth in intelligence several years earlier than white children. The average mental age of recruits in the American army during the war is said to be thirteen. It is highly probable therefore that the average age at which growth ceases is considerably below sixteen. The exact determination of this average age is of importance in estimating the intelligent quotient of adults; for if, following Terman, we use sixteen as the denominator for all chronological ages over sixteen, we undoubtedly get a quotient which is too low.

There is obviously need for an investigation which aims at finding the separate curves of growth for a large number of individuals, an investigation that will be wide in its scope and will be carried over a long series of years. It is only by an extensive research of this kind that we can find an answer to the question: What connexion is there between the quality of a person's mind and the age at which his intelligence reaches maturity? Are 'late bloomers' better than 'early bloomers'?

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[1] "Mental Tests with Delinquents and Australian Aboriginal Children." *Psychol. Rev.* xxiv. 32.

There is also need for testing the intelligence of representative samples of adults at, say, 20, 30 and 40 years of age. It is just possible that the growth of intelligence after sixteen, imperceptible as it is from year to year, may in ten years reach an amount that may be easily measured.

8. THE FURTHER USE OF THE TESTS.

Those who wish to repeat my experiments with the same tests can use either the whole series, or any selection that would present the same difficulty as the whole series. As the questions are arranged in order of difficulty this selection may be made by taking alternate items, or every third item, or the middle item of each section, or two items from each section equally removed from the middle of that section, or any other arrangement that contains a balanced number of easy and difficult items. Having made a selection, however, it would be well to adhere to it for the whole inquiry; for the series is not so carefully standardised that the items rise in equal steps: selections made in accordance with the above instructions will be approximately, not exactly, of equal difficulty. The objection to using a small number of items is that a few bright subjects may get them all right. This is not likely to occur if the full series be used. None of my own subjects got more than 32 marks out of the total of 34. The absurdities should be mixed with a few pseudo-absurdities, either those given above or any others of a similar kind: but the pseudo-absurdities should be ignored in marking the papers.

The percentage of absurdities detected and explained gives the grade of intelligence. The normal adult seems to get about 50 per cent. of the marks.

9. THE NATURE OF INTELLIGENCE.

The Editor of *The Journal of Educational Psychology* recently invited a number of leading investigators in America to state what they meant by 'Intelligence,' and the replies sent in by thirteen of them appear in the issues of the *Journal* for March and April, 1921. These replies reveal a surprising variety of opinions. No common nucleus of meaning is discoverable, unless it be that Intelligence is something that is measured more or less exactly by Binet's scale and by the American Army tests. Nor do the views of European psychologists show much more unanimity. It is possible, however, to group the various opinions under three heads: those that regard intelligence as a single ability common to all intellectual processes; those that regard it as a group of two or three abilities of varying degrees of generality; and those that regard it as representing no real entity but as merely a convenient term for the average of all specific abilities.

Of those writers who hold the first theory many accept Stern's[1] definition: "General adaptability to the new problems and conditions of life." Burt[2] defines intelligence as "inborn, all-round mental efficiency." Woodrow[3] calls it "a capacity to acquire capacity"—a phrase that suggests an endless regression. Spearman[4], whose name is most closely connected with this theory prefers calling the central factor "general ability" (symbolised by g) without committing himself to the view that g is identical with intelligence as commonly understood.

The second theory, the group theory, is held by Binet[5] who regards intelligence as involving at least three factors which are thus summarised by Terman[6]: "(1) Its tendency to take and maintain a definite direction; (2) the capacity to make adaptations for the purpose of attaining a desired end; and (3) the power of auto-criticism." Another interesting view of the composite nature of intelligence is suggested by Maxwell Garnett[7] who says: "It is probable that genius, as the word is commonly understood, is more directly measured by $\epsilon = \sqrt{g^2 + c^2}$ than by g (measuring General Ability or capacity to concentrate attention) alone, or even by c (measuring Cleverness, as we have defined it, or tendency to associate by similarity) alone." In other words the general factor g is compounded with one, or perhaps more than one, group factor to constitute Intelligence.

The third theory, the theory of the independence of the several traits that go to make up the mind, has been advocated with wavering confidence by Thorndike[8]. Though, in his latest pronouncement, he admits a certain possibility of prophesying from a test score "how well a person will do in other intellectual tasks," he warns us that "to assume that we have measured some general power which resides in him and determines his ability in every variety of intellectual task in its entirety is to fly directly in the face of all that is known about the organisation of intellect." Under the same head comes G. H. Thomson's[9] Sampling Theory of Ability which assumes that each man's mind is a sample group of qualities, resembling Mendelian units, which come from the hereditary stock. "If there be a General Factor at all, it might be the power to shake down rapidly into good team work."

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[1] *J. of Educational Psychol.* XII. 127.

[2] *J. of Experimental Ped.* I. 95.

[3] *J. of Educ. Psychol.* XII. 208.

[4] This *Journal*, V. 51–84.

[5] "L'intelligence des imbeciles," in *L'Année Psychologique* (1909), 1–147.

[6] *Opus cit.* p. 45.

[7] *Education and World Citizenship*, 125.

[8] *J. of Educ. Psychol.* XII.

[9] *Essentials of Mental Measurement*, by Brown and Thomson, 1921, 188–192.

In spite of this divergence and apparent inconsistency of opinion, many of the individual views seem merely to accentuate one or more aspects of a complex whole. At any rate the majority of the theorists would probably subscribe to the following tenets: Intelligence is innate mental ability which operates in many different ways; it is more fully manifested in the higher mental processes than in the lower; it is specially active in dealing with a situation which presents points of novelty (in other words with the solution of problems); it is more concerned with the dissecting, planning and re-arranging of the data of experience than with the mere reception of impressions.

The aspect or mode of intelligence which absurdity tests call into play seems to be the power to construct coherent wholes. It is that synthetic power which Bosanquet[1] regards as essential for inference. "Ultimately," he says, "the condition of inference is always a system....a group of relations of properties or things so held together by a common nature that you can judge from some of them what the others must be." Some indeed require the system to be apprehended as a spatial and concrete thing before they are satisfied. Lord Kelvin[2] did not at first accept Clerk Maxwell's electromagnetic theory of light, because he could not make a model of it. An absurdity test contains an incongruity—something that does not fit; and the failure to fit becomes apparent only when all the pertinent elements are brought together in the mind to form a whole. In these tests all the elements of knowledge requisite for a solution are just as familiar to the youngest testee as they are to the oldest. The oldest is robbed of his advantage of a larger store of knowledge, so that any difference in the response is due to a difference in synthetic power—provided the mental attitude is the same. If the attitude of the testee is one of suspicion; if he looks upon the test as a 'catch' and not a *bona fide* question; if he is so wary that he avoids the answer that appears most obviously right, it is clear that the results cannot be taken at their face value. When this frame of mind is suspected steps should be taken to dispel it before the test is given.

The probability being that intelligence is a composite thing, and means more than synthetic power, then the most that can be claimed, as the outcome of my investigation, is that, given a normal environment, a certain factor of intelligence—the power to integrate experience—arrives at maturity when the subject is sixteen years of age or younger. How far this factor correlates with the other factors—if there are other factors—this investigation does not show. From the way, however, in which Binet's absurdity tests fit in with the whole scale and correspond with the teacher's estimate[1], there is every reason to think that the correlation is high, and that no single type of test can gauge intelligence better than the absurdity test.

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[1] *The Essentials of Logic*, 140.

[2] *Life of Lord Kelvin*, by Silvanus P. Thomson, II. 835–36.

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[1] See Burt's forthcoming book, *Mental and Scholastic Tests*, Tables III and XXXI.

(*Manuscript received* 29 *March*, 1921.)

A VINDICATION OF THE RESONANCE HYPOTHESIS OF AUDITION.

II.

By H. HARTRIDGE.

(*From the Cambridge Physiological Laboratory.*)

1. *When the periodic stimulus applied to a vibrating resonator is reversed, or changed by* π, *the resonator must first come to rest and must then restart in time with the new period* (pp. 142–145).
2. *When a musical note is altered in phase by* π *the observer hears the sound die away momentarily to return a moment later to its original intensity* (p. 145).
3. *These facts cannot be explained unless there is a resonating mechanism somewhere in the auditory apparatus; and therefore Wrightson's theory cannot explain them* (pp. 145, 146).

1.

In a previous paper[1] I have described experiments explicable by the resonance theory (Helmholtz) but at variance with the displacement theory (Wrightson's) of hearing. I have also in the same paper answered Perrett's objections[2] to the resonance theory.

In this paper further experimental evidence will be given which is in favour of the resonance hypothesis and at variance with its antagonist. This evidence is based on the principle that if the periodically applied forces, which are causing a mechanical system possessing resonance to vibrate, are reversed, or altered in phase by π, then the motion of that system must first be arrested and must then be restarted in the new phase. As examples of this principle the case of a body of men crossing a suspension bridge and setting it swinging may be cited; if the men change step the motion of the bridge will first be arrested and will then be restarted again in time with the new step. This process will be repeated each time the step is changed.

It should be noted that any mechanical system that is to exhibit this phenomenon must possess a natural period of vibration and the stimulus undergoing the change of phase of π must have the same rate.

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[1] This *Journal*, 1921, xi. 277.

[2] *Questions of Phonetic Theory*, 1919, ch. v, 32.

With regard to the different mechanical structures which together compose the ear, all authorities are now agreed that the drum, its ossicles and muscles together, form a mechanism which is dead beat and has no apparent natural period. With regard to the organ of Corti of the cochlea one authority (Helmholtz) states that certain parts (the different fibres of the basilar membrane) have natural periods of their own, whereas other authorities (e.g. Rutherford and Wrightson) postulate that this mechanism is dead beat.

So far as I am aware neither authority makes any statement as to the kind of mechanism (if any) in the auditory area of the brain where the nervous impulses are interpreted as sensations.

The essential difference then between the rival theories is that one (Helmholtz's) says that the ear contains a mechanism, parts of which have natural periods and act as resonators, whereas the other denies this. Now it would seem that the principles outlined above could be applied to the problem of providing a crucial test on this point. For if on suddenly changing the phase of a certain musical note by π there is observed to follow a temporary cessation of the note followed by its return to the normal, then this would seem to give definite evidence for the presence somewhere in the ear of a resonator mechanism, the natural period of which corresponds to that of the note. If further all musical notes behave in a similar manner, then there must be a series of such resonators in the ear. If, on the contrary, a change in phase of π made in turn in a series of notes, is found to produce no audible effect, then it would seem clear either that no resonators having natural periods are to be found in the ear, or alternatively, that the sensations conveyed by the auditory nerve lag so far behind the change in amplitude of motion of the hair cells that the temporary stoppage of the motion is not consciously recognised. Now we know from other experiments that even slight changes in amplitude are very readily perceived, the alternative mentioned above is therefore clearly ruled out.

The physical difficulties of changing the phase of a musical note suddenly by π, without at the same time introducing some accompanying change, were found to be very great. Several experimental methods had to be abandoned in turn either because the change could not be effected without noise or because the intensity or quality (or both) of the note were altered. The only method found satisfactory was one employing the de la Tour's syren for producing the musical note.

The change in phase of π was effected as follows: The mounting of the wind chest of the syren was modified so that a rotation could be effected about a vertical axis (the syren disc shaft was also vertical). There were eighteen holes in both the syren disc and the wind chest, so that this number of puffs of air were produced during each complete revolution of the disc. The angular interval between successive puffs was therefore 20 degrees (360° ÷ 18). By means of fixed stops (covered with rubber to absorb shock and so prevent noise) the rotation of the wind chest was limited to one half the above angle, viz. 10 degrees.

Supposing then the syren disc to be rotating to the right at a speed of 100 per second, then there will be 1800 puffs of air produced per second and a uniform note of 1800 vibrations per second will be the result.

Suppose now, that a puff of air has at a given instant issued from the holes in the disc and that immediately afterwards the syren disc is suddenly rotated 10° in the *same* direction as the disc, then the disc will have to rotate 20° + 10° before the next puff can be produced. That is to say, the normal time interval between the puffs is increased from T to $\frac{T \times 3}{2}$, or from $\frac{1}{1800}$ second to $\frac{1}{1800} \times \frac{3}{2}$.

Now in the normal note compression of the air follows compression at an interval of $\frac{1}{1800}$ and therefore rarefaction follows compression at an interval of $\frac{1}{1800} \times \frac{1}{2}$. If then by rotating the wind chest an interval of $\frac{1}{1800} \times \frac{3}{2}$ (i.e. $\frac{1}{1800} + \frac{1}{1800} \times \frac{1}{2}$) is introduced, it is clear that the compressions in the air in the note which subsequently follows must occur at those instants which would have been occupied by rarefactions and *vice versâ*; that is that a change of phase of π has been introduced.

In the same way when the wind chest is rotated 10° in the *opposite* direction to the disc (so that the holes tend to approach one another more rapidly), the disc only need rotate 10° for a fresh set of holes to coincide and therefore the time between the puffs is reduced from T to $\frac{T}{2}$, i.e. in the case we have assumed from $\frac{1}{1800}$ sec. to $\frac{1}{1800} \times \frac{1}{2}$.

It follows therefore that in this case also compression of the air will occur at the instant that rarefaction would have occurred and therefore a change in phase of π will have been introduced.

We have now to consider what will happen if the rotation of the wind chest cannot be effected suddenly, e.g. suppose the rotation of 10° to take one second. Then it will be found that during this second $1800 + \frac{1}{2}$ (or $1800 - \frac{1}{2}$) puffs will have occurred, that is that the frequency of the note is 1800·5 (or 1799·5) per second, according as the wind chest is rotated in the opposite or the same direction as the disc. The note during rotation of the wind chest will therefore be found to become slightly sharp during the instant that the wind chest is rotated one way and slightly flat during the instant that it is rotated the other.

Observation shows that as the time required to rotate the wind chest is shortened at first, the more obvious does the change in pitch become; but as the time is further decreased the less obvious does it become, until finally when the change is made as suddenly as possible the momentary alteration in pitch passes unnoticed.

2.

A new phenomenon however is now observed; for when the change in phase of the note is effected, the intensity now suffers a sudden diminution, while the pitch remains unaltered. After having fallen to zero intensity, the note rapidly recovers its original strength or even momentarily reaches a higher loudness than before.

Physiologically the phenomenon has the characteristics of a beat (that produced for example when two organ pipes sounding together have a slightly different pitch) and is best so described. It is found to occur equally well with all notes within the range of the syren, and it is readily heard by a large audience.

The following observations show that the beat is not of instrumental origin: (1) If the wind chest be rotated slowly when the syren is in operation no beat is heard. (2) If with the syren in rotation the wind be momentarily turned off and the wind chest be then rotated either slowly or fast no beat is heard.

3.

The presence of this beat is readily explained on the resonance hypotheses because the change in phase of a musical note by π will put it out of step with the resonators which it has set in motion. These resonators must first be brought to rest and must then commence vibrating again until once more they reach a steady state. The temporary rise of the intensity of the note above the general level after the beat, may be due either to error of judgment, successive contrast, or possibly to temporary excessive vibration on the part of the resonator soon after the recommencement of its motion.

It would appear very difficult to explain the presence of the beat on any displacement hypotheses. If it is suggested that the beat is really a short silent interval brought about by an increase in time between the successive impulses from T to $T \times \frac{3}{2}$ which the ear perceives owing to its dead-beat properties, then it can be objected that in the first place experiment shows that a short interval of silence, produced artificially in a musical note, is perceived by the ear as a short noise[1] (a phenomenon itself already explained on a resonance but not on a displacement hypothesis), and that in the second place it is only when the wind chest of the syren is rotated one way, that the time between successive impulses is temporarily increased, when on the contrary it is rotated the other way, the impulses should come closer together and therefore on the displacement hypothesis no silence should be produced; yet experiment shows that the beat is as clearly heard in this case as it is in the other.

Two criticisms can therefore be advanced against the displacement hypothesis: (1) that it does not explain why a short silence in a musical note is heard as a short noise apparently added to the uninterrupted note, and (2) that it does not explain the beat which is produced when the phase of a musical note is suddenly altered by π.

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[1] This *Journal*, 1921, XI 277.

(*Manuscript received* 4 *July*, 1921.)

A PORTABLE CHOICE REACTION TIME APPARATUS.

By A. G. SILLITOE.

(*From the Cambridge Psychological Laboratory.*)

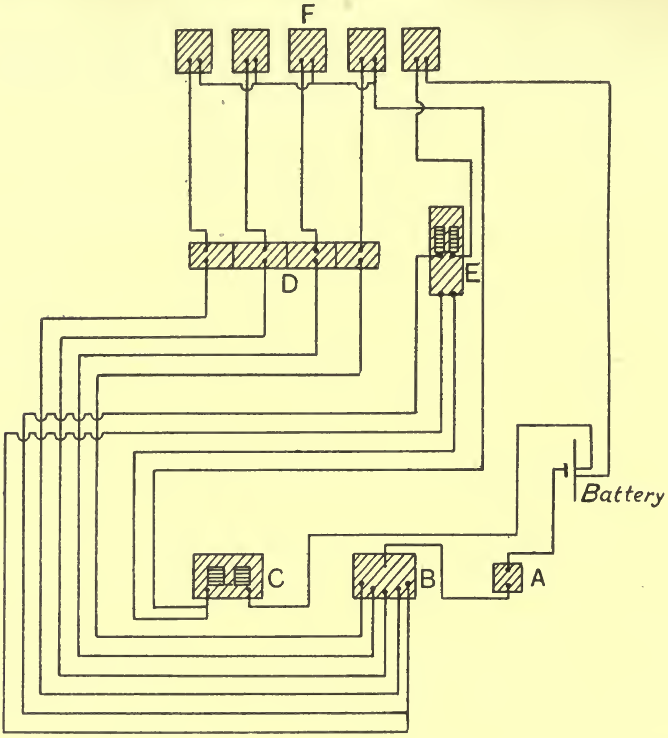
The apparatus about to be described is a development of the instrument devised by the late Dr Stamm for testing the reaction-times of aviators during a flight. The special features of this development are: the lightness and portability of the apparatus, and the new electrical control of the stop-watch.

The whole is enclosed in a box 12″ × 8″ × 8″, its total weight being about 8½ lbs. Two opposite sides of the box are hinged, and may be opened outwards, so that when the apparatus is in use these sides lie flat on a table. To them most of the working parts of the apparatus are affixed: they will be referred to as the experimenter's side, and the subject's side respectively.

On the experimenter's side is a 100th secs. "Mikograph" stop-watch (*C* in diagram), firmly fixed in position, and controlled (as will be explained later) by an electro-magnet placed immediately below the watch. On the right of this is a five-way switch *B* connected with the various stimuli, and on the right of the five-way switch is the experimenter's switch *A* for exhibiting a given stimulus and starting the watch.

On the subject's side are situated, in a position convenient for the fingers, five keys *F* after the Morse key pattern. Any of these, when depressed, stops the watch and, if the correct reaction is made, interrupts the current which exhibits the stimulus, each key being connected with its own stimulus. Underneath the topside of the box and facing the subject are four small compartments *D*, each containing a 2 v. lamp, the front of the compartments being fitted with red, blue, green and white glass respectively. Fitted inside the box, on the subject's left, is a small specially constructed hammer *E* for use as a sound stimulus. The experimenter has then a choice of five reactions to one of five possible stimuli.

The box contains two dry cells and two spare cells. The mechanism is satisfactorily operated by a current of approximately 2·5 volts.



A. Experimenter's Switch. *B.* Five-way Switch. *C.* Electro-magnet operating watch. *D.* Visual Stimuli. *E.* Sound Stimulus. *F.* Subjects' Reaction Keys.

DIAGRAM OF CONNEXIONS.

The mechanism for starting and stopping the watch may be briefly described. Connected with the armature of an electro magnet is a light lever which is fixed outside the watch, but passes through an opening drilled in the watch case. In the resting positions, when no current is passing through the magnet, this lever allows a spring which is a part of the ordinary mechanism of the watch, to exert pressure upon a revolving serrated wheel, thus bringing the wheel out of gear with a second serrated wheel. The second wheel is therefore now at rest, and as the hand which revolves around the dial-face is fixed on the pinion of this wheel, no time record is taken. At the beginning of the experiment, then, the watch is started, as in an ordinary stop-watch, by pressure upon the lever at the top; but the hand does not revolve until the current is allowed to pass through the electro-magnet. When this happens, the attraction of the armature prevents the aforesaid spring from exerting pressure upon the revolving serrated wheel. The latter is therefore, by the operation of the ordinary starting mechanism of the watch thrown into gear with the wheel which carries the dial hand, and the time record at once begins to be taken.

In a long series of experiments either the watch may be kept going the whole time, the time of each reaction being read off as in the case of the Hipp Chronoscope, or, if it is preferred, the hand may be restored to zero after each reaction by pressure upon the lever at the top of the watch.

The apparatus has been given a large number of trials in practical work in the laboratory and has been found highly reliable and satisfactory. In a long series of trials with the control hammer, the mean variation for an average time of 526^{σ} was found to be 10^{σ}.

(*Manuscript received* 1 *August*, 1921.)

FEELING-TONE IN INDUSTRY

(*A Report to the Industrial Fatigue Research Board.*)

By B. MUSCIO.

I. *The 'Feelings of Fatigue'* (pp. 150–152).
II. *Experimental Data* (pp. 152–158).
III. *Discussion* (pp. 158–161).
IV. *Summary* (pp. 161, 162).

I. The 'Feelings of Fatigue'

At an early stage in the investigation of 'fatigue' it became clear that the 'feelings of fatigue' were not reliable criteria of the 'capacity to work.' Rivers states this conclusion in the following words: "In the performance of mental work especially, decided sensations of fatigue may be experienced when the objective record shows that increasing and not decreasing amounts of work are being done; and there may be complete absence of any sensations of fatigue when the objective record shows that the work is falling off in quantity, or in quality, or in both[1]." And Thorndike writes: "Kraepelin has emphasised this fact [that the feelings of fatigue are not measures of mental ability] but chiefly in connexion with the claim that we can be mentally fatigued without feeling so. What I wish to emphasise is that we can *feel* mentally fatigued *without being so*[2]."

In dealing with the data the general character of which has been expressed in the above quotations, the factors actually correlated have been *output* and 'feelings of fatigue.' The output, however, has always been *experimentally obtained*, and under conditions of *maximal effort* on the part of the subject; and in such experimental conditions it has usually been regarded as the best attainable measure of 'fatigue.' But 'fatigue' investigations have been chiefly concerned with the attempt to find a *test* of 'fatigue.' Consequently—since (1) output was accepted as a measure of 'fatigue' and (2) output and 'feelings of fatigue' could not be correlated by any straightforward formula—'feelings of fatigue' were altogether abandoned as a possible 'fatigue' *test*. And, largely because of the preoccupation of 'fatigue' investigations with the question of a 'fatigue' *test*, the 'feelings of fatigue' have come to be almost entirely disregarded, not only in 'fatigue' *test* investigations, but also in the investigation of the characteristics of normal working activity; and their exclusion from these investigations has appeared the more final in view of the failure of physiological psychology up to the present to arrive at a plausible conception of the physiological concomitants of 'feeling.'

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[1] *The Influence of Alcohol and other Drugs on Fatigue*, 2.

[2] *Psychol. Rev.*, 1900, VII, 481.

So far as the question of a 'fatigue' *test* is concerned, this position doubtless is final, because of general considerations affecting the question as well as the special relation of the 'feelings of fatigue' to output. But from the point of view of understanding what happens in much of the normal work of the world, these feelings seem far from negligible. They might indeed be regarded as negligible if their correlation with output were *nil*, and many statements could be quoted which appear to be based upon this assumption. But though what might be called the paradoxes of activity feelings are striking, it would be false to say that 'feelings of fatigue' bear *no* relation to activity or to output. If the 'feelings of fatigue' were to be used as a *test* of 'fatigue,' the coefficient of correlation between the two factors ('fatigue' and 'feelings of fatigue') would need to be approximately + 1; but to say that these feelings cannot be used as a 'fatigue' *test* is not to imply that the correlation between the two factors is approximately ·00. And, in fact, if 'fatigue' be measured indirectly by reference to output, this correlation would certainly be considerable[1], as common experience shows, and as is obvious from the data given below. This being so, there are possibly many characteristics of normal human 'work' that find their explanation in human *feelings.* For instance, any fall in normal industrial output curves may be very largely due, in a sense to be explained below, to the painful feeling-tone of 'fatigue' feelings. The data now to be presented bear upon this point.

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[1] Cf. T. Arai, *Mental Fatigue*, 114: "we conclude that the feeling of fatigue is somewhat, though far from perfectly, correlated with the state of mental inefficiency." It was found, for instance, that in memorising German equivalents of English words the average time taken per word when the subject (the writer herself) felt 'good' was 15·7″ (A.D.=4·1″), when she felt 'medium' it was 16·9″ (A.D. = 3·5″), and when she felt 'tired' 18·8″ (A.D. = 5·37″). In mental multiplication of a 3-place by a 3-place number the time taken per example when the subject felt 'good' was 138·8″ (A.D. = 30·6″), when she felt 'medium' 142·9″ (A.D. = 20·6″), and when she felt 'tired' 151·3″ (A.D. = 28·8″). Of nine adult subjects who did two hours of mental multiplication, five felt 'medium' at the end of the period and four felt 'tired.' Both before and after this two hours of work nonsense syllables were memorised. For the five who felt 'medium' after work "the average ratio which the average number of nonsense syllables remembered *after* is to that *before*" is 101·2 (A.D.=3·1), while for the four who felt 'tired' it is only 80·6 (A.D. = 14·9). In this case correlating change of feeling for the worse with decreased efficiency at the end of the work period gave a coefficient of + ·99. Finally, for twelve adult subjects, decrease of efficiency in mental

II. Experimental Data.

The question concerned in obtaining the data given here was whether the 'fatigue' feelings of a group of workers vary in any characteristic way throughout a day's work. That they would do so might seem to be obvious, but it is by no means obvious just what variations they would show. Attempts were therefore made to obtain records of their 'feelings' from two groups of workers at regular intervals during each of several days. The 'feelings' concerned were what would be regarded generally as 'feelings of fatigue.' No attempt was made to analyse them minutely. The subjects were asked merely to state whether or not they were experiencing certain degrees of 'fatigue' feelings at certain periods throughout the day.

A. Data obtained from a group of twenty women medical students.

In the carrying out of an experiment dealing with the relation of certain mental tests to mental 'fatigue[1],' a group of twenty women medical students[2] was given three tests (lasting altogether about four minutes) once an hour; the first test period on any day being at 10 a.m. and the last at 5 p.m. Thus, there were 8 test periods in a day, any two separated by approximately an hour; and in the intervals between the test periods (and also for an hour before the first) the subjects carried on their regular academic work—attendance at lectures, demonstrations,

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multiplication (as a result of two hours work) was correlated with fatigue feelings and yielded a coefficient of + ·37 (*ibid.* 72–79).

G. E. Phillips, also (*Mental Fatigue*, 65–66) presents data bearing upon this point. He classified fatigue feelings as 'Not tired at all,' 'Slightly tired,' 'Fairly tired,' 'Tired,' and 'Very tired,' and correlated these feelings with depreciation in multiplication and cancellation tests during ninety minutes of continuous work with these tests. The correlation coefficient between feeling tired and depreciation in multiplication in these conditions was ·42 (p.e. = ·086), though between feeling tired and cancellation it was not significant. The subjects were 35 boys between the ages of 11½ and 13½. These subjects however were working at maximal effort and were doing their best to disregard fatigue feelings—which indeed was also true of Arai's subjects.

[1] See This *Journal*, 1920, x. 327–344.

[2] These students were taking the medical courses at the London Royal Free Hospital School of Medicine for Women.

etc. The experiment lasted for 5 days. The work in this case was exclusively 'mental,' and the question here to be considered was whether the 'fatigue' feelings varied in any characteristic way during a day of such work. The subjects were therefore required, before starting the tests at each test period, to record in writing on the cover of the test leaflets their feelings of relative 'fitness.' The printed instructions were:

Underline the word or words best corresponding to your feelings.

Fit.
Fairly Fit.
A Little Tired.
Very Tired.

The subjects (who were very trustworthy) were of course instructed to make their records as exact as they could, and it is fairly certain that they did. It was part of the plan of the experiment that none of the subjects was to discuss it or anything connected with it with anyone while it lasted. As regards the 'feelings of fatigue,' therefore, no subject knew what any other subject had recorded while the experiment was in progress.

In order to be able to compare these feelings at different times of the day, numerical values were arbitrarily assigned to each of the four degrees of felt fitness recorded. 'Fit' was given the value 4, 'Fairly Fit' the value 3, 'A little Tired' the value 2, and 'Very Tired' the value 1. The 'fatigue' feelings of the whole 20 subjects at any test period could thus be expressed numerically.

The question of course arises how far the ratio of the numbers 4, 3, 2 and 1 expresses the relations between the four degrees of 'fatigue' feelings; but to answer this question one would require a 'hedonic calculus'—indeed, a 'hedonic calculus' for each subject. Still, there is no doubt that 'Fit' indicates an experience more pleasantly-toned than that indicated by 'Fairly Fit,' and similarly that the experience indicated by 'Fairly Fit' is more pleasantly toned than that indicated by 'A little Tired,' and so on. But whether the hedonic difference between the experiences indicated by 'Fit' and 'Fairly Fit' is identical with that between the experiences indicated, for instance, by 'Fairly Fit' and 'A little Tired,' is a question to which no given answer could be accepted as absolutely accurate. It is therefore assumed that the values here assigned to the four degrees of 'fatigue' feelings are sufficiently indicative of their differences to give a valid idea of diurnal changes in these feelings.

In Table I and Fig. 1 the results are given for the last four days of the experiment, the first day being omitted as an adjustment period.

The *average* feeling is employed for comparative purposes. Thus, the number given as the 'Mean' at any period in Table I is obtained by the following formula: (No. of subjects recording themselves 'Fit' × 4 + No. recording themselves 'Fairly Fit' × 3 + No. recording themselves 'A little Tired' × 2 + No. recording themselves 'Very Tired' × 1) ÷ the total number of subjects (20). For instance, on Wednesday of the experiments (see Table I) at 10 a.m., 8 of the 20 subjects recorded themselves as 'Fit,' 6 as 'Fairly Fit' and 6 as 'A little Tired.' Hence, we have

TABLE I.

Numerical expression of diurnal changes in the feelings of weariness of 20 women medical students engaged in academic work.

		Hour of Day							
		10 a.m.	11 a.m.	12 (noon)	1 p.m.	2 p.m.	3 p.m.	4 p.m.	5 p.m.
Tuesday	Mean	3·22	3·21	3·10	3·00	3·10	2·90	2·42	2·42
	M.V.	·69	·66	·66	·63	·66	·66	1·01	·59
Wednesday	Mean	3·10	3·15	2·95	2·75	3·10	2·90	2·75	2·40
	M.V.	·72	·76	·73	·52	·63	·63	·60	·56
Thursday	Mean	3·28	3·40	3·44	3·12	3·12	2·60	2·20	2·40
	M.V.	·56	·54	·44	·40	·33	·66	·44	·87
Friday	Mean	3·44	3·33	2·83	2·60	3·00	2·80	2·44	2·44
	M.V.	·68	·67	·46	·63	·44	·64	·88	·88

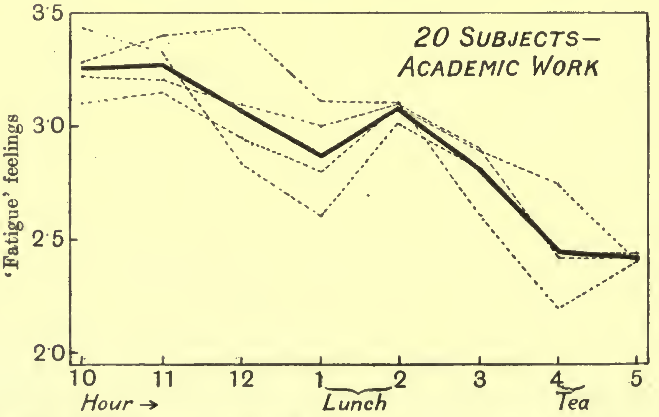

Fig. 1. The broken lines are the curves for each of the four days; the solid line is the average of the four daily curves.

$$\{(8 \times 4) + (6 \times 3) + (6 \times 2)\} \div 20,$$

which gives 3·1 as the numerical expression of the *mean* 'fatigue' feeling at that period. It will be observed that the larger this number, or the higher the curve in Fig. 1, the more pleasantly toned are the activity feelings of the group.

Lunch was taken between 1 p.m. and 2 p.m., and tea, occupying about twenty minutes, soon after the 4 p.m. test period.

B. Data obtained from a group of fifteen young women typists.

The data now to be described were obtained in a typewriting office from a group of fifteen young women typists. The working conditions in the office were good: physical features, such as lighting and ventilation, were satisfactory, and the personal relations between the management and the employees were excellent. Hours of work were 7¼ per day, except on Thursdays when they were 4. The morning spell was from 9 a.m. until 1 p.m. without any regular break, though an interval for refreshment was allowed about half-way through the spell if it were desired. After lunch work was resumed at 2.30 p.m. and ceased at 6 p.m., there being a break of fifteen minutes for tea at 4.30 p.m. Wages were on a weekly time basis.

The work in the office consisted mostly of typing from MSS. but involved as well certain clerical duties other than typing.

During three days in one week (Tuesday, Wednesday and Saturday) this group of fifteen typists recorded at intervals their feelings of relative weariness. Blanks for recording these feelings were distributed to them at the times when the records were desired: the character of the blanks is shown below. It was intended that these should be filled out 10 times during the day; but it seemed desirable that the distribution and collection of them should be made by one of the office employees rather than by a stranger, and this resulted in their not being distributed at all of the times originally determined upon. On two of the days they were distributed 8 times only.

The Record Blank.

FEELINGS OR FITNESS OR TIREDNESS

Underline the word or words *which best describe your feelings* at this period.

1. DEGREES OF TIREDNESS.
   Fit (not tired).
   A Little Tired (generally, or in some special part).
   Tired (generally, or in some special part).
   Very Tired (generally, or in some special part).

Note also, *by underlining,* whether you have any *aches, pains, or feelings of tiredness* in any of the following parts of the body: Eyes, Back of neck, Head, Shoulders, Arms or wrists, Sides or back, Legs.

2. OTHER FEELINGS.
   Feeling of *clumsiness* (awkwardness).
   Feeling of *boredom* (small or great).
   Feeling of *excitement.*
   Feeling of *tension* (strain).
   Feeling of *hunger.*

Note any other feelings you may have..

Name..............................*Hour*.................................

General character of last hour's work ....

The subjects, who were naturally not trained in self-observation, were very carefully instructed to record as exactly as possible just how they *felt* at the various times, and to add to the blank if it did not provide a sufficient range of feelings. In some instances additions were made; *e.g.* 'pressure on head' was written in several times by one typist. The fact that a number of alternatives was presented in the blank would operate against suggestion. It was made clear to the subjects that the records would be treated confidentially, and it was requested that each subject should preserve absolute secrecy concerning her records until all the data were collected. On the whole, therefore, these records seem as reliable as any that could be obtained from subjects not specially trained in introspection.

For the purpose of comparing the feelings at different times of the day, the data were treated in the same way as the data obtained from the group of medical students; the only difference being that an attempt was here made to make finer distinctions between various weariness feelings. This difference is indicated by the numerical values here assigned to the different feelings recorded, which were as follows: 'Fit' = 4; 'A little Tired, in some special part' = 3; 'A little Tired, generally' = 2·5; 'Tired, in some special part' = 2; 'Tired, generally' = 1·5; 'Very tired, in some special part' = 1; 'Very Tired, generally' = 0. For comparative purposes, *average* feelings were used as before, the method of obtaining them being identical with that explained in connexion with the former group. The results are shown in Table II and Fig. 2, which are to be read in the same way as Table I and Fig. 1.

The records made in the second part (2) of the blank were relatively few and unimportant; the results given below are therefore concerned altogether with the feelings recorded in the first part (1) of the blank.

The detailed records of 'aches, pains, or feelings of tiredness' (see blank) in special parts of the body give concrete feeling to these generalised results. The parts of the body mentioned most frequently are the eyes, the head, the neck and the shoulders. The individual records tend to become extensive as a spell of work proceeds, and towards its end assume a form showing clearly the predominance in the experience of the workers of unpleasant feeling-tone. The following is the record of one typist towards the end of a morning's work: 'Tired generally; in

Table II.

Numerical expression of diurnal changes in the feelings of weariness of 15 young women typists.

Hour of Day (*Tuesday*)

	9.15 a.m.	10.15 a.m.	11.10 a.m.	12.10 p.m.	1 p.m.	2.35 p.m.	3.35 p.m.	4.30 p.m.	5.15 p.m.	6 p.m.
Mean	4·00	3·73	3·58	3·00	2·93	3·85	3·44	2·90	2·72	2·61
M.V.	·00	·39	·60	·54	·40	·27	·60	·50	1·00	·70

Hour of Day (*Wednesday*)

	10.30 a.m.	11.15 a.m.	12 (noon)	12.50 p.m.	2.40 p.m.	3.45 p.m.	4.50 p.m.	5.50 p.m.
Mean	3·37	3·25	2·77	2·27	3·65	2·94	2·82	2·30
M.V.	·78	·57	·62	·53	·56	·50	·74	·84

Hour of Day (*Saturday*)

	9.15 a.m.	10·30 a.m.	11·30 a.m.	12 (noon)	12.50 p.m.	2.30 p.m.	3.30 p.m.	6 p.m.
Mean	3·72	3·50	3·40	3·05	2·88	3·67	3·05	2·58
M.V.	·42	·64	·70	·55	·79	·50	·61	1·07

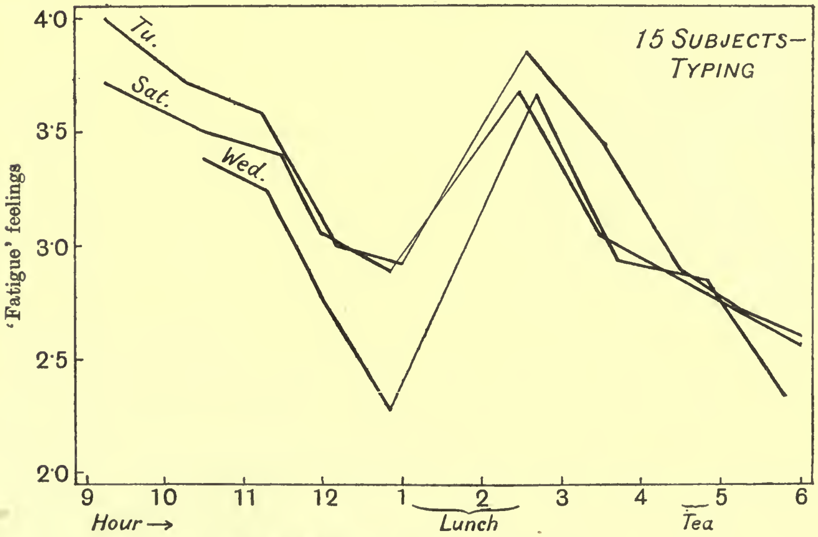

Fig. 2. Each curve represents one day.

head, eyes, neck, wrists; hungry[1], bored, feeling of strain.' The records make it clear that as work proceeds the area of the body in which unpleasantly toned sensations are localised becomes wider and wider.

III. Discussion.

The main tendency of the data obtained from both medical students and typists is perfectly definite[2], and might be expressed by saying that during continuous work feeling-tone, in so far as it is dependent upon 'feelings of fatigue,' tends to become more and more unpleasant. This conclusion would probably be accepted as obvious; and it is not implied that the results given above were necessary to establish it. These results have been presented in order to give definite expression to common experience concerning the relation of continuous activity to feeling-tone, and thus to make possible a comparison of diurnal variations in feeling-tone during work with diurnal fluctuations in industrial output.

Such a comparison shows at once that the normal industrial output curve and the curve for feeling-tone during work are in certain respects strikingly similar. At the same time they are in other respects different. Thus, whatever the class of work, output tends to fall off in quantity towards the end of a spell, and to rise again after the mid-day interval. In these respects the output and feeling-tone curves are so nearly the same that one might reasonably suppose *either* that these variations in output are directly due to the variations in feeling-tone *or* that both sets of variations are due to variations in some third factor—a physiological factor. On the other hand, with most classes of work output *tends to improve* over the first part of the morning spell; and while there is some evidence of a similar improvement in feeling-tone in the results obtained from the medical students, there is no evidence at all of such improvement in the results obtained from the typists.

Now the normal response to the feelings of weariness is to slacken effort or to cease work altogether. This response occurs independently of any deliberate intention of effecting a decrease in the unpleasantness of the feeling by means of it, although normally its occurrence produces this result immediately[3]. The smith who swings his hammer until every movement of his arm causes an 'ache' relieves himself at once of this unpleasant sensation by simply ceasing to hammer. Such facts as this explain the universal *belief* that relaxation of effort lessens the feeling of weariness; and if this belief be taken in conjunction with the fact that the normal *involuntary* response to the feelings of weariness is to slacken effort or to cease work altogether, we may be certain that where such feelings occur work will be slowed down, and will be slowed down the more the greater the intensity of these feelings. There can be no doubt what the normal response would be to 'Tired generally; in head, eyes, neck, wrists; hungry, bored, feeling of strain.' Have we not here the explanation of decreased output towards the end of a spell of work?

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[1] In the last two recording periods of the morning, hunger is frequently mentioned.

[2] The curves given here are of the same character as those of Thorndike (*J. of Applied Psychol.* I. 3, 266–267) for the variations in 'satisfyingness' felt during 2 and 4 hours work at grading compositions. Thorndike had 29 adult subjects.

[3] Cf. Ward's *Principles of Psychology*, ch. II, § 5. On p. 55 it is said that if a "painful presentation is a movement, it is suspended."

It may be argued that such an explanation is at any rate not complete because the feelings of weariness with their unpleasant feeling-tone will themselves be determined by physiological conditions, and that a complete explanation would need to determine the precise nature of these conditions. This is certainly not denied. But what it seems reasonable to deny is that the physiological conditions concerned, whatever they might be, would have anything to do with output except as affecting feeling-tone; for it is this which seems to stand in a direct dynamic relation to the will of the worker[1]. Without entering further into a discussion of what is or is not a complete 'explanation' here, it may be said that the character of the feeling-tone towards the end of a spell of work *should* affect output in precisely the way it is found to be affected; and it may be concluded that any explanation that neglects feeling-tone is incomplete.

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[1] Cf. Thorndike, *Educational Psychology*, III. 124–125: "The behaviour which results in certain products such as sums done...is subject to the laws of all behaviour, and to no others. If a continuance of the productive responses at the same speed and in such form as to give equal quality, is satisfying to the individual concerned, he will continue them. If such continuance brings discomfort, he will tend to stop them altogether, or to intermit them, or to make them in such altered form and speed as lets them bring relative satisfaction. Stopping the work outright does not of course occur in the great majority of experimental investigations of fatigue, but is very common in ordinary mental work." And Joteyko writes (*The Science of Labour*, 20–21): "The psychic factor which regulates the expenditure of energy of the human motor...is the sensation of fatigue...Pain and fatigue are the physical and psychical conditions which we are endeavouring to avoid throughout our lives."

In Thorndike's valuable discussion of 'fatigue' what is emphasised are the normal responses to the 'satisfyingness' or 'unsatisfyingness' of continuing at work. These opposite 'feelings' are of course very complex and arise from a variety of conditions. Thus, for instance, the 'unsatisfyingness' of work may be due primarily to the fact that continuance at work involves the temporary repression of various social impulses, or simply to the fact that it is producing sensory aches and pains. In the present paper the 'satisfyingness' of continuing at work is considered only in so far as it is dependent upon 'fatigue' feelings.

It may be noted that where continuing at work involves the inhibition of strong impulses the total work of the organism is not at all measured by what is ordinarily called the product or output of work. The internal work involved in such inhibition of impulses gives no obvious indication of its occurrence, though its effects may be serious.

If this view is correct, it is necessary to try to explain why the curves for the 'fatigue' feelings and for industrial output are not in agreement throughout; why, that is, if these feelings explain or help to explain certain features of output curves, they do not explain all features. In particular, why should output improve during the early part of the day, since, assuming the data given above to be reliable and typical, the 'feelings' of fatigue apparently become more intense almost from the very beginning of work?—It would seem that the explanation of this fact should be in terms of the operation of incentives and warming up.

The incentives to much modern industrial work are certainly not exclusively the feelings of pleasure derived from the mere performance of it. Indeed, such work is frequently so confined and specialised that its performance can yield nothing very satisfying in itself; nothing comparable, for instance, to the pleasurable cœnaesthesis resulting from the moderate or even vigorous exercise of the whole body. When these conditions obtain, work is carried on because of incentives (including habits) extraneous to the work itself. In so far as the tendencies excited by these incentives are being realised through the continuance of the work, the feeling-tone of the worker will be to some degree pleasurable, and thus will contribute to the further continuance of the work. As the 'feelings of fatigue' become more definite, the worker will oscillate between the two responses that are characteristic of pleasurable and painful feeling-tone respectively: if he attends to the pleasurable feeling-tone aroused by the knowledge that he is accomplishing his ends, he will tend to continue his work without relaxation of effort, whereas if he attends to the 'feelings of fatigue' with their painful feeling-tone he will tend to relax his effort. Extraneous incentives will operate more or less effectively so long as the mere performance of the work itself does not become obviously painful. Consequently, although feeling-tone, in so far as it is dependent upon 'fatigue' feelings, may become more and more unpleasant from the very beginning of a spell of work, effort may not be relaxed until its painfulness reaches a certain degree of intensity. Of course, these feelings may be altogether disregarded by the worker, as they often have been under experimental conditions; but normally they are not. The degree of intensity which they must attain before they give rise to a slackening of effort is doubtless different for different persons and for the same person at different times, but it will generally be directly related to the strength of incentives.

The pleasurable feeling-tone arising from the sense of realising tendencies excited by incentives extraneous to the work will not, however, explain the early actual improvement in output (during a day's work); all it does is to provide a reason for maintaining for some time the working situation and would thus only seem to guarantee that output would not actually deteriorate from the very beginning of a spell of work. To explain the actual improvement in output in this early stage, recourse must probably be had to the physiological factors (and perhaps psychological[1] factors also) forming the basis of warming up, the 'facilitation' due to which would more than compensate for any 'inhibition' due to 'fatigue' feelings of mild intensity. Granting such factors, the cause of the divergence between the output and 'fatigue' feelings curves in the early part of a spell of work seems clear enough.

While there can be no doubt what will be the effects of the feelings of weariness (including local aches due to the maintenance of one posture or one set of movements over long periods) upon immediate effort and output, at least when these feelings have attained a certain intensity, their more remote effects upon the preservation of health are quite unknown. They may be a preventive of over-work, or, as Thorndike[2] suggests, they may be a useless by-product of activity to which hygiene need pay no attention—a "mere accident" of activity which "habitual disregard may abolish," or suppress. The field for research here is certainly an interesting one, especially in view of the widespread belief that the seeking and avoiding reactions characteristic of pleasure and pain are of profound biological significance.

IV. SUMMARY.

The object of this paper has been to suggest the importance of considering certain of the subjective phenomena of 'fatigue' when dealing with the characteristics of normal work. Curves representing diurnal variations in the 'feelings of fatigue' caused by work were given, and these show certain striking resemblances to common industrial output curves. It is strongly suggested that the deterioration of output towards the end of a spell of work is a direct response to the painful feeling-tone of these 'feelings of fatigue.' These feelings may thus play a far greater part in normal work than current 'fatigue' investigation, engrossed with physiological problems, recognises. And it is suggested that 'feelings' generally—and not merely 'fatigue' feelings—should be accorded more notice in connexion with various sides of industrial life than they at present receive.

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[1] Cf. Thorndike, *Educational Psychology*, III. 76: "The notion of Warming-Up...has included the diminishing annoyance of a process due to the progressive inattention to competing desires, interest in achievement, and the like."

[2] *Psychol. Rev.* 1900, VII. 482.

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THE INTELLECTUAL RESPECTABILITY OF MUSCULAR SKILL[1].

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I. WHAT IS INTELLECT?

THE word intellect has been used in many senses, of which at least two are important in this connexion[2]. It may cover all forms of cognitive events, in which case intellection becomes merely the equivalent of cognition. On the other hand, it may designate the processes of elaboration which the mind applies to its material, and in particular that kind of elaboration which leads to conceptual thinking. The point of view which this paper seeks to justify is that, in both these senses of the word 'intellect,' muscular skill has a higher intellectual value than is usually assigned to it, and that this value is susceptible of being increased considerably.

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[1] The substance of this paper was read to the British Psychological Society on 12 March, 1921.

[2] Cf. Baldwin's *Dictionary of Philosophy and Psychology.*

We may first inquire whether such skill can be justifiably described as intellectual.

Bodily skill, or the ability to deal with the world by means of one's muscles, joints and tendons, carries with it a specific and unique kinaesthetic knowledge. This knowledge can obviously be called intellectual in the first sense mentioned above. Yet it is as incapable of perfect translation into the terms of another sense as is music into colours or words, for as we shall see, it possesses scarcely any words in its own right. Many persons therefore require to make a mental effort in order to realise that, in spite of its comparative wordlessness, kinaesthetic knowledge may form a basis for conceptual thinking, thereby establishing a primary claim to be called intellectual in the second sense of the term. The peculiar nature of such knowledge, however, raises certain difficulties when it is dealt with from the psychological aspect. These will be described later.

Kinaesthesis is usually accorded scant respect by the world of intellectuals, as we know them today. Classed with the 'lower' senses, it is contrasted with the 'higher' senses of sight and hearing[1]. Though such a division seems justified at present its utility is possibly over-estimated[2].

II. SOME PROBLEMS STATED.

It is now possible to suggest a number of questions which arise from these considerations. Some attempt at answering them will be made in this paper.

(1) Do not most educated persons regard the intellectual value of kinaesthesis as slight?

(2) Is such an attitude justified, in the light (*a*) of primitive man's early development[3], (*b*) of certain present tendencies in social behaviour?

(3) What are the chief factors which have brought about this attitude?

(4) Is the discovery of these factors likely to help in predicting future developments in man's mentality?

(5) Is an intellect, the foundations of which do not include contributions from the 'higher' senses of sight and hearing, necessarily inferior

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[1] Cf. E. B. Titchener, *Textbook of Psychology*, New York, 1911, 114 f.

[2] Perhaps Professor Nunn feels this when he writes kindly of the 'humbler' senses. Quoting Miss Margaret McMillan, who says "The patience of the poor is not all patience. It is largely insensibility," he adds, "To such children a shower-bath, with its powerful appeal to dull senses and flaccid muscles, may mean a veritable beginning of intellectual and moral enlightenment" (*Education, its Data and First Principles*, London, 1920, 164).

[3] The chapters on "Tactile and Motor Impressions," in F. Wood-Jones's *Arboreal Man* (London, 1918, 157–173), are full of suggestions upon this subject.

to that of the normal person? If so, in what respects? May it on the other hand, even be supernormal in some directions?

(6) Would it be possible to increase the present intellectual value of kinaesthesis by extending man's conscious appreciation of the different modalities and qualities of the group of sensations which compose it?

III. SOME CHARACTERISTICS OF KINAESTHESIS.

As is well known, when muscles contract, the sensory nerve endings in both the muscles and the tendons are excited. When parts of a limb move, the joints are stimulated. The sensations from these various organs, together with those arising from the different tensions of the skin surface involved, fuse to form in consciousness a whole which introspection alone is almost powerless to analyse, but which partially breaks up in certain pathological conditions, allowing the respective rôles of these different factors to be studied separately[1].

It seems unnecessary to remind psychologically-trained readers of the importance of distinguishing the subjective from the objective aspect of motor activity. Yet the difference is often forgotten. Evidently Professor Knight Dunlap considers it advisable to emphasize the necessity of remembering the distinction:

> When I speak of 'muscular sensation' I mean the *peculiar aspect of the actual muscle-contraction which is perceived by the owner of the muscle, and by him alone*[2]. The contraction has visible aspects, and tangible aspects, which may be perceived by several people: in addition it has this 'kinaesthetic' aspect[3] which can be perceived by one person only[4].

This reminder is timely, for our thinking runs a risk of being confused by the stress which is usually laid upon what motor activity would *look like* to an external observer rather than what it feels like to its initiator, and upon what it *does* to external objects rather than upon what it *is*. Its physiological aspects and its behaviour-effects are subjects for consideration quite distinguishable from its subjective or strictly psychological aspects.

(*a*) *The loose connexion of kinaesthesis with language.*

In the following pages no attempt will be made to discuss the old and important question, whether kinaesthetic *images* really exist, or if all so-called images of kinaesthesis are in fact actual, though faint, sensations[1]. For the sake of convenience, however, and for no other reason, the words 'image' and 'imagery' will be used in the ordinary way.

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[1] A detailed account of the kinaesthetic senses is given by Titchener, *op. cit.* 160–182.

[2] Italics mine.

[3] Not only the muscles, of course, possess this kinaesthetic aspect.

[4] *Psychobiology*, 1920, II, 1, 33–4.

Man's memory of his own movements as they felt to him at some earlier time is a subject of which comparatively little study has been made. To speculate why might lead to interminable discussion of a host of possible causes. Prominent among them, however, is the difficulty of finding comprehensive and comprehensible ready-made names by which these experiences could be described to others. Almost as impressive is the fact of their faintness, vagueness and comparative unimportance in the lives of many leaders of thought, whose intellect has often been erected almost exclusively upon foundations provided by those aristocrats of sense, sight and hearing. Yet it is not a biological necessity that man should be endowed with these two senses. One can conceive him as existing without them, and, in consequence, attributing more importance and paying greater respect to kinaesthesis[2]; even as she who writes:

> The delicate tremble of a butterfly's wings in my hands, the soft petals of violets curling in the cool folds of their leaves or lifting sweetly out of the meadow-grass, the clear, firm outline of face and limb, the smooth arch of the horse's neck and the velvety touch of his nose—all these and a thousand resultant combinations, which take shape in my mind, constitute my world[3].

And the chief denizens of this world must inevitably be the experiences of touch and kinaesthesis, for Helen Keller never heard a sound in her life, and the vision which illuminated her early days flickered out in a year, leaving but a few faint gleams in her memory. Yet, she tells us:

> With the dropping of a little word from another's hand into mine, a slight flutter of the fingers, began the intelligence, the joy, the fullness of my life.
>
> My world is built up of touch sensations, devoid of physical colour and sound, but without colour and sound it breathes and throbs with life. Every object is associated in my mind with tactual qualities which, combined in countless ways, give me a sense of power, of beauty, or of incongruity, for *with my hands I can feel the comic as well as the beautiful in the outward appearance of things.* Remember that you, dependent on your sight, do not realise how many things are tangible...."
>
> The hardness of the rock is to the hardness of wood what a man's deep bass is to a woman's voice when it is low. What I call beauty I find in certain combinations of all these qualities, and is largely derived from the flow of curved and straight lines which is over all things[4].

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[1] For a modern discussion of this subject, the reader may be referred to M. F. Washburn, *Movement and Mental Imagery*, New York, 1916, 48–61.

[2] Throughout the paper the simultaneous occurrence of cutaneous sensation with kinaesthesis is assumed.

[3] Helen Keller, *The World I Live In*, p. 6.

[4] *Ibid.* pp. 5 and 7.

The ludicrous, too, is felt by her in "the bulge of water-melons, the puffed-up rotundities of squashes." But her amazing insight—the word is not permissible here, but we have no other—into realms which seem to many to be explorable only through sight and hearing, may easily distract attention from an important consideration. Helen Keller was educated by and obtained her language from persons whose knowledge of their own world had been received chiefly through the channels of sight and hearing. It seems clear, therefore, that the terms of her language, while often merely analogical, are just as often almost meaningless if regarded as descriptions of fact. The words 'ruddy' and 'dissonance'; the statement "I know how budding trees look" can have no direct sensous meaning for her[1].

Perhaps we may be allowed—though it seems scarcely permissible—for one brief moment to adopt towards this life-history the detached attitude of the scientist. At once a speculation forces itself forward. Only by the devoted efforts of her teacher and friends was her mental development made possible; yet to what extent did this assistance cause her mind to deviate from the path which we may imagine that it might have taken for itself, under impossibly ideal circumstances of protection and companionship?

Let us suppose for a moment that, while being sheltered from the rigours of life, she had developed ideas both simple and complex about her world chiefly upon the basis of kinaesthesis, and with no reference whatever to sight or hearing. We may now further imagine her, having constructed a mental world quite different in quality from that of the ordinary person, to have been entrusted completely and exclusively with the bringing-up of another child, also lacking vision and hearing, who, in turn, had become the educator of still another. Gradually these references to sight and hearing, as analogical as our own attempts to picture the waves of wireless telegraphy, would have faded away, leaving a structure of knowledge which would be the unique possession of the congenitally blind-deaf; unique not only in the nature of its fundamental sensation material, but also in the degree of perfection attained by its functions. Moreover, in many of those adjustments to environment in which these attainments entered into competition with those of any ordinary human being, they would have proved to be superior.

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[1] "....a venturesome spirit impels me to use words of sight and sound whose meaning I can guess only from analogy and fancy" (p. 45).

(*b*) *The significance of individual differences in kinaesthetic imagery.*

The evidence derived both from the comparison of the introspection of different individuals, and from experiment, makes it clear that the kinaesthetic imagery of different persons is characterised by very considerable variations. Some people appear to possess kinaesthetic imagery of a degree of excellence comparable with the common cases of well-developed visual memory. Yet up to the present there have been few to collect the details of such gifts. The earlier *questionnaires* gave their answerers comparatively little opportunity to record this kind of imagery. Even if they had done so, the 'motile'—to use a somewhat old-fashioned phrase—would have found it difficult to express himself in words, except of course when describing the processes of speech or writing. Important as these inquiries undoubtedly are, they teach us almost nothing concerning the mental life of the 'muscular man,' and much more investigation is needed into the kinaesthesis of the larger musculature and of the body as a whole.

Until more is known about the characteristic individual differences in kinaesthetic memory, we must content ourselves with speculations based upon other spheres of imagery, aided by our casual self-knowledge. With this warning in mind, the somewhat bold speculation may be made, that kinaesthetic memories may differ in their vividness, clearness, 'constancy,' or unchangeableness, readiness for use and ability to carry meaning.

The task of assessing all these characteristics would not be easy. It is obviously more difficult than in the spheres of sight or hearing. For the person who describes visual imagery may command all the subtle shades of significance with which centuries of language and literature have provided him; for the recorder of auditory memory the whole notation of music and the rich resources of physics are ready to help him to hammer out as thinly as possible, and then to nail down, the edges of his meanings. But where is the notation of action; where shall we seek for the grammar, the syntax and the theory of harmony of bodily movement? Perhaps some beginnings of them lie in the basal ideas, many of them as yet scarcely explicit, of manual training, of eurhythmics, of folk-dancing and of 'motion study.' Certainly their material lies tantalisingly hidden in the minds and bodies of Anna Pavlova, of Annette Kellermann, of C. B. Fry and W. T. Tilden. But whether these happy mortals, when they move, are making poetry without knowing it; how far their perfection is the result of hard-won synthesis and to what extent it is a delicate natural polish which self-analysis would crack; of these things we—and possibly they—know almost nothing.

But while we marvel delightedly at the grace of their movements, perhaps feeling in ourselves the while feeble and faint tendencies to copy them, the sight suggests another psychological question. May the pre-eminence of these persons be closely connected with their ability, on seeing a new movement, to register and subsequently to recall it directly through their kinaesthesis, and not through the aid of any intermediate imagery, like that of sight? May it be that the number, vividness, clarity, readiness and meaning-carrying capacity of their kinaesthetic images enable them to learn a new movement in swimming, dancing, fencing or skating as easily as a first-class visualiser may take in details of a picture after one brief glimpse, or as Mozart wrote down from memory the *Miserere* of the Sistine Chapel after hearing it twice?

This seems to be possible, for before a new co-ordination of movements can be learnt by most ordinary people they find it necessary to overcome something very like a pronounced physical and mental resistance against placing their limbs or body in this particular position. They feel as if, in order that the new movement shall be learnt, it were necessary to blast a path through some stratum of high resistance in the neuromuscular system. Often, moreover, once this newly taken-up bodily position has been abandoned, many unfortunates can evoke no genuinely kinaesthetic memory of it, and so, in order for it to be learnt, it has to be assumed a wearying number of times. As a stepping-stone to these new achievements they resort to visual or verbal aids, or use both together; they picture the arm or leg in a certain position before putting it there, or they anxiously mutter, quietly or audibly, some exhortation given by their teacher, "follow through," "watch the side-line," "bend the knee," "right shoulder forward."

It is almost certain, however, that at least some persons, endowed with well-marked kinaesthesis, remember their new movement from the very beginning in the same 'language' in which they will eventually wish to express it, in the wordless language of kinaesthesis. And in doing so they ensure their greater success in more than one way. Obviously they save their own and the instructor's time. But loss of time is not the only annoying feature of a *bureau de change*; the value of the foreign currency with which one emerges is always less than that paid in, by the amount charged for changing. And this depreciation of meaning, when knowledge gained in one sense-sphere is translated into the language of another—so vividly realised when one reads the 'meaning' of a great symphony which a concert-programme description vainly attempts to convey, or hears the musical 'representation' of a sea-scene—is at least as great in the case before us.

In order to illustrate these and other facts, some examples from my own experience may perhaps be admissible. By way of preface, I will mention that the variety, range, readiness and plasticity of my visual imagery seem to be inversely matched by the paucity, narrowness and 'stickiness'[1] of my kinaesthetic equipment[2]. The following observations are taken from my notebook, with a very few minor corrections of obscurities due to the clumsy wording of notes written hastily:

> At noon to-day I finished an elementary lesson in figure-skating, during which I had found considerable difficulty in getting the 'feel' of the various muscular coördinations—chiefly concerned with feet, knees and hips—as described and exemplified by the instructor. At 6.30 the same evening, while writing these notes, I tried to recall, in kinaesthetic imagery, the morning's lesson on the 'back outside edge.' In spite of all my efforts, the recall persistently came in the following terms:
>
> A very clear visual image of the instructor, seen facing me at the distance of a foot. The clearest part of the image was the top of his head, the next clearest his face and his smile, and the upper part of his body. The image pushed itself into consciousness almost to the exclusion of everything else, and the utter inability of such an experience to assist, in any way, a person learning to skate is too obvious to require further emphasis.
>
> With an effort on my part there then came another visual image of the instructor's skating-boot executing a back outside edge. This boot, if not firmly held in check, began to focus itself into details such as the pattern of lacing, scratches, and an occasional patch on the side. At no time did even a visual image of my own boot come into consciousness, probably for the very good reason that I had been forbidden by the instructor to look at it.
>
> The only kinaesthetic imagery which could be recovered at this time (i.e. 6½ hours after the lesson) was a very faint and feeble image of scratching the ice with the toe of the skate; i.e. an image of a movement which I had been expressly forbidden to make.
>
> At another time it was recorded that "any number of visual images of the instructor in any position can be obtained and these must be re-translated mirror-wise into visual images of my own body before they are of any use."

A further extract from notes written on the same evening as those first quoted may make another point clear.

> In trying to remember a particular skating movement which involves keeping the right shoulder back, I 'see' my own shoulders much more easily than I can

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[1] This characteristic is described on pp. 171–2.

[2] How far such lopsidedness of imagery development is attributable to heredity, to environment or to both is a psychological question which often arises in a practical form. It is usually ignored or begged.

'feel' them in the right position. I visualise quite clearly my right shoulder when it is held back, including even the part of the (brown) coat which covers it, quite regardless of the anatomical impossibility of seeing my right shoulder when my eyes are looking over my left. When the instructor shows me how to execute a new movement, I am naturally inclined merely to admire him passively as a moving picture (after one evening's skating I had continual visual images of him in all kinds of skating positions before I went to sleep) but, unless I force myself to do so, I get no muscular imagery while I am watching him demonstrate a movement.

How then do I learn to skate at all? In so far as any progress is made, it appears to be[1] through visualising myself and others performing the movement in question. The images are very clear though not vivid; they are like a clear well-taken carbon photograph, or like a sketch made with an H.B. pencil, and therefore gray. When taking a lesson I tend to look on at the whole performance as a passive spectator, just as if I were watching a stage dancer, and only occasionally do I remind myself that it is my lesson, and that therefore I must do something more than merely looking on.

It is very easy for me to attend to the look of the performance; it is correspondingly hard to attend to the 'feel' of it[2]; it is as if the nervous energy had to blast its way through some resistive stratum of the brain. This illustration of blasting one's way is perhaps supported by the fact that the 'feel' of a new movement, when it does arrive, often comes quite suddenly. Perhaps Dr Montessori may be describing some similar experience when she writes of the "writing explosion"; the sudden discovery, often attended with great excitement, by her little pupils that they are able to write.

In learning a dance there is, too, the same tendency to attend to the look and not to the 'feel' of it. In order to learn any new dance I must go behind the demonstrator and see the position of the person's feet, just as if they were my own. I must then place my feet in the same position (imaged visually if the demonstrator is absent) attending very carefully to the 'feel' of the position.

Lastly, I might mention that though my muscular memory for movement is so poor, vague, unready and 'sticky,' so to speak, my sense of balance appears to be normal; e.g. I have noticed that keeping my balance in a new position, or deliberately falling from a new position, presents few difficulties to me.

I should not be surprised to discover that the apparent paucity of my kinaesthetic images is due, to a considerable extent, to their 'stickiness' or general agglutination, by which words I mean to indicate:

(*a*) The lack of clear-cut differentiation between different combinations of imaged movements, especially if they are somewhat similar. It seems to me, as a visualiser, comparable to the lack of differentiation which would exist in the visual imagery of an uneducated person, attempting to recall, let us say, a series of differently shaped human skulls which he had seen; while in the imagery of a visualising anthropologist these distinctions would be prominent. I imagine that in a first class athlete, especially in one who plays well several somewhat similar games, e.g. tennis, lawn tennis and badminton, this differentiation (which of course, is one of the first necessities for thinking) has gone on to a much greater extent than in myself. Perhaps it is comparable to a good actor's power of speaking the same sentence successively in several different dialects of the same language.

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[1] (Note added subsequently): "Or rather, 'one important factor appears to be.'"

[2] This reminds one of the fact that, on injury to the post-Rolandic convolutions of the brain, a patient may lose his power of attending to his touch sensations while retaining that of attending to everything else. Might it be that in the case of the person whose kinaesthetic imagery is undeveloped some corresponding part of the brain has never been properly 'opened up'? (Cf. footnote [2] on page 172).

(*b*) The fact that my kinaesthetic images seem—if it is not unsafe to use a pictorial metaphor—difficult to coax out of their holes. Perhaps it is that they are not only faint, but extremely fleeting (like the visual images which Dr Rivers describes as forming part of his experience when awake[1]), and that only after many efforts can one get a grip on them.

These examples[2] may help the reader whose imagery tends predominantly towards the visual or the kinaesthetic type to realise the great difference between the kinds of mental apparatus with which different persons may attempt the same task. The utter irrelevance and futility of many of the visual images recorded in the notes are worthy of special attention. Some of these images were not only useless but their presence may have obstructed any incipient attempts to learn in the direct way. For example, visualising the upper part of the instructor's body seems scarcely likely to have assisted in any way whatever; visualising one's own skating-boot may have been a little more helpful, though this is doubtful.

It is, however, impossible to be satisfied with any simple belief that visualisation is valueless in learning muscular coordinations. Many visiles cherish those diagrams and pictures which, though they are sometimes execrable, both in conception and in execution, often form almost the only intelligible information in many books which profess to describe how to play any particular game. After a glance at such illustrations the visile may carry away, once and for all, the gist of a wordy explanation which may never have been clear even to its writer.

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[1] *Instinct and the Unconscious*, Cambridge, 1920, p. 11 f.

[2] When I wrote these notes, I was unacquainted with Professor Washburn's book, *Movement and Mental Imagery*. In connexion with my own ease of attending to the 'look,' and difficulty in attending to the 'feel' of a muscular performance, her discussion of the relation of differences in imagery-type to "individual differences in the appeal of certain kinds of stimuli to attention" (p. 43 f.) is helpful, while Professor Wood-Jones's statement (*op. cit.* p. 171): "It is perhaps not beyond possibility that the full lodgment of all pictured movements (his term for kinaesthesis) is not yet permanently effected in all human brains, and that the process is still in progress" is hopeful.

IV. THE 'INTOLERANCE' OF PERSONS WITH PREDOMINANT KINDS OF IMAGERY. THE UTILITY OF VISUAL IMAGERY IN LEARNING MUSCULAR COORDINATIONS.

It is very common to meet with athletes who maintain stoutly that books are of no use in learning a game. If by this be meant merely that books are useless if not supplemented by diligent practice, the remark is so obvious as to be uninteresting. If it means that many existing books on games are bad, the psychologist's interest in it becomes a little keener. But in the mouths of some people the assertion illustrates a most interesting and important psychological phenomenon; the mutual intolerance, arising from ignorance, of people with different types of mentality.

This fact was not missed by Galton, who writes of those men of science to whom visual imagery was unknown:

> They had no more notion of its nature than a colour-blind man, who has not discerned his defect, has of the nature of colour. They had a mental deficiency of which they were unaware, and naturally enough supposed that those who affirmed they possessed it, were romancing[1].

This attitude is often very strikingly shown (though its results and implications are, probably, not yet sufficiently realised by psychologists and others) by visiles towards motiles, and *vice versâ*. The visile often finds it almost impossible to realise how a motile can ever recall in any terms other than visual images an experience which, it would seem to the visile, 'ought' to be naturally recalled in such imagery. The motile has similar difficulties of comprehension. (It is more correct to write that these difficulties would arise in the improbable event of such thoughts occurring to either of these persons.)

For those readers whose predominant imagery does not happen to belong so exclusively to one of these two classes, a few examples may illustrate the importance of this intolerance. Perhaps I may be allowed to begin with the type to which my own mind conforms; the visile. For me visualisation, not only of obviously pictorial experiences, but even of abstract and general meanings, in the form of similes, metaphors, analogies and semi-diagrammatic pictures[2], is so ubiquitous that I frequently forget that many other people do not visualise with the same degree of facility or viciousness. It is unfortunately a somewhat common experience for me to realise, after having lectured to a class for some considerable time, that I have made no attempt to transfer to the blackboard any of the mental diagrams which I have been using. Such outward behaviour might perhaps be interpreted by others as indicating poverty rather than profusion of visual imagery in a lecturer's mind. On the contrary, however, visual imagery of diagrams is so prevalent with me that I have often to remind myself that in order to transmit even an approximately faithful rendering of my meaning to others, I must make my thinking 'visible.'

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[1] *Inquiries into Human Faculty*, London, 1883, p. 85.

[2] There is, usually no *conscious* attempt to picture such meanings. They simply 'are there.' Sometimes they are adequate to the meaning, often they are mere caricatures of it. But like most caricatures they usually cause me to realise with especial intensity some salient point, and not always one which is welcomed.

Many visualisers too, never realise, or refuse to believe that complicated behaviour and thinking may go on in others without the slightest trace of visual imagery. From conversation with some of my friends who have predominant kinaesthetic imagery it seems to me certain that a similar forgetfulness of the peculiarities of others characterises many motiles, especially those for whom kinaesthetic processes,—in particular, perhaps, those of language and gesture—seem to be so prominent in their thinking that they even write of the "fiction of visual imagery[1]." In fact, the possibility of any real *rapprochement* of the visile and motile seems at present to be remote, for the following reason. While the visualiser's power to express to another person his experience of visual imagery is limited only by his capacity for psychological observation and for using his mother tongue, which has chiefly been developed to describe the seen world, the motile is less fortunate, in that very few of his characteristic inner experiences[2] are expressible in language at all[3]. For the function of many words relating to bodily movement is merely to describe its visible aspects, seen by others or by its initiator. Some words, moreover, refer less to the bodily movement itself than to that of some instrument moved by the person, or, even more remotely, to some object propelled by this instrument. English out-door games illustrate especially well this tendency for the reference of the term to creep farther and farther away from the initiating limbs. For instance, I take it that such a term as a 'late cut' in cricket may conceivably refer to any or all of the following separate events:

(*a*) the genuinely personal experience; kinaesthetic, visual, tactual, etc., of the batsman;

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[1] The question is discussed in greater detail in this *Journal*, 1920, XI, 1, 77–79.

[2] I venture to call the reader's attention once more to the significance of this word 'inner,' which was emphasised on p. 165.

[3] Or, as a behaviourist might perhaps express it, speech habits are not closely connected with the functioning of the muscles, joints and tendons.

(*b*) the movement of the body and arms and of the bat, before and after it strikes the ball (1) as seen by the batsman, (2) as seen by the spectators;

(*c*) the behaviour of the ball after its impact with the bat.

An indispensable preliminary to the construction of any satisfactory psychology and any really expressive language of kinaesthesis will be a distinct separation in thought of all those events which occur outside the agent's skin from those which take place inside this envelope. Until that is achieved the motile will remain, as he is at present, inarticulate. For of the recognised phrases describing action many bear a spectacular or behaviouristic rather than a psychological meaning, describing the experience of the looker-on, not of the performer, while not a few carry a depressingly complex and unanalysed mixture of both.

It is probably the ability of the motile to recall a new movement 'in his muscles,' after having performed it but a few times, and connected with this, his inability or disinclination to visualise how his body looks when placed in the position required, which leads him to say that reading books on a game is of little use in learning it. If by this is meant that one cannot learn a game without performing the muscular actions which it requires, it is a platitude. But if, on the other hand, the motile means that for him photographs, diagrams, and diagrammatic pictures, to say nothing of the slowed-up cinematographic representation, and the tri-dimensional wire model of a movement, are of little or no use to him, he expresses very clearly his difference from the visile. For from a really intelligently drawn diagram the latter can often learn in a flash what he has been trying for weeks to discover from watching an actual demonstration. Eventually, of course, he must translate this visual experience into kinaesthetic memories of the movements as they were actually carried out by himself. In this respect therefore, he is at a disadvantage; comparable, perhaps, with that of a person who knows a language but cannot think in it. But whether in the highest flights of any branch of games, the use of a good book does not become essential for rapid progress is still a question well worth asking.

V. IS A 'LANGUAGE OF KINAESTHESIS' POSSIBLE?

We must now inquire whether such a language of kinaesthesis is conceivable, and if so, how it might arise. An attempt will be made to sketch an approach to this problem, and to speculate how, neglecting for the moment practical difficulties, such a language might conceivably be formed.

To the utmost possible degree it should be rid of analogical references to other senses. Sight has one language, hearing has another. Cross-references, such as the colour of a voice or the key of a landscape are, statistically speaking, so rare that they do but call attention to the normal self-sufficiency of these two notations. Kinaesthesis, then, would have to renounce its present bad habit of borrowing terms from the other senses, and botching make-shift tools out of them. Ideally, a visual or auditory term, used in a description of motor experience, would appear unambiguously in its own right.

Supposing it to be possible to furnish descriptive words for different kinaesthetic experiences, it would then be necessary to discover elementary or *quasi*-elementary movements of groups of muscles which are of fundamental and outstanding importance in some branch or branches of industry or physical exercise. Here one can offer only tentative suggestions as to how this might be done. Search might be made amongst existing manuals of drill and physical training, of eurhythmic exercises and of physical culture systems[1]. Anatomists and physiologists might give valuable information concerning those important bodily movements which, structurally and functionally, are relatively simple. In this way there might be isolated from others, so that they could be fairly strictly defined, a hundred simple, elementary and typical attitudes of body and limbs. Persons could then be taught to take up these positions again and again, carefully and attentively, until the descriptive names given to them had associated themselves with definite kinaesthetic experiences.

Such a plan does not seem wildly impossible. In fencing, golfing, cricket, figure-skating—to take only a few cases—definite 'stances' are learnt. In the last-named exercise, 'new' positions and movements appear to be deliberately composed of definite elementary groups which have been previously given standardised names and consciously realised in experience. All such lessons involve the isolation, from thousands of possible muscular combinations, of some, which for a particular purpose, are 'fundamental.' Perhaps some day, with the assistance of anatomy and physiology, it may be possible to define more strictly the fundamental groups for any special purpose.

Supposing, then, that the important elementary or essential experiences in the world of kinaesthesis had been isolated and labelled, so that agreement exists concerning their meaning; the next step would be to classify them more scientifically—for of course some degree of classification will have taken place already—into categories. Just as in the language of sight there are divisions of words into those representing colours, forms, directions, sizes, etc., so there might be a subjective or psychological classification of man's most important movements. While gross divisions of the movements, corresponding to limbs, fingers, etc., will already have been achieved, it is obvious that some compromise between such a classification and that based upon anatomy and physiology may be necessary. The most useful final grouping might conceivably be guided exclusively by anatomical and physiological considerations.

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[1] The writer's present acquaintance with the Ling and other systems is insufficient to justify any opinion being recorded here concerning the extent to which a scientifically satisfactory beginning has been made in this direction.

At this point the reader will naturally and justly inquire why, if such a badly-needed kinaesthetic language is theoretically possible, it is not already in existence. He may draw the conclusion that there must be some insuperable difficulty which this essay has not taken into account. This may be the case. But, until recently, one valid reason for the non-existence of any satisfactory objective analysis, classification, and standardisation of human movements (and, therefore, of human kinaesthetic experiences) was that usually no two persons could agree not only as to the exact motions which ought to be made for any particular purpose, but even concerning the movements which they themselves had made a moment previously[1]. For most human movements are so very complicated, rapid and individually different that even an unusually favourable combination of natural aptitude and special training in observation cannot ensure a faithful account of them.

VI. THE IMPORTANCE OF 'MOTION-STUDY' IN THIS CONNEXION.

But the last few years have seen a great increase in the number and variety of successful attempts experimentally to record the behaviour both of man and of animals. One of the most interesting branches of this work is that known as 'motion-study.' It has developed, and is still improving, apparatus which, freezing in mid-air, so to speak, the path of any movement, records it permanently by means of a photograph or tri-dimensional wire model, painted in a special way to represent not only the direction of any component movement but also its relative and absolute velocity[2]. Such models will make it impossible in future for there to be two opinions concerning the exact orbit which any limb has described in executing a set of movements.

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[1] Disputes as to whether a particular golfer moved his head at a certain moment are not unknown in club houses. Unfortunately it is quite likely that the person whose evidence is of least value in this connexion is the player himself.

[2] For description and photographs see F. B. Gilbreth, *Motion Study*, New York, 1911; *Fatigue Study*, London, 1916; *Applied Motion Study*, London, 1919. A concise account of motion-study, with illustrations, is given in Chapter I of C. S. Myers's *Mind and Work*, 1920, pp. 2–35.

It seems therefore not inconceivable that as a result of careful motion-study of various occupations there may be built up notations both of objectively seen movements, and of the kinaesthesis corresponding to them. The words used would then refer (*a*) to the objectively demonstrated movements—photographed, cinematographed or 'motion-modelled,' (*b*) to the kinaesthesis experienced when these movements, and these movements only, are made.

The advantages of such an objectively 'checked' language are clear. A name given to a kinaesthetic experience would relate to the 'inner' feeling of some movement, set of movements or attitude the nature of which had been objectively demonstrated so that its meaning would be constant for everyone. Such a movement could be carried out again and again until its 'feel' became unmistakable. Related movements might be given related names. Movements common to several occupations might be analysed out and their relations to these different functions studied, as Mr Gilbreth has suggested. In the hands of skilled and educated teachers, the supplementary teaching of movement by pictures, diagrams, models, and the slowed-up cinematograph might then become really effective. Visual 'representations' of movement, obtained by the use of the apparatus of motion-study, would really represent and not caricature the motions which they profess to depict. This would inevitably lead to the detection of the less desirable methods of carrying out any movement, and therefore, again, to increased skill, or kinaesthetic knowledge. Not only would the status of such knowledge be raised, but also the status of those people who, through their teaching, aid in its acquisition and development.

VII. IMPROVEMENT OF THE SOCIAL AND INTELLECTUAL STATUS OF KINAESTHETIC KNOWLEDGE.

In a psychological discussion of this subject it is obviously relevant to comment upon the present disparagement, by an influential section of the community, of kinaesthetic knowledge. The tradition that knowledge worth having is almost exclusively confined to that which has reached us through our eyes and ears has been confirmed and hardened by the powerful mechanisms of class distinction and class tradition, and by generations of a certain type of teacher in school and University. To such influences the ordinary man owes his view of culture. From most people, therefore, the motiles, as compared with their socially-established brothers, the visiles and audiles, seldom get fair play. The two latter groups, themselves recognised as belonging to a kind of *intelligentsia*, not infrequently fall into the bad habit of regarding with some contempt all persons whose motor activity is expressed through channels other than the socially-approved ones of speech and writing[1]. A vicious circle is thus made; many intelligent persons are never encouraged to contemplate the study of such non-verbal occupations, and not a few are actively prevented from taking them up. This is one reason why the intelligent professional in sport, the intelligent teacher of handwork, and the intelligent teacher of games are still so much of a rarity that when they do appear one never fails to remember them. If the knowledge obtainable through kinaesthesis were increased and transmitted to others in the best and quickest ways by teachers who were themselves good performers, their social status would rise. But this is the less important consideration; their intellectual status would be improved too. By increasing their kinaesthetic experience, they would not only have gained deeper knowledge of a comparatively new aspect of their world, but such knowledge might form basic material for the elaboration into concepts which characterises intellect in the second sense of the term; the process by which we acquire the knowledge of truths as distinguished from the knowledge of facts.

I have attempted elsewhere[2] a crude analysis of the process of thinking. Its essence seems to be the recall of past experiences, abstraction of their relevant aspects, their comparison, re-comparison with some aim in view, combination of the results of this comparison into a 'new' conclusion, and the expression of this conclusion in action, gesture, speech or writing. All this happens so regularly when its raw material is formed by visual or auditory experiences that many people are apt to forget that this series of processes may be carried out just as successfully upon the basis of touch and kinaesthetic memory[3]. Few would have the hardihood to object that in such a case the processes were not intellectual. If they did, Helen Keller, with her university degree and her literary achievements, would provide the answer.

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[1] Mr Bertrand Russell has placed on record his suspicion that what people should mean by intellect is simply "certain habits in the use of words," and his lack of "mystical reverence for these habits" (*Mind*, XXIX, N.S. No. 116).

[2] This *Journal*, 1920, XI, 71–4.

[3] May it be that really superlative excellence in games or manipulative labour is distinguished from mere 'goodness' by a much greater degree of development of the process of working up the raw kinaesthetic material into new combinations; a particularly good example of 'kinaesthetic intellect'?

VIII. SOME FURTHER SPECULATIONS.

The relation of these considerations to the doctrine of *Bewusstseinslagen* or "conscious attitudes" is obvious and interesting. It seems certain that the thinking which goes on in the possessors of a rich and pliant kinaesthetic memory tends to be strongly influenced by such conscious attitudes, perhaps to the discouragement of visual and auditory images. How far the left wing of behaviourism is constituted by psychologists with this type of mentality, whether, consciously or unconsciously, they contemn the visual-auditory intellect in much the same way that many intellectuals contemn the kinaesthetic variety; to what extent and in what ways these different kinds of mental apparatus may assist in the formation of those different attitudes towards life which are called introvert and extrovert respectively; the rôles which different kinds of imagery play in aesthetic appreciation, in the causation and the cure of mental disorders,—all these problems are related to this subject.

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A NEW METHOD OF MARKING GROUP TESTS.

By CHARLES FOX.

The distinguishing feature of Mental Tests such as Binet's is constituted by the fact that the tests are graduated by being tried on normal unselected persons. Such tests have been widely used to discover the mental age of school children, but are not aimed at discovering the existence of any special ability. As their purpose is primarily to assign a 'mental age' they are too crude to be used for discriminating grades of excellence within any mental age, and any attempt to use them in this way is due to a misconception of their origin. The main object of this paper is to suggest a new method of detecting differences within any group of persons.

It has recently become the custom to hold an annual examination of the children in elementary schools in various counties and boroughs in order to pick out those who are fitted to receive a higher education. The examinations usually consist of the ordinary school subjects such as English, Composition and Arithmetic and attempts have been made to devise appropriate 'tests' to take the place of, or to supplement such examinations. In an ordinary borough the numbers of children dealt with are too large to enable tests to be given individually by the examiners. It must be admitted at once that any 'mental test' which is not given individually loses a good deal of its discriminative value.

The Cambridge Borough consented this year to include a mental test as part of its examination in order to try the value of this procedure, but the candidates for higher education were selected in the ordinary way by examination. All the children in the elementary schools of the Borough, without exception, over the age of eleven and under twelve on September 1st, sat for the examination in the preceding June; the subjects being Arithmetic, Reading, Composition and Dictation. There were 336 boys and 303 girls of this age. At the same time a 'mental test' was given to the candidates. Only 15 minutes could be allowed for this test and it was necessary therefore to devise some Group Test which appeared likely to yield satisfactory results in such a limited time. It was decided to make use of Burt's reasoning tests[1] but to mark them on a new plan. The tests were selected from those which Burt assigns to the ages 7 to 13 years using only the more thoroughly revised ones and making certain minor verbal alterations which experience had shown to be necessary. A preliminary trial on selected children of the age range 11 to 12 yielded an estimate of the amount that could be attempted by brighter children in the time allowed.

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[1] *J. of Exp. Ped.* June and Dec. 1919.

The ten following questions were chosen:

1. I have bought the following Christmas presents: a pipe, a blouse, some music, a box of cigarettes, a bracelet, a toy engine, a bat, a book, a doll, a walking stick, and an umbrella. My brother is eighteen; he does not smoke, nor play cricket, nor play any instrument such as the piano. I want to give the walking-stick to my father and the umbrella to my mother.

Which of the above shall I give to my brother?

2. I don't like sea voyages, and I don't like the seaside. I must spend Easter either in France, or among the Scottish hills, or on the South coast.

Which shall it be?

3. Three boys are sitting in a row. Harry is to the left of Willie; George is to the left of Harry.

Which boy is in the middle?

4. Violet has caught an illness. If she has a rash it is chicken-pox, or measles, or scarlet fever. If she has been ailing with a cold or cough she will develop whooping-cough, or measles, or mumps. She has been sneezing and coughing for some days, and now spots are appearing on her face and arms.

What do you think is the matter with Violet?

5. Father has just come home in a brand new overcoat; there is clay on his boots and flour on his hat. The only places he can have been to are Northgate, Southgate, Westgate or the City. He has not had time to go to more than one of these. There is no clay anywhere in the streets except where the pavement is up for repair. There are tailor shops only in Southgate, Westgate, and the City. There are flour mills only in Northgate, Westgate, and the City. I know the roads are not being repaired in the City, though they may be in the other places.

Where has father been?

6. Where the climate is hot, gum-trees and rubber will grow; heather and grass will only grow where it is cold. Heather and rubber require plenty of moisture; grass and gum-trees will grow only in fairly dry regions. Near the River Amazon it is very hot and very damp.

Which of the above grows there?

7. In a school a third of the children play football and a third play cricket.

(1) Are there any who play neither football nor cricket?

(2) Can you tell without any more information whether there are any who play both?

8. Field mice devour the honey stored by the humble-bees; and honey, which they store, is the chief food of the humble-bees. Near towns there are far more cats than in the open country. Cats kill all kinds of mice.

Where, then, do you think there are most humble-bees—near towns or in the open country?

9. I started from the church and walked 100 yards; I turned to the right and walked 50 yards; I turned to the right again and walked 100 yards.

How far am I from the church?

10. A pound of meat should roast for half-an-hour; two pounds of meat should roast for three-quarters of an hour; three pounds of meat should roast for one hour; eight pounds of meat should roast for two hours and a quarter; nine pounds of meat should roast for two hours and a half.

From this can you discover a simple rule by which you can tell from the weight of a joint how long it should roast?

As is well known a 'mental test' is designed to set all persons on a level with regard to acquired knowledge, and data are consequently selected which involve no special attainments but simply those which a normal individual is bound to acquire in his every-day dealings with his physical and social environment.

One thing, however, is frequently overlooked. A game can only be properly played when the players agree explicitly or tacitly to abide by the rules of the game. This agreement gives a certain 'set' of mind. In an examination this 'set' is assumed by the examiners; and in giving a mental test the experimenter takes for granted that the person will assume this attitude of 'make believe.' Now a bright child may resent this attitude and refuse to be bound by it. An incorrect answer may therefore only mean that the child has adopted some 'set' of mind which the examiner did not intend. In question 10 for instance the child who says that the length of time a joint should roast depends on "the kind of joint" is refusing either deliberately or unconsciously to adopt the attitude of mind towards the question which the examiner assumes. Again, in question 1 a child who wishes to give his brother a cricket bat though he does not play cricket may desire that his brother should learn cricket; or if he gives him a box of cigarettes though he does not smoke, he may think that his brother may wish to give the cigarettes to a friend. A candidate, again, who in question 7 says that the boys play both cricket and football and the girls neither may be thinking of the suitability of the games for the sexes. All the above replies were frequently given. The attitude of 'supposal' with reference to a given 'universe of discourse' which the question delimits is assumed to be the attitude which the candidate will take spontaneously. But this assumption is not universally valid. It sometimes happens with bright children that they regard the questions as ridiculous and the procedure as silly and so refuse to play this particular game. A group test cannot take this incalculable factor into account, except by assuming that the ability to make definite suppositions is an essential part of intelligence. With these considerations in mind the children were instructed to answer the questions in order as printed. They were told that each question had one definite answer and that there was no 'catch' in any of the questions.

It should be remembered that the object of this test was not to discover the mental age, but to pick out the brighter children of the whole group. A difficulty arises from the fact that some children work faster than others. If their work is accurate they should obviously get credit for this speed but the marking should be designed mainly to discriminate ability within the Group.

In essence Binet's principle, which has been used by all subsequent investigators, is to use a standard group to mark all other groups or individuals. The attempt is made in this article to modify the principle so that each group may assign marks to the individuals within the group. The tests should be marked for each individual in proportion to the difficulty which the group as a whole finds in dealing with the test. The children were working under the same external conditions and with the same preliminary instructions. Assuming that they also accepted the condition of 'supposal' suggested by the tests the following scheme of marking gives credit chiefly for ability to cope with the test. All the questions yield an unambiguous answer.

After the papers had been marked a table was drawn up of the number of boys and girls separately who had answered each question correctly. The table was as follows:

		1	2	3	4	5	6	7	8	9	10
No. of Candidates answering correctly	Boys	166	215	178	121	76	71	140	95	48	17
	Girls	157	181	170	154	77	90	69	67	28	9
Percentages	Boys	49 %	64	53	36	23	21	42	28	14	5
	Girls	52 %	60	56	51	25	23	23	22	9	3

The reciprocals of some function of these numbers yields a measure of the difficulty experienced by each group in answering the separate questions. A glance at the table will show that the reciprocals of the numbers themselves would give undue weight to those who were able to work more rapidly. It is therefore necessary to choose such a function as will ensure the same serial order whilst placing more stress on accuracy as compared with speed. Raising each number to some fractional index would accomplish this but the most convenient mathematical function appears to be the logarithms of the separate numbers. A consideration of the logarithmic curve shows that the further we proceed the more relatively difficult does scoring become. A candidate finds it more and more difficult to score marks in the later questions and he cannot sacrifice accuracy to speed without being penalised. It is better to work accurately than speedily but accurate rapidity yields higher marks. Hence the logarithms of the above numbers were found and the reciprocals of the logarithms were used in marking the questions. This method of weighting provides a new and convenient way of discriminating differences such as those due to sex in dealing with mental tests.

The following numbers are closely proportional to the reciprocals of the logarithms:

Question	1	2	3	4	5	6	7	8	9	10	Total
Boys	5	5	5	5	6	6	5	6	7	9	59
Girls	5	5	5	5	6	6	6	6	8	12	64

The papers were now examined a second time and the marks found in the second table were assigned to the correct answers. By this procedure each boy and girl was measured by the members of his own group and received marks for each question which varied with the difficulty found by the whole group in dealing with the question. The actual performances of the boys and girls therefore determined the marks given for each test.

It is seen that with regard to difficulty the questions fall into three groups thus 1 to 4; 5 to 8; 9 to 10; and that boys and girls responded to the first nine questions in the same degree but the girls found question 10 much more difficult than did the boys. Questions 7, 9 and 10 are questions involving mainly mathematical concepts and in these the boys scored better than the girls. Question 4 was answered correctly by relatively a greater number of girls than boys and the reason would appear to be that they were more interested in the subject matter; as their replies even when wrong showed an observation of symptoms of illness not displayed by the boys. We are justified in the belief that the ability to cope with a mental test depends on the mental 'set' or attitude towards the subject matter of the test.

The examination in school subjects was conducted by a committee of head teachers and the Local Education Authority, and was carefully marked on an agreed plan. In Dictation the boys were penalised more than the girls for mistakes, which explains the great discrepancy in the marks. The Arithmetic test consisted of 12 questions on simple rules and very simple problems to be done in 1¼ hours; the Composition consisted of an essay to be written in 1¼ hours and was marked for style only; and a headmaster and headmistress respectively heard each individual boy and girl read. Although the marks of the two sexes are not strictly comparable we may compare the standard deviations in each subject.

It will be seen from the table that the boys on the whole are more variable than the girls in the examination subjects, with the exception of reading where the difference is slight.

The Mental Tests were all marked by the writer of this article assisted by his pupils, and as each question yields an unambiguous answer the marks are strictly comparable. Very little difference was found between the sexes; both the average mark and the standard deviation are practically the same. This result agrees with the work of Terman[1] who found that there was practically no difference in the Intelligence Quotients of boys and girls between the ages of 5 to 14 years.

Average marks in the Examination and Tests.

	All School Subjects (Max. 300)	Std. Dev.	Arithmetic (Max. 100)	Std. Dev.	Composition (Max. 100)	Std. Dev.	Reading (Max. 50)	Std. Dev.	Dictation (Max. 50)	Std. Dev.	Mental Tests		Std. Dev.
											Boys (59)	Girls (64)	
Boys	113·3	65·5	33·2	26·4	38·0	23·0	28·6	12·3	13·8	15·0	16·9	(28·6 %)	11·3
Girls	156·9	58·0	30·2	24·9	53·7	18·3	37·0	11·5	36·4	12·7	17·7	(27·6 %)	10·5

The candidates were selected for scholarships entirely on the examination results, no regard being paid to the results of the mental tests except in about half a dozen border line cases. All the candidates who gained more than half marks in the examination were chosen for scholarships, namely 282. We are able therefore to compare the results of the examination with the independent results of the mental tests. By selecting all the children who gained over 20 marks in the tests it was found that only 48 of these were not chosen for scholarships; in other words good scoring in the tests made it highly probable that the candidate would be chosen by the examination[2].

The Frequency-distribution of Marks in the Mental Tests.

Boys

Marks (max. 59)	0–6	7–13	14–20	21–27	28–34	35–41	42–48	49–55	Total
Candidates ...	68	73	93	44	34	10	10	4	336

Girls

Marks (max. 64)	0–6	7–13	14–20	21–27	28–34	35–41	42–48	49–55	Total
Candidates ...	47	68	79	58	28	17	5	1	303

In order to get a statistical comparison the coefficients of correlation were calculated. The coefficients show that there is a high correlation between the performance in the whole examination and the results obtained by the reasoning tests. As might be expected, the correlations are highest between the tests and the Arithmetic papers especially in the case of the boys. With regard to the girls, the coefficients of correlation diminish as we pass from Arithmetic to Composition, thence to Reading and Dictation. With the boys, however, the correlations between Composition, Reading and Dictation respectively and the reasoning tests are practically the same; being in each case much lower than the correlation with Arithmetic. The following table shows that the results are trustworthy as the probable errors are very low in every case.

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[1] *Measurement of Intelligence*, Harrap, 1919, p. 68 ff.

[2] An instructive case occurred. A boy whose total marks were only 101 was chosen by the Headmaster who heard the reading on the ground of General Intelligence. It was subsequently discovered that his marks in the Tests were very high, namely 30.

Table of Correlations.

		All 4 School Subjects	Arithmetic	Composition	Reading	Dictation
Mental Tests	Boys ...	+·62 ±·023	+·64 ±·022	+·47 ±·028	+·48 ±·028	+·46 ±·029
	Girls ...	+·57 ±·026	+·56 ±·026	+·47 ±·030	+·43 ±·030	+·38 ±·030

Burt[1] maintains that performances in school subjects depend upon 'General Educational Ability' which is a factor common to all elementary school subjects and also upon group factors of Specific Educational Abilities. The above results appear to confirm his views as regards the General Factor and to support the existence of the group factors but they tend to show that Composition should be included in Burt's linguistic group instead of being separated from Reading and Dictation as he suggests.

The writer desires to thank Mr G. Udny Yule for his generous advice in dealing with the statistical results.

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[1] *Distribution and Relations of Educational Abilities*, London, P. S. King, 1917.

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CRITICAL NOTICE.

By CHARLES FOX.

Education and World Citizenship: an Essay towards a Science of Education. By James Clerk Maxwell Garnett. Cambridge: University Press, 1921. pp. x + 515. 36*s*. net.

This is the most ambitious attempt made in recent times to construct a science of Education on the basis of scientific principles. In building the edifice the author uses the results of the most recent relevant research in experimental psychology and elaborates an epistemology of modern science. The final section of the book is a survey of the types of education and educational institutions as they might exist in England if we desired to have a complete and consistent national system of Education.

In Book I the author follows closely Professor Adams's account of the origin and growth of the doctrine of 'formal training' and rejects this doctrine completely. This theory which is better described as the doctrine of the 'transference of training' rests on the assumption that the results of education in any sphere of activity are transferable to other spheres; or as a schoolmaster once said, "It does not matter a bit what you teach a boy provided he thoroughly detests it." Apparently this master believed that the will is strengthened by constant exercise in dealing with material which the pupil would avoid if left to himself. It is curious that the author first rejects this view and in Book II quotes with approval Dr Temple's dictum that "It scarcely matters what subject is taught: the vital matter is that the child should learn 'attention' in general."

The main part of the second book is concerned with an elaborate attempt to show that the materialistic view of the structure of mind may be safely used as a working hypothesis in dealing with the problem of education. With this object physiological and psychological data are used interchangeably so as to establish the doctrine that there is a permanent record in the brain of all life's experience. Instead of the better known term 'engram' the word 'neurogram' suggested by Morton Prince is adopted for such records. Any system of connected neurograms of which one at least is a cognitive neurogram is called an interest-system, and by using McDougall's hypothesis of drainage the principle is laid down that "of all active neurograms that one which is connected with the widest and deepest interest-system tends to drain the impulses from all of them." In order to carry the argument further some useful technical terms are employed. An interest-system and especially a "future-interest-system" is assumed to be constructed in such a way that the more central elements within it exercise the greatest influence on the flow of nervous excitement. Such a system is called an 'endarchy' as it is ruled from within outwards. An endarchy which is so organised as to yield the minimum average path between any one element in the neurography and any other, and therefore gives rise to the shortest trains of reasoning within the system, is called a 'maximal endarchy.' If all sciences were complete, containing no redundant facts or essences; and if all the reasoning within the whole body of such sciences were 'elegant' in the mathematical sense of the word; and if all were united into one philosophical system, we should have the maximal endarchy of science. An omniscient being who was familiar with this system and had been trained to reason perfectly with the minimum expenditure of mental effort would have neurograms corresponding to the essences of the endarchy of science! The neurograms of such an endarchy would constitute a sort of magnified tree of Porphyry with the branches not limited to two at each bifurcation. Most people would admit with Poincaré that there is a hierarchical arrangement of facts within a particular body of science, the order of importance of which would be recognised by experts; but it is extremely doubtful whether we ought to go further with the author and assume that at some distant future time this order will assume a final form corresponding to the maximal endarchy of science. Just as that myth of Spencer's, the completely adapted man in the completely evolved society, has not aided the science of Ethics or pointed out the path to correct conduct, so the maximal endarchy of science seems equally unfitted to give guidance in education, and for the same reason. Both conceptions ultimately assume that the path of progress leads to a stage when progress will cease. No doubt in considering educational procedure it is wiser to try to coordinate the pupil's knowledge into an organic whole rather than to leave the various parts disjointed, and an ideal of complete organisation aids this process. But there are branches of education in which the procedure to be adopted is totally different from that used for cultivating organised scientific knowledge, namely those which deal with aesthetic and religious education. The attempt to teach such a subject as literature as part of the organised system of scientific knowledge resulted in those text books which contained a few pages of text and a volume of notes which the unfortunate pupil was expected to learn, and which prevented him ever afterwards from appreciating the text.

In dealing with the question of 'purpose' the inadequacy of the neurographic scheme from the standpoint of education is apparent. The crux of the discussion turns on the view that "everyone of my present purposes whatever the date of its fulfilment is represented in my brain by a neurogram." For this reason Dr Webb's Group Factor in character qualities (*w*) which is intimately related to purpose is taken to correspond with certain neurograms. This can be done only if we are willing to admit that the sole meaning which can be assigned to the idea of 'importance' is that of 'depth' of neurograms. Surely, however, the importance of an event, a fact or essence is quite distinct from the number of times it is thought about by an individual. Habitual routine usually occupies most of a man's waking life and the neurograms corresponding to it are presumably deeper than anything else which he has acquired, but that does not make routine more important than the single original idea which may occur to him once in a lifetime, and which may have a supreme importance belonging to a totally different category.

In treating of neurographies a very valuable suggestion is made, namely, that the growth of scientific knowledge enables us to organise our thoughts so as to avoid mental conflict. The reason that knowledge is organised similarly in each individual is due, apart from the influence of the social environment, to the operation of the same instinct, namely, curiosity. The author also suggests that the aesthetic pleasure yielded by the operation of organising our knowledge so as to form an endarchy is due to the satisfaction of the emotion which is united with the instinct. Thus it follows that ultimately the ground of the maximal endarchy of science is our instinctive endowment. Left to itself the instinct of curiosity would result in numerous intellectual conflicts but by the operation of Reason a solution is brought about. If our knowledge is to be true our neurography "must correspond, element for element, with the endarchy of science." Since however the structure of the neurographies has been invented to fit in with the facts of knowledge and to make the psychology of the book complete, there would not seem to be any meaning in their failing to correspond. The neurographies having been constructed to fit the psychological facts, it is not at all to be wondered at that a correspondence is found, since it has been put there from the start. This may be admitted without detracting from the value of the advice that the education of any pupil in school should be in the hands of the smallest possible number of teachers and as large a proportion of his time as possible should be with form masters as opposed to specialist teachers; for the form masters are to lay the basis of the single wide-interest-system of the pupil.

The most valuable chapter in Book II is that dealing with Will and General Ability which treats a difficult subject lucidly. A useful distinction is made between 'Will' at short range *i.e.* a single effort of will, and 'will' at long range which consists in the persistence of motives. The latter is called 'purpose' and it would clear several psychological discussions if this distinction were uniformly adhered to. The discussion of the relations between the General Factor (g) and the group factors of Dr Webb and the author is thorough and suggestive, ably summing up the work on the subject. From the educational point of view it is important to learn that whilst the 'Will' is educable the degree of its educability is probably innate.

The argument of the several chapters on conduct, character and the aim of education rests on the tacit assumption noted above that the concepts of 'importance,' 'value,' 'worth' etc. may be equated with the concept of 'depth.' The purpose neurograms are supposed to be more important because they are 'deeper' and "the influence of Purpose upon conduct is wholly accounted for by the corresponding purpose-neurograms." So "A man's thought is determined by his neurography and his Will." Consequently "his character is determined when his neurography and his Will are determinately known." In the future when (g) is measured we shall only have to investigate a man's nervous system to know him all in all! The conversion of a personal endarchy into a religion is according to the author straightforward. The neurography determines his philosophy, and united with emotional elements it is transformed into a religion. If, however, men are to live together in society there must be a harmony of their central purposes and this can only happen if each has a single maximal endarchy corresponding to the endarchy of science. The central elements in the endarchy will ensure this because they are deepest and therefore most frequently come into activity. Harmony, too, is therefore due simply to depth of neurograms.

But the most important and fruitful facts in science are not yet discovered and it would seem therefore that maximal endarchies are not attainable. The author, however, has a solution which makes it possible for him to harmonise all neurographies in a maximally progressive society and to state the aim of education for all world citizens. No society can be maximally progressive which is liable to interference from rival societies. Hence such a society must include the whole human race, and a League of Nations is necessary in order to formulate the ultimate aim of education. Before the League can get to work properly, it is essential to get a clearer view of the undiscovered central portions of the endarchy of science. Bacon long ago suggested that the world of knowledge might be mapped out and a college of research students set to work. The author however is satisfied that the Christian account of the universe adequately "completes the discovered part of the endarchy of science with a hypothesis concerning the hitherto undiscovered central essences." When science is so completed we arrive at the Kingdom of God as interpreted by Christianity. Thus the aim of Education, the world over, is to form Christian characters. It may be difficult to persuade the Japanese, Chinese and Hindu members of the League to adopt this hypothesis, but until it is done world citizenship is obstructed and the maximal endarchy of science impossible.

Book III deals with a national system of education which might be established in England in the next decade and is the most comprehensive account of types of education and of schools and colleges in the English language. It is conceived and worked out on large plan reminding one of the plan of Comenius except that it is more practical and based on existing institutions. The author's suggestions are illustrated by a 'flow diagram' drawn to scale indicating types of education by colours and varieties of schools and colleges by lines. The diagram will repay careful study, not only by students of education but by administrators, as it is eminently practical and can be adapted to different areas. All this Book is extremely important but is outside the purview of this Journal. There is one section however which should be of interest to all readers, namely a distinction between two types of education, or two distinguishable ways in which the same subject may be studied. For those who devote many years to any subject the study should be 'transitional' aiming at coherence rather than comprehensiveness, dealing with abstract conceptions and root ideas, capable therefore of growing after formal education is finished. Those whose period of education is more limited should learn in a 'terminal' way *i.e.* mainly through concrete facts and wider concepts, treating the subject from the outset comprehensively and aiming at width rather than depth. The difference may be expressed by calling the latter multifocal and the former unifocal. In other words boys who leave school at the age of 16 to enter occupations should have different courses, though they deal with the same subjects, from those studied by boys who intend to stay at school till 18 or 19 and then proceed to the University.

It remains to add that there are several useful appendices to elucidate the text.

PUBLICATIONS RECENTLY RECEIVED

An Introduction to the Psychological Problems of Industry. By FRANK WATTS. London: George Allen & Unwin Ltd. 1921. pp. 240. 12*s.* 6*d.* net.

The Psychology of Industry. By Dr JAMES DREVER. London: Methuen & Co. 1921. pp. xi + 148. 5*s.* net.

No doubt a case could be made out for the publication of both of these books. They appear in response to a popular demand. They are fairly well-informed, and although both are hastily written, are undoubtedly of considerable general interest. Dr Drever's is perhaps the better of the two, for he confines his discussions to matters about which at least some things are definitely known. More than half of Mr Watts's book deals with problems in social psychology which are hardly worth discussing at all unless they can be considered very thoroughly. "The springs of conduct" cannot be made clear in six and a half pages, nor can the psychological characterization of State socialism and syndicalism be adequately dealt with if only a couple of pages are devoted to each. Mr Watts in fact hunts in too many directions to bring down any very valuable quarry. In the case of both of these books there is exceedingly little evidence that original research has been attempted. The old illustrations, that seem to have appeared already in all the books that have ever been written on industrial psychology, come up again. Again we learn about the pig iron, the bicycle balls, the trench digging, the brick laying, and the American Army tests. It is not that these illustrations are not striking, or useful, or well worthy of notice; but only that by repetition they become wearisome, and that they are not enough to base a whole science upon.

The Science of Ourselves. By Sir BAMPFYLDE FULLER, K.C.S.I. London: Henry Frowde and Hodder & Stoughton, 1921. pp. x + 326. 16*s.* net.

This is an interesting book, on account of a certain originality of out-of-date thought, not because of its psychological value, as the following quotation serves to illustrate—"All pleasures and displeasures involve the intervention of the brain, since it is this which renders them *conscious*, by combining motor excitement with sensory excitement. When the motor excitement is simply the effect of sensory impressions upon the brain...it gives pleasure of the subdued type which we describe as 'interesting.' It becomes aggressively pleasurable or displeasurable when it is sympathetically controlled or intensified by the vibrations of motor excitement on the psychic plane" (226).

The Beloved Ego: Foundations of the new Study of the Psyche. By Dr WILHELM STEKEL. Translated by Rosalie Gabler. London: Kegan Paul, 1921. pp. xv + 237. 6*s.* 6*d.* net.

The nineteen chapters of this book, dealing with doubt, the fear of joy, envy, impatience, etc., are delightful reading. The writer is a poet as well as a physician. "It is a generally accepted fact," he declares, "that a doctor must be an artist." He claims to have retained the best of Freud's teaching and to have evolved from it a new one.

How to Use your Mind. By Dr H. D. KITSON. Philadelphia: J. B. Lippincott & Co., 1916. pp. 217.

The author was Instructor of Psychology in the University of Chicago, where special attention is given to "supervision of study. All freshmen in the School of Commerce and Administration of the University are given a course in Methods of Study, in which practical discussions and demonstrations are given regarding the ways of studying the freshman subjects" (4). The book deals in a popular, common-sense way with note-taking, first aids to memory, examinations and kindred topics.

Anatomy of the Nervous System: from the Standpoint of Development and Function. By Professor S. W. RANSON. Philadelphia and London: W. B. Saunders & Co., 1920. pp. 395. 32*s.* 6*d.*

The title of this book indicates the author's point of view. He is to be congratulated on the result of his labours. The book contains 260 illustrations, some in colour, all beautifully reproduced, and most of them made from drawings specially prepared for the author. Sherrington's classification and terminology have been adopted.

How to Measure. By Professors G. M. WILSON and K. J. HOKE. New York: The Macmillan Co., 1920. pp. vii + 285.

This book is intended to serve as a text-book in the training of teachers. It does not attempt to estimate the value of all available tests, but describes only those "which on account of their use, purpose and adaptability have been found to be most serviceable." Successive chapters deal with the measurement of spelling, handwriting, arithmetic, reading, composition, drawing, general intelligence, etc. References solely to American literature are added to each chapter.

Measure your Mind. By F. P. STOCKBRIDGE and Dr M. R. TRABUE. London: G. G. HARRAP & Co., 1920. pp. v + 349. 10*s.* 6*d.* net.

This is a well-written, easily readable book, giving a very useful account of a large number of tests. These, however, are of somewhat unequal value, and hence are not to be adopted without discrimination.

Trade Tests: the Scientific Measurement of Trade Efficiency. By Dr J. C. Chapman. London: G. G. Harrap & Co., N.D. pp. vii + 435. 15*s.* net.

The contents of this book are in great part pamphlets which have been already published, describing the trade tests devised to meet the needs of the American War Office. It is disappointingly lacking in constructive criticism.

Dr Klages's Entwurf einer Charakterkunde. Von Dr O. HERMANN. Leipzig: J. A. Barth, 1920. S. iv + 63. Mk. 15.

This is a précis of Klages's *Prinzipien der Charakterologie,* issued in the same year by the same publishers, according to which the 'material' of character is classifiable into ideational capacities varying in quantity, quality, distinctness and mobility, and into apperceptive dispositions varying in grade, direction and form: while the 'structure' of character is constituted by a number of factors according as it is of the affective or volitional type. A detailed account of this condensed presentation by the author of an undoubtedly difficult book is, however, quite impossible here.

Psycho-analysis, Sleep and Dreams. By ANDRÉ TRIDON. London: Kegan Paul. pp. xii + 160. 7*s.* 6*d.* net.

This is a popular, 'scrappy' book about sleep and dreams. Its journalistic style may make it attractive for some readers, but it cannot be regarded as having any particular interest for the serious student.

NOTES ON RECENT PERIODICALS

Journal of Experimental Psychology, Vol. III

No. 5, October 1920.

The Effect of Fatigue on Retention (John J. B. Morgan).

When a period of learning is long continued, a large amount of material learned, and the learning partial only, the capacity of retention may diminish considerably in the later stages of the task. Under the same conditions, the recognition of material used in the later part of the series presented is defective. This is interpreted to mean that adaptation to fatigue involves a narrowing of attention to the precise task on hand.

Effects of Smoking on Mental and Motor Efficiency (Sven Froeberg).

The experiments support the view that there is no reliable evidence that the moderate use of tobacco produces either beneficial or harmful effects in a measurable degree.

A Study of Ocular Functions with Special Reference to the Look-out and Signal Service of the Navy (C. E. Ferree, G. Rand and D. Buckley).

The conditions under which satisfactory tests of keen visual discrimination in low illumination must be carried out are discussed. The most useful type of test must determine directly a candidate's power to see clearly at low illumination.

Block-Design Tests (S. C. Kohs).

Performance tests, in which blocks painted in different colours have to be combined so as to produce certain designs, are described, and some of their results discussed.

Recent Apparatus from the Psychological Laboratory of McLean Hospital (F. L. Wells and C. M. Kelley).

The apparatus described comprises: a galvanometer control board, a "fall" apparatus, a choice reaction switchboard, a "fall" screen, a multiple pen chronograph, and a water recording device useful in relation to tests of motor steadiness or tremor.

Sex Differences in the Effect of Discussion (Harold E. Burtt).

Experiments on the effect of discussion upon decisions show that there is "considerable tendency to change one's decision as a result of discussion, but the change is as often in the wrong direction as the right." No appreciable sex differences are found.

Practice Effects in Intelligence Tests (Knight Dunlap and Agnes Snyder).

It is shown that the practice effects in the taking of intelligence tests may be considerable. A plan for equalising these, as far as possible, for different groups of reaction is proposed.

No. 6, December 1920.

Psychological Tests used for Selecting Aviators (G. M. Stratton, Henry C. McComas, John E. Coover, English Bagby).

The tests which seem best adapted to determine aviational ability are: (1) judgment of relative speeds of moving objects, (2) judgment of parabolic curves, (3) complex reaction time. These must be given further trial.

The Relative Value of Grouped and Interspersed Recitations (E. B. Skaggs).

Taking it as proved that recitation (attempted recall) facilitates the learning process more than mere reading, the experiments show that to alternate readings and recitations produces better results than to group both. The results are more conclusive for nonsense than for sense material.

Results of some Experiments on Affection, Distribution of Association and Recall (C. H. Griffiths).

The most interesting conclusions are: (*a*) pleasant associations tend to be more numerous than unpleasant associations, (*b*) affective reaction of any kind is favourable to recall, there being a slight preference for pleasant associations, (*c*) there is evidence for the existence of a general affective sensitivity.

Two New Time Control Instruments (Donald A. Laird).

(i) "An acceleration time control apparatus," consisting essentially of an inclined U-shaped groove, with alternating strips of insulating and conducting material, and (ii) a disc-commutator time control instrument are described.

Journal de Psychologie normale et pathologique (28^{me} Année)

No. 1, January 1921.

Sur le psychisme inconscient (Fr. Paulhan).

All psychologically distinguishable processes—such as perception and recognition—may be carried on unwittingly. If afterwards they should become known, their nature, as facts of mental structure, is in no way affected, though they may acquire new associations. What has been unwittingly apprehended may later be consciously recognised.

Le rire (G. Dumas).

Five problems are distinguished and discussed: (1) what are the physiological and anatomical mechanisms of laughter? (2) what are the main conditions of laughter considered as an expression of well-being? (3) why is it that the perception of certain relations are considered comic? (4) by what psychophysical mechanism does laughter result simply from the perception of these relations? (5) in what ways may laughter be said to constitute a social language?

La conscience et la conscience du moi (H. Wallon).

The development of the consciousness of self as contrasted with consciousness of a world of extended objects is discussed. Eventually the self is conceived as wholly independent of all spatial determinations.

No. 2, February 1921.

De l'automatisme dans l'imitation (H. Delacroix).

Various forms of imitation and their accompanying mechanism are analysed. Their development and application are studied particularly in reference to language. The precise relation between perception and movement in imitation is given particular consideration.

Les oscillations de l'activité mentale (P. Janet).

A course of study in the conditions determining the degree of intensity of different psychical responses is sketched. Special reference is made to variations in emotional expression in the normal and the abnormal subject.

Sur le psychisme inconscient (Fr. Paulhan).

The unconscious is considered in reference to tendencies and desires, and it is found that these, just as much as cognitive and affective processes may proceed unwittingly. In fact the unconscious mental life may be described in just the same terms as the conscious mental life. Underlying both are the same tendencies, the same structure, and both have to deal with very much the same material.

André Gilles writes a short note on the illusion of false recognition, and a paper by Etienne Rabaud on a "Contribution to the Study of the Instinctive Behaviour of Certain Spiders," read at a meeting of the *Société de Psychologie* is reproduced.

No. 3, March 1921.

Théorie de la perception (B. Bourdon).

The paper discusses both the general psychological character of perception, and certain special problems, including: the perception of space, whether visual or auditory, the perception of movement, and the development of spatial perception. Most of the traditional questions are discussed.

L'orientation lointaine (E. Claparède).

The ability to pursue a goal which cannot be immediately perceived by the individual who seeks it, is here studied in detail. A number of proposed explanations are discussed. The problem is itself analysed. The general conclusion is that many complex problems are involved, which are as yet far from being satisfactorily settled.

La folie, pensée organique (A. Hernard).

The organic basis of various forms of nervous disease is studied in its relation to the conscious and symbolic expression of such disease.

Caractère individuel et aliénation mentale (W. Boven).

The purpose of this paper is to show precisely how investigations into individual character may throw light upon the real causes of mental disorder. The study of individual differences should take into account, not only the mental development of the patient himself from his earliest years, but also the characters of his immediate ancestors.

No. 4, April 1921.

Sur le centre de la faim (J. P. Pawlow).

There is a thoroughgoing analogy between the respiratory and alimentary centres. The latter possesses complex functions which demand analysis. Its localisation is uncertain, but it undoubtedly must be conceived as distributed to different positions within the central nervous system. It is a regulating nervous structure determining the reception of solid and liquid material necessary for vital chemical changes.

Le mécanism de la pensée (G. Revault d'Allonnes).

The paper is presented as a criticism of Taine. Its main contention is that the notion of 'image,' widely used in the psychology of thinking, is confused and misleading. For images, schemata must be substituted, when the difficulties incident to the image-type of psychology can be overcome.

Activité mentale, travail intellectuel et fatigue (G. Poyer).

The essay deals in general with mental fatigue, its main physiological and psychological manifestations, with individual differences in these manifestations, and with fatigue in an extreme and abnormal degree. The psychology of work and fatigue is destined to play a most important part in industrial organization.

A report of a meeting of the *Société de Psychologie*, with a paper by G. Dumas on *Les mouvements volontaires*, is added, and also a report of a meeting of the *Société de Psychiatrie de Paris*, with an abstract of a paper by Mm. G. Maillard and Codet, on a *Diagnostic rétrospectif d'encéphalite épidémie avec sequelles psychiques.*

No. 5, May 1921.

Le Mouvement sans nerf (P. Wintsebert).

The paper presents an experimental study of primitive types of movement, chiefly in regard to the dual development of muscular and nervous systems.

Under the head of *Notes et Documents* L. Livet contributes a descriptive account of *Les rêves narcotiques et leur conséquences.* Reports of meetings of the Société de Psychologie contain papers by Ch. Lalo on *L'Art et la Vie*, by H. Wallon on *Psychonévroses et troubles d'origine sympathique*, and by Marcel Mauss on *L'expression obligatoire des sentiments*, a discussion dealing with certain of the oral ritual of Australian funeral ceremonies.

No. 6, June 1921.

La notion de partie chez l'enfant (J. Pieget).

The experiments show that the notion of 'part' is exceedingly ill-defined in young children. Its growth is traced, and its complex nature analysed.

La mémoire des noms propres et la fonction du réel (L. Dupuis).

Memory for proper names is a special practical application of verbal memory to purposes having a social value. It is found to a high degree only in persons who have a lively interest in their actual social environment.

A paper read to the Société de Psychologie by Étienne Rabaud entitled, *L'instinct maternel chez les mammifères*, and a sketch "*D'une généalogie des catégories esthétiques*," by G. Belot contributed to the same society are included.

The Psychological Monographs, Vol. XXIX

No. 1, whole No. 130.

A Qualitative Analysis of the Process of Forgetting (Harold R. Crosland).

Specific to forgetting are processes of 'typification' and 'analysis,' together with the mechanism of condensation, displacement or transposition, dramatization and secondary elaboration. In typification some facts stand out as central, the rest being dissociated from these, and forgotten, while in analysis the facts retained are selected and clarified. Interpolative or constructive processes in forgetting proceed much more slowly than destructive processes. Equally important are the mechanisms of certainty and uncertainty, acceptance and rejection. Affective factors appear to play comparatively little part in forgetting.

No. 2, whole No. 131.

The Growth of Intelligence (Edgar A. Doll).

The accepted theoretical curve of normal growth of intelligence misrepresents the probable facts. For practical purposes an empirical straight line curve of average normal growth in terms of relative units of mental age may be employed as a standard of comparison. Probably the adult level of intelligence is reached between the ages 13 and 14 years. Individual variability in the rate of intellectual growth of superior children is very marked. The contention that the intelligence quotient is approximately constant between the ages of 4 and 16 is not justified.

No. 3, whole No. 132.

Mental and Educational Measurements of the Deaf (Jeannette Chase Reamer).

A set of non-language mental tests, and an "educational survey test" were given in group form to 2500 deaf children. The non-language tests give normal distributions at each age from 8 to 21 +. The Educational tests give normal distributions from 10 to 18. Mixture of deaf and hearing children in the same school is markedly productive of bad results. The congenitally deaf are slightly below the adventitiously deaf both in mentality and in educational attainment. There is practically no difference between the partially and the totally deaf. Onset of deafness after six is less injurious to future educational progress than is deafness acquired before that age. The average difference in mental ability between hearing and deaf children is about two years, while for educational purposes the latter, as compared with the former are retarded by about five years, the discrepancy between the mental and educational retardation being due to the language handicap.

No. 4, whole No. 133.

A Socio-Psychological Study of Fifty-three Supernormal Children (William T. Root, Jr).

The results of a large number of different psychological tests applied to the group of superior children of varied age are described, and the consequently superior performances are analysed. Special attention is given to the effect of social factors,—the home influence, the child's social group, etc. Moral as well as intellectual factors are considered.

Beihefte zur Zeitschrift für angewandte Psychologie.

No. 23, 1920. Mk. 18.

Übungsfähigkeit und Übungsfestigkeit bei geistiger Arbeit (E. Gellhorn).

Three tasks were employed,—letter cancellation, multiplication and addition of single numbers. The practice curve for a given individual is found to be of the same form for all three tasks. Some persons show practice only during the work, some only after it, some both during and after the work. The latter show a want of uniformity in all three tasks. The course of the curve of retentivity of practice is generally the same for a given individual, but it differs widely in different people.

No. 24, 1920. Mk. 22.

Über die Fernald'sche Methode zur Prufüng des sittlichen Fühlens und über ihre weitere Ausgestaltung (L. Jacobson-Lask).

An investigation upon children, youths and adults. The author concludes that with appropriate precautions and modifications this test can evaluate in young persons the ethical standard as effectually as the Binet-Simon tests can evaluate their intellectual standard.

No. 25, 1921. Mk. 28.

Zur Psychologie des Amputierten und seiner Prothese (David Katz).

An investigation into the illusions of those whose limbs have been amputated. The author studies the sensibility to touch, the localisation and the spatial threshold of the stump, and the differential sensibility for raised weights in the stump and in subjects provided with Sauerbruch's 'muscle canals.'

No. 26, 1921. Mk. 30.

Die Feststellung der psychischen Berufseignung und die Schule (Erich Stern).

The author insists that despite vocational advice the subject must believe that he has himself chosen his vocation. He analyses the material collected by the late Dr Minkus at Breslau concerning the written preferences of school-children for their future vocation, and his own material similarly obtained from Hamburg children, and he shows the insufficiency of the results unless supplemented by such specific questions as "Why do you choose that vocation?", "Who advised you to choose it?", "Which of these groups of vocation would you choose (unskilled, manual, office, business, personal, domestic, agricultural, artistic, higher professional)?", "What would be your order of preference of the following vocations within the particular group you have chosen?", "Are you good at such and such an occupation?", "Do you consider yourself well fitted for the occupation you prefer?" Questions were similarly asked of the teachers and parents of the children, and to the children were given certain group vocational tests.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY

GENERAL MEETING

June 11, 1921. Nonsense Syllables and the Word Association Method of Jung, by R. J. Bartlett.

A preliminary Investigation into Suggestibility, by F. H. Cracknell.

July 23, 1921. Methods of Dream Analysis, by W. H. R. Rivers.

The Nature of Sentiments, by C. S. Myers.

(i) Some new Apparatus for Auditory Experiment; (ii) A portable Reaction time Apparatus; (iii) Tests of manual dexterity, by F. C. Bartlett.

Experiments on Aesthetic Appreciation, by L. Feasey.

(i) A Test of Muscular Precision; (ii) Motion study applied by Miss Bevington to the Lay-out of a Compositor's Case, by B. Muscio.

(i) Vocational Tests for Typewriting; (ii) the Smedley Dynamometer, by S. C. M. Sowton.

Psycho-galvanic Reactions in Asylum Patients, by E. Prideaux.

The Effects of Deprivation of Oxygen on Mental Efficiency, by J. P. Lowson.

(i) A Vindication of Helmholtz's Theory of Hearing; (ii) Colour Filters having Remarkable Properties, by H. Hartridge.

SECTIONAL MEETINGS

Education Section.

April 19, 1921. A National System of Education, by J. Clerk Maxwell Garnett.

May 24, 1921. The 'Ineffable' Affect of the Psycho-neurotic known as Anxiety and its bearings on the problems of Adolescence, by J. Turner.

June 8, 1921. Intelligence Tests, by Maria Montessori.

Medical Section.

April 27, 1921. Enforced Psycho-synthesis in certain cases of Analysis, by Paul Bousfield.

May 25, 1921. Emotion and Eye Symptoms, by W. Inman.

June 22, 1921. The Influence of the Endocrines in the Psychoneuroses, by W. Langdon Brown.

VOLUME XII DECEMBER, 1921 PART 3

THE NORTHUMBERLAND MENTAL TESTS.

BY GODFREY H. THOMSON.

(*From the Department of Education, Armstrong College, Newcastle-upon-Tyne.*)

I. *Object of the tests* (pp. 201, 202).
II. *Preliminary investigations* (pp. 202, 203).
III. *The test booklet* (pp. 203–207).
IV. *The practice test* (p. 208).
V. *Marking the tests* (pp. 209, 210).
VI. *Norms of performance* (pp. 211–214).
VII. *Comparison with Stanford revision, etc.* (pp. 214–216).
VIII. *Distribution of the intelligence quotient in different types of school* (pp. 216–219).
IX. *Promotion within the elementary schools* (pp. 219–221).
X. *Conclusions* (pp. 221, 222).

I. OBJECT OF THE TESTS.

THE primary object of these tests was to discover gifted children worthy of free secondary education among those elementary schools of the County of Northumberland which had not this year sent in any candidates for the orthodox examination in English and Mathematics on which such free scholarships are usually awarded.

From one quarter to one third of the elementary schools of this county present no such candidates: and from year to year these are in the main the same schools, chiefly found in the remoter country districts. Northumberland is indeed rather sharply divided into a small densely populated district on the coalfield in the south east, and a large and very sparsely populated area comprising *inter alia* Tyndale, Redesdale, Coquetdale, all the Cheviots and Ottercaps, the Till and Breamish valleys, and many fell districts not in the Cheviots proper. The lack of candidates from the small schools in this area is presumably due in the main to (i) the difficulties of reaching a secondary school even if a scholarship were won and (ii) the difficulty of an isolated country schoolmaster or schoolmistress (teaching perhaps all the standards single handed) competing in the preparation of candidates with experienced heads of urban schools aided by a capable staff. It was felt that these difficulties could be in part overcome if some maintenance scholarships were given on a mental test which gave an equal or almost an equal chance to an unprepared candidate. Accordingly the County Education Authority, inspired mainly by Dr Andrew Messer of Lemington, offered twenty such scholarships (in addition to those given on the orthodox examination) open only to schools which had not competed in that examination, and asked me to prepare and to superintend a mental test for the candidates.

In order to gain a proper background for comparison, the mental test was to be given not only to the candidates who thus offered themselves, but also to whole schools and to certain whole districts as a survey, and finally also to a number of the already successful candidates on the orthodox examination. Thus there were nearly 3000 subjects for the tests, 414 of these being candidates for scholarships on the result of the tests.

II. PRELIMINARY INVESTIGATIONS.

As scholarships were to be awarded on the results, there was a strong feeling, shared both by myself and by Mr C. Williams, the Director of Education, that tests which were already public property could not be used. It was decided therefore to prepare and to standardise a test specially for this purpose, and it was planned to give Binet or other well-known tests ultimately, by way of check, to the best candidates. The preliminary work in connexion with the preparation of the tests was begun about Christmas 1920. I was indebted during its progress to a number of psychologists who aided me with suggestions, and to many teachers and headteachers of schools in distant parts of the United Kingdom, many being old pupils and old fellow students of my own[1]. In this way single tests, usually requiring ten minutes to be given to a group of 50 children, were tried in a rural county in the south, in a middle-sized mill town, in a cathedral city in the midlands, in the Channel Islands, in Scotland, in a large English city, and elsewhere, including one batch in America. Approximately 1000 children in all were subjects of some one or other of these preliminary trials, and each test was given usually to about 250 children.

By these preliminary trials the (for our purpose) less satisfactory tests were 'scrapped,' and gradual improvements were made in the survivors. The list of tests at Easter, 1921, was as follows:

---

[1] I am very grateful to all who helped and am only prevented from thanking these by name by their number and by the desire of some to remain anonymous.

Newly devised tests:

- (i) Hindustani Test.
- (ii) Extra Number Test.

Tests in part new:

- (iii) Middle Word Test } both based on the work of Professor Stern of Hamburg
- (iv) Schema Test } but new in form of presentation.

Well-known forms of tests:

- (v) Extra Word Test.
- (vi) Number Series Test.
- (vii) Analogies Test.
- (viii) Opposites Test.
- (ix) Directions Test.
- (x) Completion Test.

Of these the first six ultimately formed the test proper. In thus choosing a large proportion of new tests I was actuated in part by a desire to make coaching for the tests almost impossible and in part, I confess, to a desire to make this experiment as original a contribution as possible. It appears to me very desirable that large numbers of new tests should be tried at every opportunity so as to increase our armoury.

III. THE TEST BOOKLET.

At Easter, 1921, a booklet of tests based on the above was put into type and between then and June 400 more children in various centres were tested by this booklet, in which only small changes were made. It is therefore given here in its final form (much reduced in size of print).

The Supervisor should check age and birthday

Name____

Age last birthday__________________________________

Birthday_______________________________________

School____

Standard______________________________________

To-day's date____________________________________

Wait for the signal before you turn over.

There are some easy questions inside and you have to answer them as quickly and carefully as you can. Begin at the beginning and go straight through. If any one is too hard for you, miss it out. You have an hour to do them in. If there is no clock in the room you will be told the time every quarter of an hour. Stay till the end.

Ask no questions at all.

TEST A 1

In the tests given below you have to cross out the extra word in each line. For example look at this first line:

wood cork stone boat bladder

The extra word there is stone, because all the others float on water. Look at this second set:

chair table stool desk roof

The extra word there is roof because all the others are articles of furniture. Now try to cross out the extra word in each of the remaining lines:

grapes oranges wool apple banana
rifle plough sword pistol lance

TEST A 2

You have to give the number that comes next in each of the following lines of numbers. The first three are answered for you to show what is meant:

1 2 3 4 5 6 (7)

Here the number that you have to write in the brackets is 7 because the first six numbers go up by steps of one at a time. The series below also have steps of different sizes but they do not always increase by adding the same number each time, but in other ways as well:

11 10 9 8 7 6 (5)

Here the numbers come down.

1 2 4 8 16 32 (64)

Here each number is twice as big as the one in front.

Now try the others:

3 6 9 12 15 18 ( )
2 4 6 8 10 12 ( )
1 3 5 7 9 11 ( )
1 2 3 4 3 2 ( )

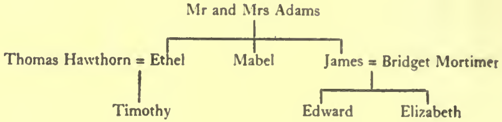
TEST A 3

Read the following paragraph carefully and compare it with the diagram. The diagram gives the same facts as the paragraph.

PARAGRAPH

Mr and Mrs Adams had three children, (1) Ethel (the eldest), (2) Mabel and (3) James (the youngest). Ethel married Mr Thomas Hawthorn, and their son was named Timothy. James married Miss Bridget Mortimer, and they had two children (1) Edward and (2) Elizabeth.

DIAGRAM



Now answer these questions (the first two are answered for you to show what is wanted):

	Answers
What relation is Timothy to Mabel?	Nephew
How many aunts has Edward?	Two
What is Timothy's surname?	
What is Elizabeth's surname?	
Who is Timothy's uncle?	

TEST A4

In your mind (without writing them down) you have to arrange the five words in each line below in their proper order, and then underline the middle word of this order. For example, consider

minute second year hour week

In their proper order these words would run thus: second, minute, hour, week, year. The middle word is hour and therefore hour is underlined above. Similarly shilling is the middle word of this next set

sixpence penny florin sovereign shilling

Now try to do the next tests in this way.

elephant sheep mouse cow puppy
forty thirty fifty ten twenty

TEST A5

You have to cross out the "extra" number on each of the following lines. For example, in

6 2 8 7 4 10

you cross out 7 because it is the only odd number, all the rest agree in being even numbers.

In this next case

4 7 8 6 19 3

you cross out 19 because it is the only number in double figures. Again in

14 3 15 9 6 12

you cross out 14 because all the rest are divisible by 3. Try to find and cross out plainly the "extra" number in each of the following lines:

18 16 4 8 20 12
5 9 3 4 1 7
75 62 20 10 15 25
17 49 4 24 13 18

TEST A6

The sentences below are in a foreign language, and their meanings are given in English. In each English sentence a word is underlined, and you have to underline the word which corresponds to it in the foreign sentence. You can do this by comparing the sentences with each other. For example, look at these:

1. Kuchh malai	some cream.
2. Kuchh puri leoge	will you take some cake?
3. Misri leoge	will you take sugar?

By comparing 1 and 2 you see that kuchh must mean some because it occurs in both sentences. Underline it in sentence 1 before you go any further. By comparing 2 and 3 you see that leoge means will you take. Underline it in 3. Then the only word you do not know in 2 is puri which must mean cake, so underline it in 2. You have now underlined in each foreign sentence the word corresponding to the underlined part of the English sentence.

Notice that the foreign words are not always in the same order as the English words.

Underline your words plainly.

You have not to write anything, only to underline the proper words.

Now try these:

Ek piyala chae	A cup of tea.
Yih chae bahut achchhi hai	This is very good tea.
Chae bilkull taiyar hai	Tea is quite ready.
Kab taiyar karoge?	When shall you make ready?
Main bahut pyasa hun	I am very thirsty.
Bahut achchhi hai	It is very good.
Yih mera rumal nahin hai	This is not my handkerchief.

(You may turn back to read examples if you wish to do so.)

TEST B 1

Cross out plainly the "extra" word in each of the following lines:

charity	kindness	benevolence	revenge	love
square	circular	oblong	hexagonal	triangular
needle	tack	nail	knife	pin
coal	bread	coke	wood	paper
bran	wool	cotton	hemp	jute
hair	feathers	wool	grass	fur

TEST B 2

Give the number that comes next in each of the following lines:

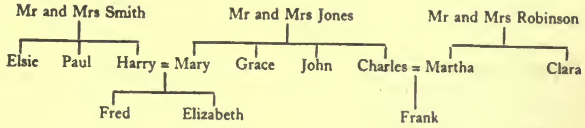
1	3	9	27	81	243	( )
17	15	13	11	9	7	( )
1	2	4	7	11	16	( )
96	48	24	12	6	3	( )
7	5	5	7	5	5	( )
9	8	7	6	7	8	( )

TEST B 3

Cross out the "extra" number in each of the following lines:

26	3	7	31	13	17
18	22	30	24	6	12
81	27	11	1	9	3
8	2	16	32	6	4
7	28	21	35	14	27

TEST B 4



From the above diagram answer the following questions:

	Answers
What is Frank's surname?	
What relation is Elizabeth to John?	
What relation is Elizabeth to Frank?	
What relation is Elizabeth to old Mr Jones?	
How many aunts has Frank?	
How many uncles has Fred?	
Who are Frank's grandparents?	

TEST B 5

PART 1

In your mind (without writing them down) you have to arrange the five words in each line below in the proper order and then underline the middle word of this order:

paragraph	book	chapter	sentence	word
house	street	room	town	county
orange	red-currant	grape	plum	melon
general	sergeant	captain	private	corporal

PART 2

In the remaining line two such sets of five words are mixed up together. You have to separate them mentally, arrange them in order, and underline the middle word of each. **Do not write them down,** just underline the proper two words.

leaf, leafvein, twig, sowing, reaping, thrashing, branch, ploughing, tree, baking.

TEST B 6

In each foreign sentence underline the word which corresponds to the underlined word in the English sentence.

Underline your words plainly. **You have not** to write anything, only to underline the proper words.

Mausam badalta hai	It is changeable weather.
Achchha khub mausam hai	It is good weather.
Sirf das baje hai	It is only ten o'clock.
Das gaz napo	Measure ten yards.
Ham kahan jaen?	Where shall we go?
Ham chha baje khama khawenge	We shall dine at six o'clock.
Yih rah kahan jati hai?	Where does this road lead to?

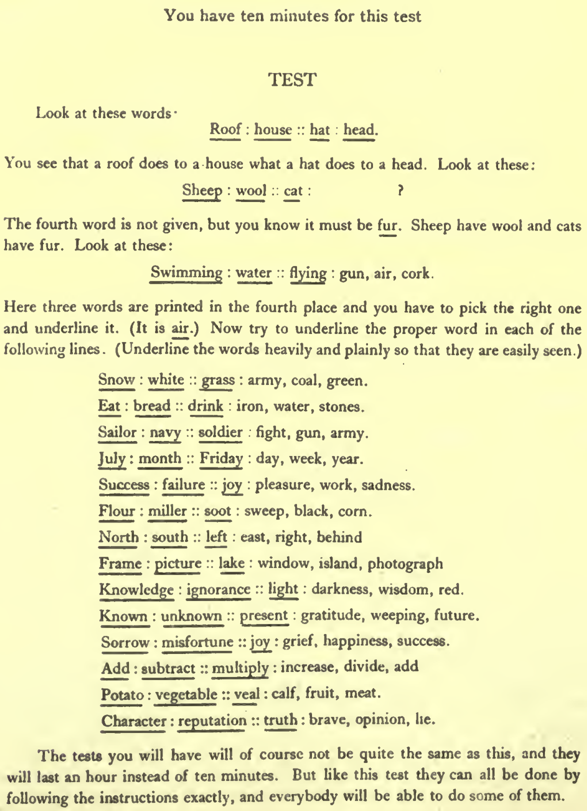
Look over the tests again, until time is up.

One principle which guided the formation of this booklet was that of leaving little or nothing to the Supervisors, who, it will be seen, are merely required to have the front page filled in and then to see that the children have exactly one hour in which to attempt the questions. The tests were given in the children's own schools, and supervised by their own teachers. It was felt by many that in these circumstances the fairest method was one which eliminated the untrained supervisor as much as possible; for there might be differences in the ability to give the tests verbally, and some teachers might even, all unconsciously, give more help than others.

Another principle was the avoidance of any emphasis on speed. It will be found that practically all children tested can finish all they can do in the hour.

IV. THE PRACTICE TEST.

Fears were expressed by some with whom the tests were discussed that certain highly-strung children might by the very novelty of the tests be thrown into a dazed condition and be totally unable to do themselves justice. It may be said at once that none of the teachers who gave the tests in the preliminary investigations reported anything of this sort whatsoever. On the contrary, it was universally reported that the pupils thoroughly enjoyed the tests. As a further safeguard, however, against undue shock from novelty, and as a preliminary practice, the following ten minutes' test was devised. It was given on the day preceding the test proper except in two schools where the parcel did not arrive in time.



You have ten minutes for this test

TEST

Look at these words:

Roof : house :: hat : head.

You see that a roof does to a house what a hat does to a head. Look at these:

Sheep : wool :: cat : ?

The fourth word is not given, but you know it must be fur. Sheep have wool and cats have fur. Look at these:

Swimming : water :: flying : gun, air, cork.

Here three words are printed in the fourth place and you have to pick the right one and underline it. (It is air.) Now try to underline the proper word in each of the following lines. (Underline the words heavily and plainly so that they are easily seen.)

Snow : white :: grass : army, coal, green.
Eat : bread :: drink : iron, water, stones.
Sailor : navy :: soldier : fight, gun, army.
July : month :: Friday : day, week, year.
Success : failure :: joy : pleasure, work, sadness.
Flour : miller :: soot : sweep, black, corn.
North : south :: left : east, right, behind
Frame : picture :: lake : window, island, photograph
Knowledge : ignorance :: light : darkness, wisdom, red.
Known : unknown :: present : gratitude, weeping, future.
Sorrow : misfortune :: joy : grief, happiness, success.
Add : subtract :: multiply : increase, divide, add
Potato : vegetable :: veal : calf, fruit, meat.
Character : reputation :: truth : brave, opinion, lie.

The tests you will have will of course not be quite the same as this, and they will last an hour instead of ten minutes. But like this test they can all be done by following the instructions exactly, and everybody will be able to do some of them.

V. Marking the Tests.

The rule for marking the tests was in general that only the *exact* answer gained points. This requires interpretation in several cases, however, as given below.

There are 60 items in the tests in all, and for simplicity the marking was made quite straightforward and the score was the number of items correct.

The following rules will appear arbitrary in some instances but they are based on an examination of a large number of cases and appear to be more justifiable than other alternatives. It must be remembered that all the tests are in part directions tests; disobeying directions should therefore lose points even if the answer is intelligible.

*Test A*1. Correct answers *wool, plough,* 2 points. No point if two words in a line are underlined, or if the words are written down, or if all the other words are crossed out and the proper one left. Underlining or ringing the word is accepted as crossing out. Alterations are allowed if they are perfectly clear. *These principles apply* mutatis mutandis *in the other tests.*

*Test A*2. Correct answers 21, 14, 13, 1. 4 points. The point is awarded also if the series is further continued correctly.

*Test A*3. Correct answers *Hawthorn* (or Timothy Hawthorn, or Mr Hawthorn, but *not* Thomas Hawthorn); *Adams* (or Elizabeth Adams); *James* (or James Adams, but *not* James Mortimer, or Mr Adams, or J. B. Mortimer). 3 points. Spelling mistakes are ignored if the meaning can be grasped.

*Test A*4. Correct answers, *sheep, thirty.* 2 points.

*Test A*5. Correct answers 18, 4, 62, 4. 4 points (4 is the only number not in double figures).

*Test A*6. The language is a simplified Hindustani. Correct answers *chae, hai, bilkull, taiyar, bahut, achchhi, yih.* 7 points. No points are deducted if words in the English are unnecessarily underlined (such candidates seldom score more than a chance point in the seven). Marks are neither given nor deducted for the explanatory sentences (kuchh etc.).

*Test B*1. Revenge, circular, knife, bread, bran, grass. 6 points.

*Test B*2. 729, 5, 22, $1\frac{1}{2}$, 7, 9. 6 points. No point for 1 instead of $1\frac{1}{2}$.

*Test B*3. 26, 22, 11, 6, 27. 5 points.

*Test B*4. Jones,
niece (*not* uncle)
cousin (or cousins, *not* half-cousin)
granddaughter (or grandchild, *not* grandfather)
three
three
Mr and Mrs Jones and Mr and Mrs Robinson. 8 points, *i.e.*, 2 for the grandparents of Frank, thus:

one grandparent only	no point.
any 2 or 3 grandparents	1 ,,

4 grandparents 2 points.
too many grandparents no ,,
Robinsons = two grandparents.
Jones and Robinsons = 4 grandparents.
Jones and Robinson = 2 ,,
Jones (alone) = no point.

*Test B*5. paragraph, street, plum, sergeant, twig, reaping. 6 points.
*Test B*6. badalta, mausam, sirf, das, ham, baje, kahan. 7 points.

The marking was done by a team of 25 of my students, under my direction. Of these students 10 were graduates of a year's standing, the remainder undergraduates of two years' residence. All had had a course of lectures on mental tests. I also had the assistance of the staff of my department, Miss Melville, Miss Bailes and Mr Vernon Brown. To all of these my sincere thanks are due.

The tests were worked on Friday June 18th. They were received by us on Monday forenoon, and we began work at 2.30 on that day. The marking, checking, and statistical work was all completed and the Intelligence Quotients entered on the lists by 5 p.m. on Wednesday, after $13\frac{1}{2}$ hours' work.

Each student was supplied with scored copies of the booklet for marking, and with ready reckoners:

(1) To enable the child's age in months to be read off from age last birthday and month of birth.

(2) To enable the mental age to be read off from the score in points (these were prepared after the first marking had been completed).

(3) To assist in performing the division of mental age by actual age so as to obtain the Intelligence Quotient.

These last ready reckoners were based on the fact that the actual ages ranged from about 100 months to 160 months. In the fraction

$$\frac{\text{Mental Age}}{\text{Actual Age}}$$

if the divisor was from 100 to 110, the I.Q. (Intelligence Quotient) was taken approximately as 100 + difference between numerator and denominator.

From 111 to 139, $100 + \frac{5}{6}$ difference.
From 140 onwards, $100 + \frac{2}{3}$ difference.

The general rule was made that all high I.Qs. (above 120) and all low (below 80) were to be exactly calculated by two of us who were expert with slide rules.

VI. Norms of Performance.

From the preliminary investigations it had been tentatively decided that the norms of performance were *about* as follows:

10½	11½	12½	13½ years
18	26	34	42 points.

This prediction was mainly based on 300 cases drawn about equally from a large city, a mill town and a very rural district. It was observed that the rural district gave results which were more than a year behind the large city, and the mill town was between these two: though of course it could not be said whether this was due to actual differences in intelligence in these areas (caused by selection) or to the tests being more suitable for city candidates. It seemed at any rate clear that the Northumberland tests should be scored from norms deduced from the Northumberland returns themselves.

The method by which the above tentative norms were obtained need not detain us here, as it is identical with that now employed in deducing the Northumberland norms, and now to be described.

The raw scores of the candidates were entered upon forms in such a way as to enable the average mark of children of a given age and standard to be readily calculated. These averages are given in the following table. They are based on 2532 cases instead of on the whole number of 2710[1] cases, because when these calculations were made the balance of 178 cases had not yet been tabulated.

Table I, showing average marks out of 60.

	School standard						
Age	Below III	III	IV	V	VI	VII	Above VII
14½				21·5	36·5	44·1	42·6
13½		8·8	15·6	23·6	31·6	**41·6**	45·1
12½	4·8	9·1	12·5	22·8	**31·4**	42·7	52·8*
11½	3·4	8·7	14·1	**23·8**	35·0	45·9*	46·8
10½		8·6	**15·5**	23·7	31·8*	47·0	
9½			18·2	26·8*			

In Table I the Roman numerals refer to the school standard. In England Standard VII is very approximately the Grade VIII of American writers, and so on downwards[2]. This Table should be read in conjunction with Table II which gives the number of cases coming under each of the above categories.

---

[1] In addition to these 2710 there were other cases which came in too late to be tabulated.

[2] Terman counts a child 'at grade' who is in grade V from 10½ to 11½ years of age. I here count a child as in the correct 'standard' who is in Standard IV from 10 to 11 (and so on), which gives, in Northumberland, a symmetrical distribution of advanced and retarded pupils. Standard IV in Northumberland appears therefore to be between Grade IV and Grade V in America, as regards the average age of the children forming these classes

TABLE II, showing number of cases upon which Table I is based.

	School standard						
Age	Below III	III	IV	V	VI	VII	Above VII
14½				2	8	8	5
13½		4	8	33	33	45	9
12½	8	15	77	238	439	206	23
11½	13	50	222	480	255	39	4
10½		20	106	115	29	4	
9½			21	13			

2532 cases in all.

In these tables the category 'age 11½' means all children of 11 but not yet 12 years of age: and so on.

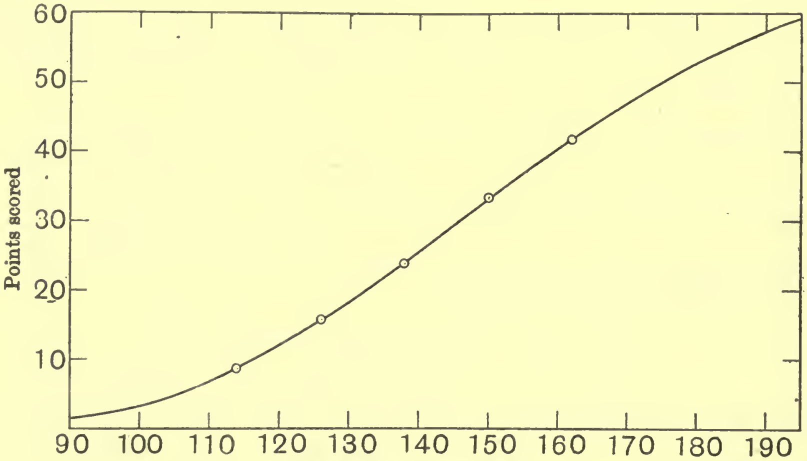


Fig. 1.

Now the obvious plan of obtaining norms of performance from these data would be simply to find the average mark for each total age group. This would however be wrong, because the 14½ year old children here represented are all average or *below* average, whereas the 9½ year old children are all *above* average. The proper numbers to consider are those in heavy type in Table I which give the scores of children who are in their correct standards, and neither advanced nor retarded. To make this clearer, consider the set of scores in Table I which are marked with asterisks. These scores refer to children who are in school standards two years in advance of their proper age. Such children are not represented in the 13½ year old line or the 14½ year old line.

After various mathematical manipulations of Table I which need not detain us here, and which may be described as 'smoothing the surface' which that Table represents, I adopted the following norms of performance:

9½	10½	11½	12½	13½ years
9	15	24	33	42 points.

These points are shown in Fig. 1, where they are joined by a smooth curve which is continued outside these limits so as to form an 'ogive,' the shape which both from theoretical reasons and from some of the preliminary investigations appears the most probable. From this curve the mental age corresponding to each point score can be read off. To facilitate this however the following ready reckoner was prepared.

TABLE III. *Northumberland Norms**.

Marks	Age in months	Marks	Age in months	Marks	Age in months	Marks	Age in months
60	198	44	164	28	144	12	120
59	195	43	163	27	142	11	118
58	192	42	162	26	141	10	116
57	190	41	161	25	140	9	114
56	188	40	160	24	138	8	112
55	186	39	158	23	137	7	110
54	184	38	157	22	136	6	108
53	182	37	156	21	134	5	105
52	180	36	154	20	133	4	102
51	178	35	153	19	132	3	98
50	176	34	152	18	130	2	94
49	174	33	150	17	129	1	89
48	172	32	149	16	128	0	84
47	170	31	148	15	126		
46	168	30	146	14	124		
45	166	29	145	13	122		

* In the case of children who obtain more than 52 marks (mental age 15 years) or less than 9 marks (mental age 9½ years) a harder or an easier test, respectively, is desirable, and the I.Q. calculated from the above norms should, in these cases, be regarded as uncertain, in most cases probably too low. For since the highest mental age obtainable on these norms is 16½, clever children of 12 and over are handicapped by the fact that a very high I.Q. is not arithmetically possible for them, while below a certain age the reading ability required may not be present. The scholarship candidates were all below 12½, but as a safeguard all cases of high marks were separately considered even if the I.Q. was not very high. Even in the full Binet tests the clever older children do not get full scope (cf. Terman, *Intelligence of School Children*, p. 191 footnote).

It must be clearly understood that in its lowest and uppermost reaches this ready reckoner is based upon extra-polation: and in any case probably no very definite mental age can be assigned to the very lowest marks from 0 to 10 (see also footnote to the table). But to facilitate the working of the team of markers it was found better actually to assign mental ages to these very low marks and to carry on the routine of calculation.

The mental age thus found, divided by the actual age (also in months) gave an Intelligence Quotient, based however on these tests alone and not to be too readily assumed to be equivalent to a proper Binet I.Q. The comparison with Binet I.Qs. is still going on, but may be in part described in the following paragraph.

VII. Comparison with Stanford Revision, etc.

The first comparison with the Stanford-Binet-Simon tests was possible during the preliminary investigations owing to the kind co-operation of one of H.M. Inspectors of Schools in the large city already referred to, who had tested by the Stanford Revision a large number of children during the preceding year. To 50 of these the Northumberland Group Test was given, and a correlation with the Binet I.Qs. of about 0·8 was found.

Binet testing of samples of the children is planned for next winter and has indeed already been begun, showing so far a satisfactory correlation.

Of fourteen candidates from rural schools who will be granted maintenance scholarships, eight gave the following comparison:

Index No. of candidate	I.Q. on Northumberland Group Test	Binet I.Q.
148 Y.J.	146	151
72 G.J.	137	134
134 N.E.M	133	135
2647 T.E.I.	133	120
38 G.J.E.	130	127
2420 I.F.W.	129	128
2741 P.E.T.	128	123
60 G.K.	128	105

These were given a full test by Terman's Revision of the Binet scale. Half of them were tested by Mr C. A. Richardson, H.M.I., and half by myself. The only serious discrepancy is the last case, in which in the Binet test there was failure in tests involving imagery of a kind not needed in the group test. The remaining six cases could not be given a full test for lack of time. Two were given a full test which had to be broken off. They gave these results:

Candidate	Northumberland I.Q.	Binet I.Q.
150 M.E.	141	greater than 120
56 S.R.	122	„ „ 109

Four were given a short test of three questions for each year (the questions carrying double the credit in months of the Terman scale). This took only 25 minutes and probably involves a good deal less strain than the full test, so that these candidates' Binet I.Qs. are possibly too high.

Candidate	Northumberland I.Q.	Binet I.Q.
74 O.B.S.	139	137
85 K.A.	133	145
58 B.T.H.	129	135
71 B.C.N.	129	149

In a rather unsatisfactory cyclostyled form the tests have been given (in addition to the elementary school children) to 104 boys in a Preparatory School, varying in actual age from 8 years 4 months to 13 years 11 months. Of these, 32 had on some previous occasion been given a full Binet test by the headmaster, working only from the instructions given in Terman's book. For these 32 cases the correlation between the Northumberland I.Qs. and the Binet I.Qs. is 0·70 ± 0·07. If, as is recommended in the footnote to the table of norms, all cases are excluded of mental ages greater than 15 years or less than 9½ years, there remain 20 cases for which the correlation has risen to 0·83 ± 0·05. They are given below:

Northumberland	Binet	Northumberland	Binet
133	130	113	106
125	113	112	111
124	120	109	98
123	120	106	114
121	136	103	98
118	132	97	89
116	137	92	97
115	121	91	84
115	116	90	90
114	122	84	93

The average Northumberland I.Q. is 110 for these cases, the average Binet I.Q. is 111, the average difference regardless of sign is 7½ points. (The probable error of a Binet test is given by Terman as 4½ points.)

An immediate mass comparison is possible in the following way. In the Northumberland 2710 cases the Test I.Qs. are distributed as follows:

Above 140	0·3 per cent.
131 to 140	2·9 ,, ,,
121 ,, 130	8·6 ,, ,,
111 ,, 120	15·2 ,, ,,
101 ,, 110	22·4 ,, ,,
91 ,, 100	23·9 ,, ,,
81 ,, 90	15·2 ,, ,,
71 ,, 80	8·8 ,, ,,
61 ,, 70	2·4 ,, ,,
60 and below	0·3 ,, ,,

This distribution when compared with that given by Terman in Figure 2 on page 66 of *The Measurement of Intelligence*, is seen to be rather more widely spread. The quartiles of Terman's distribution are about 91 and 109, those of the Northumberland distribution 89 and 111.

Detailed correlations of the Test I.Q. with success in the orthodox examination have not yet been fully worked out. But a remarkably high correlation exists, and is obvious on looking over the lists, between the I.Qs. and the head-teachers' opinion of very bright and very slow children. Whenever on the lists a child is noted as 'very bright,' or 'already successful in scholarship examination,' the I.Q. is high and the opposite is the case with defective and specially slow children.

The single candidates submitted for scholarships on the test from remote country schools formed a group very noticeably in advance of the average. The highest I.Q. found (174, the next highest being 153) was of an eight year old boy in a small border village in the heart of the Cheviots, close to the source of the North Tyne.

VIII. DISTRIBUTION OF THE INTELLIGENCE QUOTIENTS IN DIFFERENT TYPES OF SCHOOL.

It is of interest to inquire whether there are any differences in the results from town schools, small country schools, schools in mining centres, etc., and also whether other differences can be seen between schools in poor and in well-to-do districts.

It must at once be admitted that no clear picture can be obtained from the present investigation, although some points are strongly suggested. A true account of the distribution of the Intelligence Quotients as between school and school could only be given had the whole child population of each school entered for the test, and this was not the case. Those schools which only entered candidates whose chances of doing well appeared to the teachers to be good, will naturally have better results than others where every child attempted the tests.

The following Table IV, and figures 2 and 3 enable a comparison to be made between 17 typical schools in which it would seem from a comparison of statistics that a large proportion of older children were tested.

The suggestion which these figures bring with them is that the most intelligent children are to be found in the Cheviots, where the results are even better than in a well-to-do town suburb. It was mentioned earlier in this paper that preliminary experiments in other counties of England had shown that children in a large city were at least a year in advance of those in a very rural environment. But in that case there were, there can be no doubt, great differences of race and social standing, and the city and the rural district were in entirely different parts of England. Here in Northumberland the population is probably fairly homogeneous as regards race, compared with many other counties, and the Cheviot children are to a large extent the descendants not so much of farm labourers as of Border troopers. The old family names are still the most numerous.

TABLE IV. Various Types of School.

School	Number of cases	Percentage over I.Q. 100	Type of school
A	25	80	Small country schools in the Cheviot valleys
B	17	70	
C	22	50	
D	380	46	Four schools in a moderate sized town
E	91	39	Two schools in a small market town
F	129	45	Four schools in large mining villages
G	128	51	
H	111	32	
I	112	28	
J	141	29	Two schools in a poor suburb of Newcastle
K	63	35	
L	101	55	Two schools in a well-to-do suburb of Newcastle
M	82	56	

Note. Newcastle schools themselves are not under the county.

Miss M. E. Bickersteth's careful inquiry into the mental ability of children in the Yorkshire Dales[1] is a model of what ought to be done in Northumberland. In this present connexion she says: "Some of the schools were only reached after a walk of many miles over the fells, and...at these schools, far from any village, and attended by children living in lonely farms on the moors, sometimes three or four miles from the school, the performance at the tests was almost invariably above the average for the Dales as a whole. In these remoter Dales 84 % of the children were above the average for the Dales as a whole in the reasoning test, and 74 % in the Memory Test, while very marked ability was not infrequently shown in the case of individual children." This verdict could be given almost unchanged as regards the Northumberland country schools, did our rough and ready experiment justify such a definite conclusion. It seems plausible to assume that the draining away of talent to the towns has not touched the most remote spots.

The difference between the poor and well-to-do districts, both equally near Newcastle and approximately equally dense in population, is more certain. Whether it be a real difference in intelligence or not is unproven, but in my own opinion it is probably real in great measure,

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[1] This *Journal*, 1917, IX, 23.

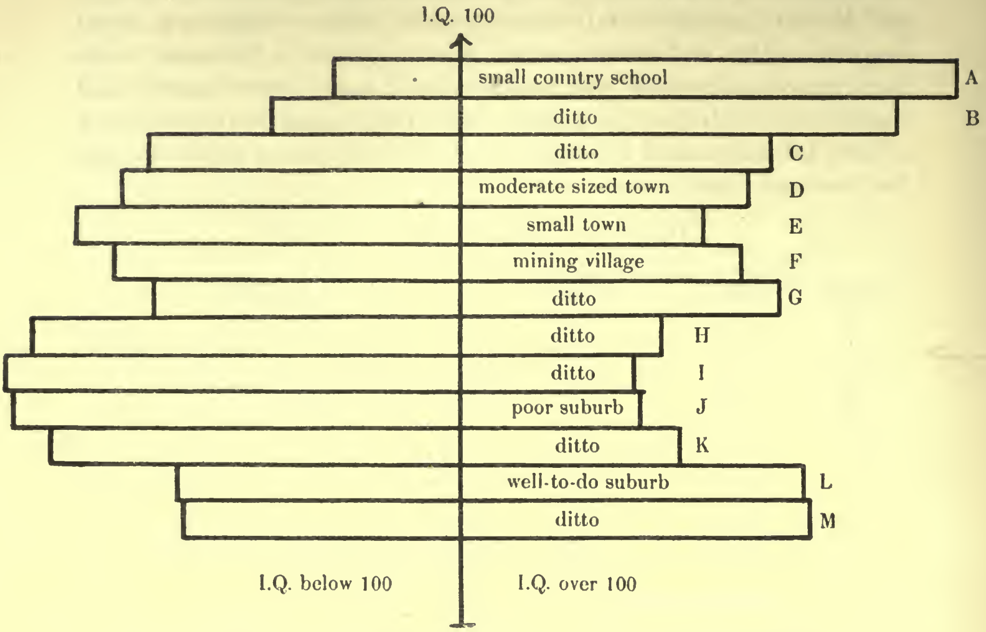

Fig. 2. Proportions above and below average I.Q. in various types of school.

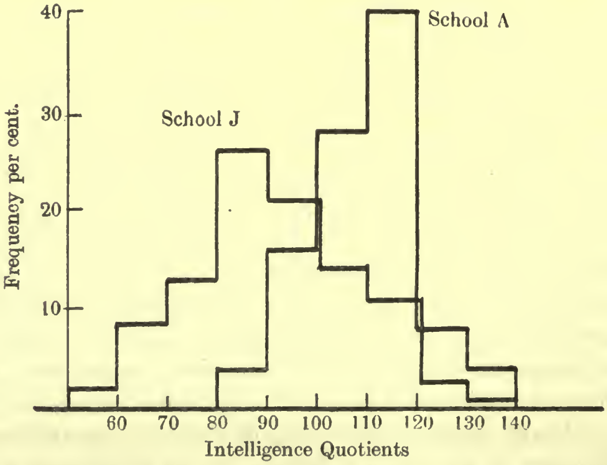

Fig. 3. Percentage distribution of I.Qs. in School A (small country school) and School J (poor town suburb).

though some allowance must be made for vocabulary and dialect difficulties. A comparison with the Preparatory School referred to above (in comparing Northumberland and Binet I.Qs.), is not allowable, for this school has a high reputation and a long waiting list, and its pupils are selected, not representative, members of the wealthier classes, which sufficiently explains the high average I.Q.

IX. Promotion within the Elementary Schools.

During the preliminary experiments (not in Northumberland) it was noticed that, when tabular statements of the results were drawn up in the same form as Table I, in which the cases are separated both as regards age and as regards 'standard' or form in the school, differences between different schools could be observed suggesting that in some promotion was more elastic than in others. Consider for example the following tabulation of 49 cases from a large manufacturing and business city:

Table V, showing average marks in a special case.
49 cases in a large city, not in Northumberland
(full test in almost the final form).

	School Standard				
Age	III	IV	V	VI	VII
13½	—	—	25·7	34·8	**34.0**
12½	—	2·0	26·7	**41.4**	—
11½	11·0	21·7	**36.2**	—	—
10½	—	**25.2**	38·0	—	—
Northumberland norms	9	15	24	33	42

Here it is noticeable that not only do the averages naturally increase in each row from left to right (meaning that boys of the same age in a higher standard do better than those in a lower standard), but also the averages increase from above downwards in each column, meaning that the youngest children in each standard are also the best. This at once suggests the possibility that promotion by ability has not been as free as it might well be. As has been said, this phenomenon was found in some schools and not in others. Very occasionally there were signs of the reverse process, indicating premature promotion, but the type shown in the last table was the most common.

If we now consider the Northumberland results in Table I in the light of this suggestion, we see that there is no steady increase or decrease in the columns; the figures are, within reasonable limits, constant or vary irregularly. This suggests that promotion is well in accord with ability, and indeed it might be expected that in small schools this ideal would be attained more readily than in large schools, for in the former, promotion to a higher standard may not involve any change of seat or of teacher, whereas, in the latter, promotion is often prevented because of difficulties of classroom accommodation, each class having 60 seats which must be kept full, and being unable to increase beyond that number.

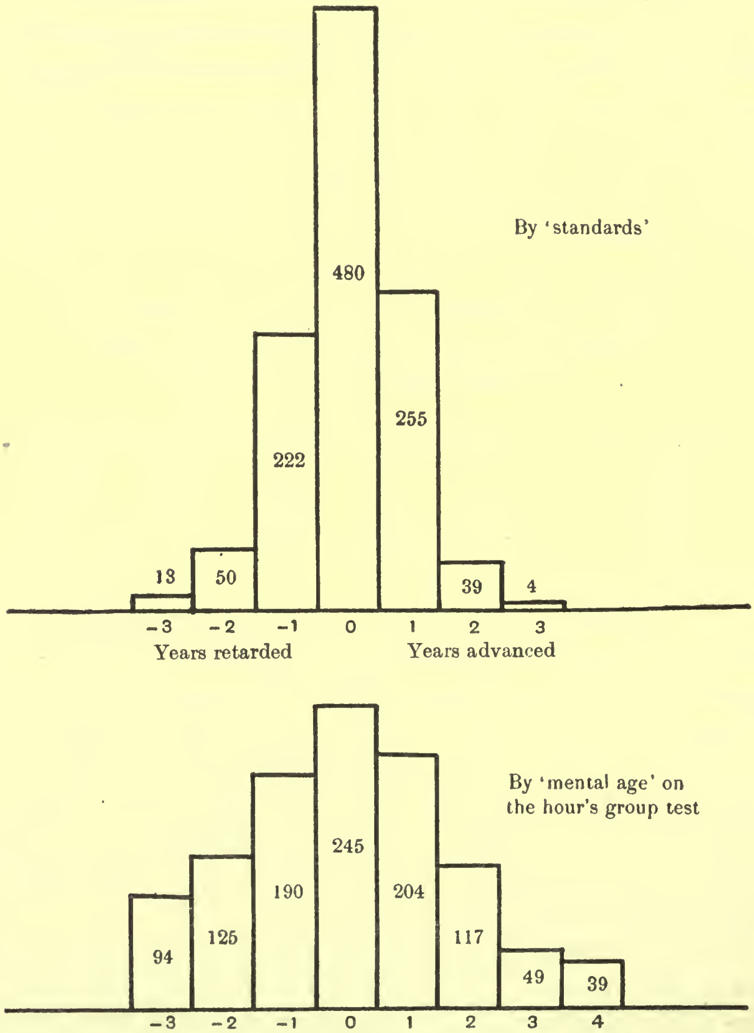

Fig. 4. Advanced and retarded children of 11 years of age.

It may be, however, that the Northumberland averages do nevertheless hide discrepancies of promotion; this possibility is strengthened by the following considerations. Figure 4 shows the distribution of the 1063 children, of 11 years last birthday, who took the test, arranged according to their standard. A similar scatter diagram on the basis of mental age is shown in the lower part of the same figure.

It is seen at a glance that the scatter by standards is much less than the scatter by mental age, although both centre round the same point. It would seem that there is, in spite of our previous argument, an undue reluctance to move a boy either above or below his proper age standard, though the average class ability is kept right. We must of course bear in mind that other factors as well as intelligence are rightly considered in promoting or retarding a boy.

Further analysis of the figures suggests that there is considerable difference, as between school and school, in the freedom with which promotion is given to ability. The figures for 104 cases from the moderate sized town (represented in Fig. 2 by D) suggest that here greater freedom of promotion is desirable. They are given in Table VI:

TABLE VI. 104 cases from a moderate sized Northumbrian Town.
Average marks.

	Standards				
Age	III	IV	V	VI	VII
13½	—	—	—	19·0	—
12½	—	2·3	16·3	**30.8**	44·7
11½	2·0	9·6	**20.1**	38·6	—
10½	—	**19.0**	38·0	—	—
Whole Northumberland	9	15	24	33	42

Here in each standard the youngest children are distinctly the best.

X. CONCLUSIONS.

The experiment which has been described is still in progress. But results so far seem to suggest that intelligence tests may assist Education Authorities (1) in gauging the differences in average intelligence in different parts of their area and in different types of schools, (2) in selecting children for higher education even from schools where remoteness or other factors are a handicap in the academic type of examination, (3) in drawing conclusions as to the freedom of promotion in different schools and (4) in comparing their area with other areas in all these matters.

The correlation of the I.Qs. found by this group test, with full Binet I.Qs., appears to be great enough to make the former a useful guide, and it would be possible without undue expense and time to obtain an I.Q. for every child over 9 in the county within a month.

The distribution of intelligence suggested by the tests is such that the highest ability appears to be found *close to the* cities and *far away* from the cities, the intermediate areas having fewer cases of high ability, as though they were drained by selection.

But all these tentative conclusions must be accepted only with the greatest caution, and the observation of the selected candidates and others with known I.Qs. in their secondary schools, may modify them. Numerous other points of interest also arise, among which may be mentioned the correlation of the different types of test (*e.g.* the Hindustani test, or the Number Series test) with success afterwards in different branches of school work.

(*Manuscript received* 6 *August* 1921.)

A STUDY ON THE PERCEPTION OF FORM.

By ARTHUR RAGNAR GRANIT.

(*From the Helsingfors Physiological Institute.*)

1. *The perception of lines* (pp. 223–227).
2. *Methods of experiments employed* (pp. 227, 228).
3. *Experiments on children* (pp. 228–237).
4. *Experiments on adults* (pp. 237–240).
5. *The construction of forms* (pp. 240–247).

I.

According to Bühler[1], we may in our perceptions of lines distinguish the following variable elements, impressions (i) of rectilinearity or curvature, (ii) of magnitude, (iii) of direction (p. 69). Bühler also gives an elaborate analysis of these perceptions. After having on the basis of exact experiments pointed out the extreme accuracy of our perceptions of rectilinearity, he criticises earlier theories concerning it and arrives at the following hypothesis; "Certain series of retinal elements are marked by a connexion (original or acquired) of the elements with one another. If these are stimulated simultaneously we receive the impression of a straight line" (p. 86). Only, he believed, by assuming in the organ of sight itself a receptive mechanism of this description can we explain our extraordinary susceptibility to deviations from rectilinearity. But this hypothesis cannot explain the perception of curvature (p. 115). We have well nigh innumerably different perceptions of curved lines. A corresponding connexion of retinal elements in this case is out of the question. Bühler emphasises that he is here advancing upon unbeaten paths, and resorts to an analogy with the perception of depth. Both kinds of perception are direct, but nevertheless seem to be caused by many different factors (p. 114). Yet it seems to me that every hypothesis concerning the perception of lines ought principally to keep to the purely linear character that belongs to them all. Besides, Bühler's theory of rectilinearity is liable to the same objection that he himself raises against a similar explanation of the perception of curvature. For we perceive straight lines under every conceivable aspect and in every direction.

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[1] K. Bühler, *Die Gestaltwahrnehmungen*, Stuttgart, 1913.

But where on the retina is it possible to imagine so great a number of "series of retinal elements" that they can be supposed to correspond to all the different directions, in which the straight lines run? We find that Bühler is in this case faced with the same difficulty that prevented him from applying his theory to the perceptions of curvature.

But if we make the linear factor, common to both kinds of impressions, our starting point, the problem assumes a different aspect. Some experiments made during the war by Goldstein and Gelb[1] on a soldier injured in the back of his head, may be mentioned in this connexion. Their patient was unable to perceive lines. The experimenters soon observed that when reading, he used to move his hand as if to follow the written text. He followed print with peculiar movements of the head. The patient was so dependent on these auxiliary movements that, if a written word was crossed over with a few strokes, he was unable to read it. He did not know what lines he was to follow. He behaved in the same manner when confronted with other visual perceptions of form. Goldstein and Gelb tried to isolate the patient's visual perceptions. For this purpose they made some experiments with negative after-images. The patient was told to look fixedly at a strongly lighted triangle and describe his impression. Then the stimulus was removed and he was again asked to describe what he saw. Now it was found that the patient had in the former case seen a triangle, but the after-image he describes in the following way: "It is neither round nor angular; I cannot describe it in any other way" (p. 68). The experimenters were also able to ascertain that he could not distinguish a circle from a triangle if he was ordered to look fixedly at their centres (p. 74). The patient thus had primary visual impressions of surfaces, but could not perceive contours. Goldstein and Gelb's explanation of the phenomenon above described is founded on those theories according to which there are special forms of synthesis or combination (*Gestaltqualität*)[2]. Thus they maintain that their patient lacked those forms of synthesis that are called rectilinearity and curvature.

What then does this 'explanation' of the perceptions of rectilinearity and curvature imply? I call attention to the vague concepts that are current in the literature on this subject. Not even the fundamental term 'Gestaltqualität' has so far been sufficiently defined. This word is used as a common appellation for a number of psychological factors that do not admit of being included within any one system. And yet it seems possible to find a common criterion for all forms of synthesis—*the synthetical impressions cannot be ranged immediately with the sensations and are probably rooted in specifically central processes.* From this point of view the perceptions of lines must be defined as the psychic results of a central psycho-physiological process.

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[1] Goldstein und Gelb, "Psychologische Analyse hirnpatologischer Fälle. I." *Ztschr. f. d. ges. Neurol. u. Psychiat.*, 1918, XLI.

[2] The term is translated according to E. B. Titchener, *A Text-book of Psychology*, New York, 1912, 371–73.

But Goldstein and Gelb seem to have overlooked the points of contact that exist between their investigations and the phenomena of simultaneous contrast. Their patient, let us remember, could not perceive distinct contours in isolated visual impressions. Now it seems most natural to assume that the man suffered from disturbances in the process to which simultaneous contrast is due. Recent investigations[1] have shown the central nature of this process. The contrast can be produced binocularly by means of suitable arrangements, when every possibility of a reciprocal action of different retinal elements is excluded. I think it extremely probably that the same central psycho-physiological process underlies simultaneous contrast and the purely linear character peculiar to the perceptions of lines and contours. A more exact description of the nature of our impressions of rectilinearity and curvature cannot, I think, be formulated by science at present. It is to be observed that this hypothesis cannot show to what factor the distinction between the perceptions of a straight line and a curve is due.

There are several analogous psycho-physiological processes, *e.g.* the filling out of the blind spot. Wertheimer[2], in his important inquiry on our visual perception of motion, seems to regard this as due to some species of 'cross-functions' (*Querfunktionen*) in the higher centres. Whether, at present, so explicit a physiological theory is justifiable or not, must be left to future research to decide. We will content ourselves with recognising that Wertheimer's experiments have shown that there is a specifically central process underlying our visual perceptions of motion. Goldstein and Gelb[3] also mention the connexion between the perceptions of lines and of motion. Some experiments that they made on their patient clearly showed that he could not perceive motion, but only a succession of stages which led him to the conclusion that motion had taken place (p. 92). This is in all respects a very remarkable corroboration.

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[1] Köllner, quoted according to M. v. Frey, *Vorlesungen über Physiologie*, Berlin, 1920, 363.

[2] M. Wertheimer, "Experimentelle Studien über das Sehen von Bevegung." *Ztschr. f. Psychol.* 1912, LXI.

[3] *Loc. cit.*

It is not altogether impossible that certain other phenomena may be referred to the same category. One meets with a similar line of thought in the writings of Watt, who has used for these phenomena the term 'integration.' Without feeling in any way obliged to accept Watt's[1] wide application of this term, we will provisionally use it in the following general sense: *The integrations are direct impressions due to psycho-physiological processes of specifically central nature. In relation to the sensations they are secondary elaborations*[2], *and thus they seem to be expressions of certain functions peculiar to the higher centres of consciousness, an addition to our sensations.* In the introduction to his first article on integration Watt[3] points out that the intimacy of connexion between nerve-paths or impulses emanating from different sense-organs has been somewhat exclusively considered to consist in the mere coordination or association of afferent or efferent impulses with one another. "Sufficient attention has hardly been paid to the possibility that upon these afferent impulses an afferent structure might be raised which is dependent upon, but essentially an addition to these. To distinguish it from mere co-ordination such a structure might well be called integration." Such integrations are, *e.g.*, the perceptions of depth, distance and motion. Melody Watt[4] regards as auditory motion, *i.e.* an integration.

After this digression we will return to the perceptions of lines and supplement what has been said with some purely theoretical speculations. Let us picture to ourselves a dark rectangle on a white surface. Simultaneous contrast causes the rectangle to appear sharply defined against the white background. Its contour has a certain marginal function. If we further imagine that the short sides are steadily shortened while the longer sides, unaltered, approach one another, the rectangle becomes narrower and narrower until at last the rectangle has passed over into a straight line. An analogous view might, without difficulty, be applied to curves, but straight lines are from a practical standpoint more important.

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[1] Henry J. Watt, "The Elements of Experience and their Integration: or Modalism." This *Journal*, 1911, IV. 127–204.

[2] This was observed in Goldstein and Gelb's patient who had primary visual sensations but lacked the integrations built on these, *i.e.* perceptions of motion and contour.

[3] Henry J. Watt, "Some Problems of Sensory Integration." This *Journal*, 1910, III 323–347.

[4] Henry J. Watt, "The Elements of Experience and their Integration: or Modalism." This *Journal*, 1911, IV. 127–204.

With the perceptions of lines Bühler has associated the perception of magnitude and of direction. A complete description of a line requires, of course, these attributes; but nevertheless, they seem to be isolated in a degree which justifies an independent treatment of them. In connexion with the perception of proportion we will later on briefly discuss the perception of magnitude and of direction.

II.

In the perception of lines we thus see the effects of the integrative process that has been briefly described on the preceding pages. The resulting integration, the surface and the contour or the isolated line, underlie all perceptions of form. But form is difficult to isolate: it belongs to the whole complex. Just as we form and divide time in rhythm, so we construct our primary perceptions of space according to definite laws. This 'construction' gives them form. If we, therefore, try to understand the essence of form, we can do this only by finding its laws. These are most clearly formulated in the imitative arts. Accordingly we are led to the study of the laws of composition in painting and sculpture.

But we are also free to adopt another manner of proceeding. We may examine form, as it were, *in statu nascendi*, by showing some more or less formless[1] figures in order to observe according to what principles the construction takes place. We may take such experiments on persons of different ages in order to find out whether the laws are constant or variable in the course of individual development. We may, in other words, treat the problem from a psycho-genetic point of view. The present investigation offers an attempt in this direction.

In accordance with the principles set forth above, the experiments are intended to answer the general question, How do individuals in different stages of development construct figures composed of surfaces or lines?

The figures used (Fig. 1) were of three kinds: (i) Figures drawn to resemble some familiar object—A-figures; (ii) simple figures without any direct resemblance to familiar objects—B-figures; (iii) complex figures, drawn without any plan—C-figures.

The size of the figures was about 1—1·5 cm. They were drawn with black ink on unpolished glass-plates. The experiments were conducted by means of a tachistoscope, in a room with subdued light, the time of exposure was 100σ. Each exposure was preceded by the call 'ready,' which was to warn the observer to concentrate his attention and to gaze at the figure in the tachistoscope, dimly visible in the middle of the evenly dark plate. A somewhat shorter time of exposure was sufficient for adults while the children required about twice the time above mentioned. Where a longer time was necessary it was given by doubling the number of exposures.

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[1] The formlessness in this case is due to absence of similarity to familiar forms. Absolute formlessness it is, of course, impossible both to produce and reproduce.

The scanty accounts of mental experiences that can be obtained from people unused to introspection, require to be made up by some other means. In our case it seemed most natural to have recourse to drawings made by the observers. Thus they were instructed immediately after every exposure to sketch, as exactly as possible, the figure seen, and, after that, to describe their experiences. All the observers were warned beforehand that some of the figures might resemble or represent something, others not. Sometimes they were asked which had been the case. In order to avoid influencing the drawings by suggestion, this question was always asked after the drawings were finished. One or two initial exposures were given for practice. They were of figures belonging to the B- or C-group that did not resemble any object. The number of observers was 55, 37 schoolboys from 8 to 13 years of age, and 18 adults, chiefly students. The total number of exposures amounted to 527. Of the 45 figures some twenty were shown comparatively regularly. The others were shown to casual persons who were allowed from time to time to take part in the experiments. The experiments were begun towards the end of the spring term 1920 and finished in the autumn of the same year. I here take the opportunity of expressing my thanks to all those who have helped me in my work and to my observers. I also wish to acknowledge my obligation to Prof. Chr. Sibelius, whose tachistoscope I have used, and to Prof. Tigerstedt for permission to work at the Physiological Institute of the Helsingfors University. Further my thanks are due to the lecturer in psychology, Dr E. Kaila, for the untiring interest he has always shown in my work and for the guidance and advice he has given me; and last but not least to Dr C. S. Myers, of Cambridge, who has carefully read my manuscript and suggested numerous valuable corrections and improvements.

III.

We will first consider some of the A-figures. Of these only a small number were used, some unambiguous like 2 A, others ambiguous like 1 A. The unambiguous figures, some of which represent familiar geometrical forms, others animals, and 2 A a human face, were not very

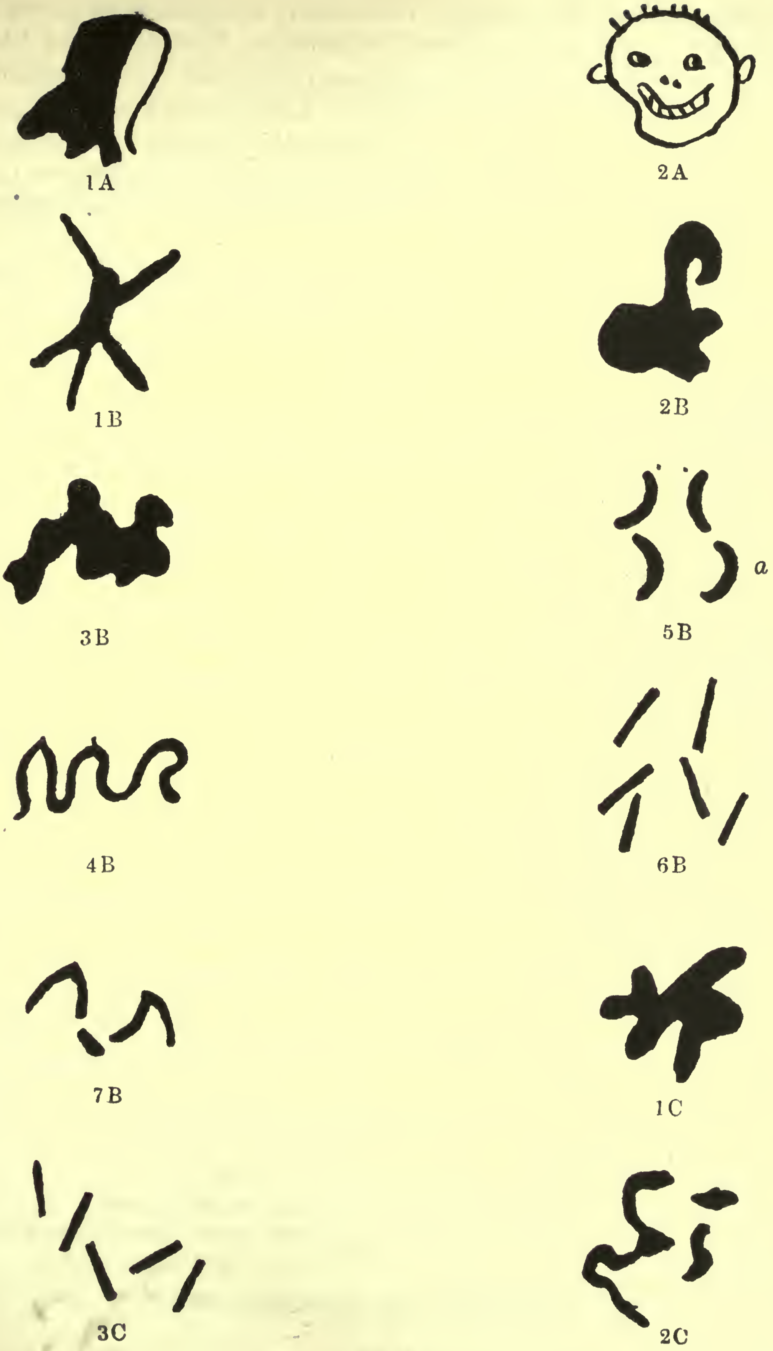

Fig. 1.

interesting. They were generally immediately recognised and drawn by the observers without any altered construction. The ambiguous 1 A is more interesting and will often be referred to further on. It is difficult, but not necessary, to draw any line of distinction between A-figures and B-figures. According to our definition the B-figures have no direct resemblance to familiar forms. Some of those most frequently used are reproduced here. It will be seen that the distinction between C-figures and B-figures is very uncertain.

I will now proceed to deal with the constructions of children. Some extracts from the records will serve to illustrate the experiments.—

A-figures.

Fig. 1 A. *Children of* 8–9.

Obs. H.R.a.[1] Exp. I. Nothing. Exp. 2. "It's a boot with a strip."

Obs. B.E.b. Exp. I. "It resembles something but don't know what."

Children of 10–11.

Obs. H.B.d. Exp. I. "It's an animal, a kangaroo." The observer had drawn an animal, and was afterwards asked if the figure had resembled anything.

Obs. H.S. Exp. I. "A bluebell."

Children of 12–13.

The figure is generally called a bluebell. Of all the children only one observed that the figure also resembles a human face.

Fig. 2 A. Immediately recognised even by the younger children.

B-figures.

Fig. 1 B. *Children of* 8–9.

Obs. H.R.a. Exp. I. "A star."

Obs. R.K.e. Exp. I. "It's a fly." Exp. II. "I saw many legs."

Children of 10–11.

Obs. J.H.f. Exp. I. "I could not quite see." Exp. II. The observer draws a symmetrical figure.

Children of 12–13.

With two exceptions all the observers draw symmetrical figures.

Fig. 2 B. *Children of* 8–9.

Obs. H.R.a. Exp. I. Nothing. Exp. II. "I saw a boot with a strip." Experimenter objects: "But then it must have been the first figure" (1 A). Obs. replies: "No, that was a top-boot."

Obs. A.W.h. Exp. I. "It was a duck swimming on the water."

Children of 10–11.

Obs. H.V.k. Exp. I.—Exp. II. "It looked like a water-cock."

Obs. B.J. Exp. I. "This is difficult." The observer was then asked if it resembled anything. "It was an elephant, but I am not quite sure." Generally the figure is called 'a bird.'

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[1] The small letters refer to the reproductions (Fig. 2).

Children of 12–13.

Obs. B.C.m. Exp. I. "I could see there was a tail." Exp. II. "Oh, what a lump. And the tail stuck up."

Obs. H.R. Exp. I. The observer was asked if there was resemblance to any object. Answer: "No."

FIG. 4 B. *Children of* 8–9.

Obs. A.W.h. Exp. I. "It's a snake that's crawling."

Obs. P.F. Exp. I. "I did not see quite well. Exp. II. on being asked, "It looked like a snake."

Children of 10–11.

Obs. G.N.r. Exp. I. "It looked like an 'm.'"

Obs. P.N. Exp. I. Draws a rhythmical line.

FIG. 6 B. The replies do not indicate any associations by similarity.

In the above we notice at once the predominance of associations by similarity. This is seen chiefly in the figures of surface, but associations by similarity occur in the linear figures also, if there is any suggestion in them. We seem then compelled to reckon with a suggestive factor—at least so far as children of 8–9 years old are concerned. It was difficult for them to understand the meaning of the word 'figure'; according to their idea the chief thing was to 'look for pictures.' Yet in my instructions I tried to make it clear that the figures did not necessarily represent anything, that some of them consisted only of lines and strokes. The figures were also shown to the children without the tachistoscope after they had made their drawings. Thus I tried to emphasise what had been said. In this way a possible suggestive factor, at least for children of 10–13 years old, must have been minimised.

Even if throughout the experiments on children we allow for such a suggestive factor, we are nevertheless obliged to admit that associations by similarity play a very important part in the children's constructions. They try to find similarities with visual images of a concrete nature. When they fail to do so, they cannot understand the figure (cf. Table I). A few simple schemata are exceptions to this rule.

The table cannot, of course, pretend to be absolutely exact. Very frequently the children were unable to state definitely whether they found resemblance or not; in other cases associations by similarity perhaps appeared only after the construction had taken place, thus without influencing it. Nevertheless we have taken into consideration both the children's statements and their drawings, and sometimes also the number of exposures, though this number has not been included in the purely schematic survey that the table is intended to give. The designation 'definite associations by similarity' indicates that the observer has been able to specify what object the figure represented or resembled, and that the drawings have been influenced by these associations. 'Doubtful associations by similarity' indicates that the observer has either been unable to tell precisely what the figure resembled, or that his drawing clearly represents some familiar object though he has not been able to say what it was. 'Perceptions of detail' means that the observer has either perceived some detail only or not apprehended the figure at all. All other statements and drawings are collected under the head 'Visual schemata.'

TABLE I.

	1 A			1 B			2 B			
Age	8–9	10–11	12–13	8–9	10–11	12–13	8–9	10–11	12–13	Total
Definite associations by similarity	3	9	8	4	3	2	5	7	1	42
Doubtful associations by similarity	3	2	0	0	2	1	1	4	0	13
Visual schemata	1	0	3	0	8	7	0	1	8	28
Perceptions of detail	0	2	1	1	0	1	0	1	2	8
Total number of experiments	7	13	12	5	13	11	6	13	11	91

	4 B			5 B			6 B			
Age	8–9	10–11	12–13	8–9	10–11	12–13	8–9	10–11	12–13	Total
Definite associations by similarity	4	6	0	0	0	0	0	0	0	10
Doubtful associations by similarity	0	2	0	0	2	0	0	0	0	4
Visual schemata	1	3	1	4	7	10	4	6	5	41
Perceptions of detail	0	0	0	2	0	1	2	0	1	6
Total number of experiments	5	11	1	6	9	11	6	6	6	61

Even a superficial perusal of this table makes it evident that the number of associations by similarity decreases with advancing age. If we consider 2B, we find that of 6 children of 8–9 years old 5 used such associations, that of 13 children of 10–11, 7 show definite and 4 doubtful associations. But of the children of 12–13 only one resorted to association, the others to general schemata. What has been said above holds good for the surfaces. As for the linear figures it may be applied to them also, when any hint of association can be detected in them. (See 4B.) But when an uncommonly simple schema was readily available, as in 5B and 6B, it was used. The latter has been considerably simplified before it fitted into the children's schemata. Some children did not apprehend the figure at all. We reproduce here some of the drawings. Associations by similarity occur in the following figures:

1 A.

9*a* A top-boot with a strip.
9*b* Not defined with precision.
9*c* A sitting man.
11*d* A kangaroo.

1 B.

9*a* A star.
9*e* A fly.
10*f* } Symmetry.
12*g* }

2 B.

9*a* A boot with a strip.
9*h* A duck on the water.
9*k* A water-cock.
11*l* A bird.

3 B.

9*o* A revolver.
9*p* A tree.

4 B.

9*h* A snake.
10*r* The letter n.
11*s* The letters rn.

This rather arbitrary selection gives a clear idea of the influence of the associations. A 'blot' with a curved projection conceals within it all kinds of possibilities. Thus 2 B is with different observers 'an elephant,' 'a duck' (9*h*), 'a bird' (11*l*), 'a boot with a strip' (9*a*), etc. The descriptions of 1 A vary still more. The observers have seen it as a 'mill with wings,' 'a kangaroo' (11*d*), 'a sitting man' (9*c*), 'a blue-bell,' 'a human face,' 'a top-boot with a strip' (9*a*). This last association clearly shows how accidental may be the circumstances that affect the children's constructions. It occurred first when 1 A was shown as 'a top-boot with a strip.' Then followed 1 B and then a ten minutes' pause. When 2 B was exposed the observer remarked spontaneously: "I saw a boot with a strip" (cf. the extracts from the records). Previous construction has obviously been influential in producing this idea. The difference between the two figures was nevertheless noticed by the observer; he expressly emphasised the distinction that 2 B was a 'boot,' while 1 A had been a 'top-boot.' This example shows that in the case of children the figures can be related to the very first visual image that makes its appearance in their consciousness. In the records we find many other instances where, in a similar way, an earlier construction has influenced a following one. The observer, *e.g.* who called 2 B 'a water-cock' (9*k*), had conceived 1 B as 'an insect,' drawing it in the same way as 9*e*, reproduced in Fig. 2, as 'a fly' with many legs. It seems natural to suppose that the legs are essentially the same as the jets of water radiating in all directions from the water-cock. 2 B was exposed after 1 B; it had only two short broad projections on one side. The long upper projection has been specially noted and drawn by the observer; it is probably meant to represent the water-pipe. It frequently happens that in the construction the same schema is used several times for essentially dissimilar figures. It almost seems as if in such cases the associations by similarity were less potent than the schema in question which happens to be active in the consciousness.

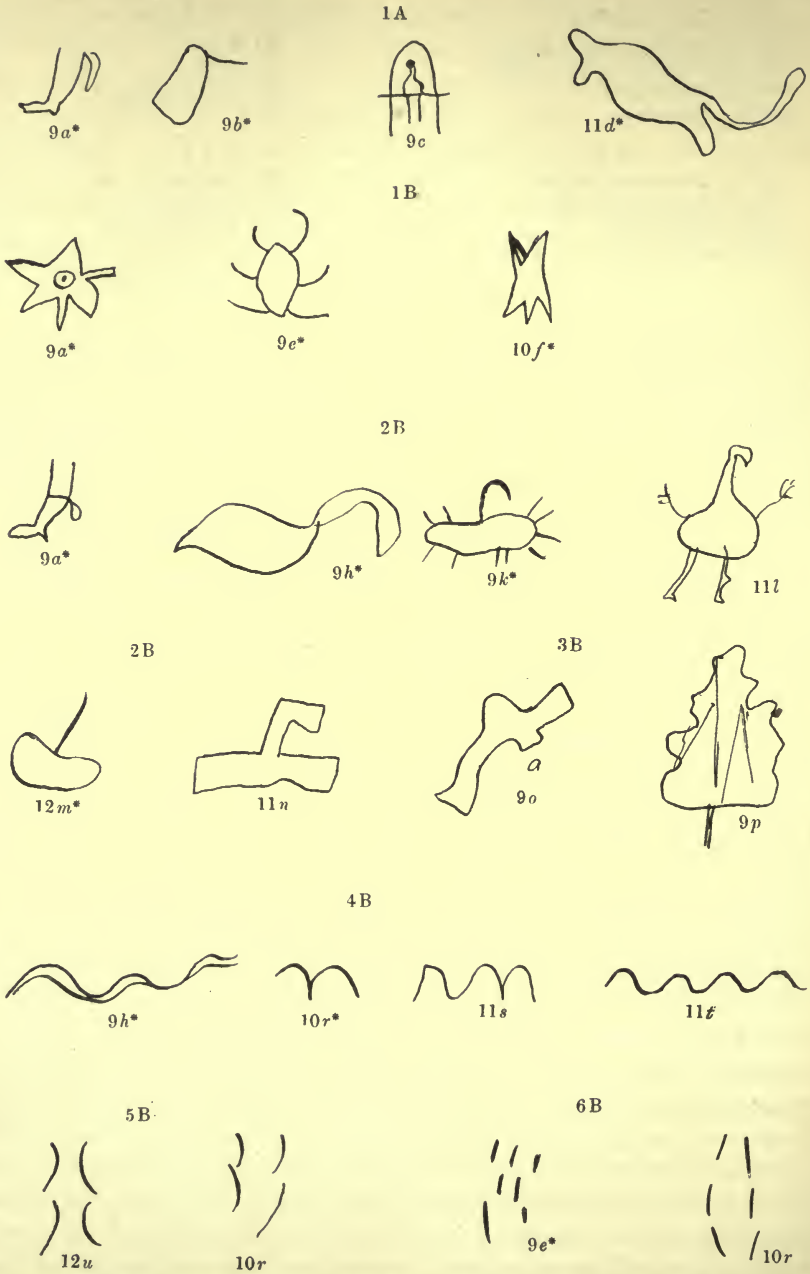

Fig. 2. The letters with * occur in the extracts from the records (pp. 230, 231). The numbers denote age.

'Chance' thus plays a very important part in the children's constructions, rough associations of similarity mediating the transition to images that are for some reason or other easily brought to consciousness.

We have so far used the term 'association by similarity' without specifying what kinds of similarity occur in the children's associations. Yet this must be done if we are to arrive at an accurate idea of the constructions. We must, of course, be able to indicate, if not all, at least some of the points of resemblance between the original sensations and the image created by the associations. If, for this purpose, we compare the figures exposed with the corresponding drawings, we find that similarity, for the most part, has no other source than some simple relation in connexion with a characteristic detail. Taken together, these form a very much simplified schema of the object in question. This fact I will illustrate with some examples. Let us consider 9*h* (Fig. 2), the drawing that represents 'a duck swimming on the water.' The relation here is very simple: A drawn out projection is curved so as to remind one of the neck of a water bird. And if we examine the 'water-cock' (9*k*), we find there the same relation somewhat more closely observed, but the lower projections have, through a casual recollection of the 'fly,' been completely transformed. In the same way we may in all the other drawings belonging to the same figure detect the same simple relation *plus* some detail; and we can be scarcely mistaken, if we say that the varying constructions are due to the fact that the associations are founded on a simple labile perception of relations which is easily adopted in regard to the various images at the moment readily brought into consciousness. The children's ideas of form depend on observations that are made without difficulty. It must, of course, at first, be rather difficult for them to observe and to remember the curvings and transitions of the lines. On the other hand the simple relations between the parts of the whole are comparatively stable and rapidly noted. When the child sees 'a blot' with 'the tail sticking up,' it also easily finds associations by similarity.

All this tallies perfectly with current opinions on the child as artist. According to Meumann[1] an early stage is characterised by the child not really drawing, but symbolising by means of a schema what it *knows* and not what it *sees* (p. 364). It interprets its experiences, not its sensations (cf. 11*l*). The next stage represents the first manifestations of 'the sense of lines and of form.' But strong reminiscences from the preceding stage still remain so that schematic indications are found in addition to the exact representations of the thing seen[1]. The schematic feature seems to be due to the predominance of the simple relations. A child's drawing of a man shows a small 'blob' on the top of a larger one, from which project two strokes, one on each side above, and two others below. The face is comparatively elaborate. Characteristic details are seldom lacking; buttons and spread fingers often recur.

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[1] E. Meumann, *Experimentelle Pädagogik*, Leipzig, 1907.

We have so far principally occupied ourselves with these two stages[2]. Something further may be said of the linear figures. 4B, 5B, and 7B (Fig. 1) are all composed of curves but none of them is quite regular. The last curve in 4B, the line *a* in 5B, and the concave line in 7B break the harmony of the arrangement. For the sake of comparison we are obliged to anticipate things and to emphasise the fact that, with adults, the irregularities are smoothed out and the figures made regular in accordance with the form suggested by them. Children behave somewhat differently. In 4B associations by similarity predominate (cf. Table I). The figures are not conceived as compositions of lines but as 'letters' or as 'a snake' (4B) or 'a flying bird' (7B). This is interesting because in such conceptions the development of our general ideas of form is seen more clearly than usual. We find that many children have the associations 'snake' and 'bird,' where others have pure perceptions of form, *e.g.* of two rhythmic lines. Where 4B is called 'letters,' there is apparently no perception of form. 5B is generally formed according to some schema or other, yet seldom along the lines of least resistance (12*u*), but more generally according to a simpler schema still, four lines curved in the same direction (10*r*); sometimes the children draw only two or four curved lines without plan. Often the figure is not understood at all (cf. Table I). 6B is, as a rule, regarded as an instance of a very simple schema. The most usual form is given in Fig. 2.

In the preceding exposition of our subject we have maintained a distinction between construction according to some schema and construction according to associations by similarity. Let us see how far this is justifiable. The continuous transition from such associations to visual schemata, as suggested by the materials collected in the table, forces upon us the supposition that we have here to do with a difference of degree and development, not of kind; the mechanism to which construction is due in adults, is, in children not fully developed. But its general mode of function is the same. By means of associations the child tries to retain certain simple relations that are familiar to it through earlier experiences. As the experiments show, these relations mostly give rise to images of a concrete nature. That the construction has not as yet become completely mechanised, is shown by the variety of the associations.

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[1] E. Meumann, *Experimentelle Pädagogik*, Leipzig, 1907, p. 365.

[2] It is difficult to draw any exact line of distinction, but it generally seems justifiable to place children of 12–13 on a level with adults—at all events this is so with children of 13.

As will appear later, schematic construction is also founded on similarity of relations. These are characterised by a greater uniformity. Different observers construct much in the same way and we are, therefore, able to detect certain principles of construction, common to all the schemata.

IV.

We now turn to the construction in adults. For the sake of clearness, we will first consider the linear figures. Below are reproduced three different formations of 6 B, illustrating three different principles of construction. Two are contained in the first drawing. The adult observer (i) gathers the scattered lines together into groups, and (ii) these groups are placed symmetrically in relation to an imaginary axis. The other two drawings show (iii) a grouping along single lines, the simplest schema conceivable, which is to be found in many different forms. The adult constructs figures 4 B and 5 B in regular formation according to principles we have already observed in certain children (*e.g.* 11*t* and 12*u*). In the case of 12*u* the figure is symmetrically divided into two congruent parts. In the case of 11*t* a number of similar parts is arranged in a continuous series, roughly symmetrically, two curves on each side of an imaginary central axis; although the figure actually exposed consisted only of three quite irregularly arranged curves. Three regular curves would, of course, also give symmetry, but in most cases the observer gives four. This seems to be due to the 'rhythm' of the

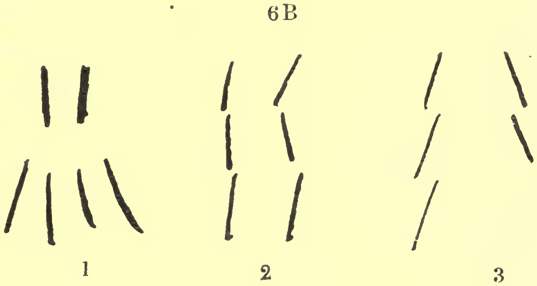

Fig. 3.

line. It suggests rhythmical motion, and where this impression is strong enough, the number of curves is found to be increased. Such expression of a feeling of rhythm is generally independent of consciousness. One observer however said, of his own accord, after a series of exposures, that he had often been uncertain as to the form of the figures, but had then taken refuge in rhythm in forming them. In this exceptional case rhythm was deliberately employed. Another observer also, in 5 B, paid attention only to the curvings of the lines and failed to notice their number, and so—after some hesitation—drew three long rhythmically curved lines instead of four bows. The same observer committed a similar error when drawing another figure not reproduced here. The figure 7 B, too, caused impressions of rhythm.

A few words on the nature of these impressions are desirable. For, as is well known, visual rhythm is a much debated phenomenon. M. K. Smith[1] (pp. 270–88), together with Meumann, has investigated visual rhythm, and their experiments indicate that it depends on motor phenomena. G. E. Müller[2] treats rhythm at some length (pp. 358–85), and concludes that there is no need to regard the motor theory of subjective rhythm as insufficient (p. 367). The motor elements of rhythm are often difficult to discover, as they may be based even on such phenomena as innervations of the larynx.

The impressions of rhythm which the lines made on our observers, I feel inclined to designate as forms of 'synaesthesia.' "Taste and odour of a picture of a fruit, sensations of cold, hard marble produced by the sight of a statue," Külpe[3] gives as examples of synaesthesia. It is true that we do not yet know much about these illusions, but the impressions of linear rhythm show an unmistakable affinity to them. A similar view seems to be held by H. Lundholm[4]. He distinguishes between marginal and motional functions in lines. The latter are due to a suggestive impression of motion, the importance of which is clearly shown by his definition of rhythm. "A rhythmical system of lines is one in which we perceive uniform motion, an arhythmical system of lines is one from which we cannot receive such an impression" (p. 57). The general aesthetic definition is "periodical repetition of an identity." Here then we have a general formula for all rhythm (directly inferred from its nature as a *temporal* phenomenon in the auditory and motor regions of mind) transferred to the visual region, where *space* predominates. It seems to me that the two definitions supplement one another. Lundholm one-sidedly lays stress upon the illusionary aspect of visual rhythm. Certain perceptions of lines are accompanied by impressions of rhythmic motion in the same way as perceptions of marble are accompanied by sensations of cold and hardness. But uniform motion cannot always be identified with rhythm. As a psychological and aesthetic term the word has a fixed meaning, which is comprised in the second definition, and must not needlessly be corrupted. Besides, Lundholm admits that "arhythmic systems of lines can, on repetition, become rhythmic" (p. 214).

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[1] M. K. Smith, "Rhythmus und Arbeit," *Philos. Stud.* 1900, XVI.

[2] G. E. Müller, "Zur Analyse der Gedächtnistätigkeit u. des Vorstellungsverlaufes I." *Ztschr. f. Psychol.* Ergänz. Bd. v.

[3] O. Külpe, *Vorlesungen über Psychologie* (Herausgegeben von K. Bühler). Leipzig, 1920.

[4] H. Lundholm, *Om objektiva faktorer i konsten*, Lund, 1919.

Jodl[1] thinks the unification of a multiplicity a fundamental law of the aesthetically effective; he distinguishes between eurhythmia and proportionality (p. 127), as its basic forms in the imitative arts. Eurhythmia comprises rhythm, symmetry, and culmination; proportionality includes particularly the law of the *sectio aurea*. Generally, proportionality prevails between the different parts of a whole if none of these parts asserts itself quantitatively at the expense of the others.

The materials already demonstrated essentially confirm this view of Jodl. Even primary forms of construction show a tendency towards unification. The lines are gathered into groups or lengthened into units placed in rows or symmetrically. Very often the symmetry is incomplete or only indicated. We do not find congruous parts on each side of a real or imaginary axis, but accumulations of lines round a centre. An example is given (Fig. 4), in which the upper figure is the exposed original and the lower the observer's drawing of it.

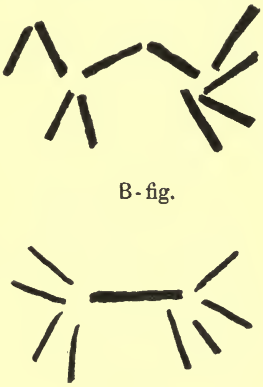

Fig. 4.

The row-formations show both simple and more complicated forms, complication being due to the more or less complete likeness between the elements forming the row. It culminates in impressions of rhythmic motion.

In the linear figures the laws of construction are expressed in the clearest way. Conscious associations by similarity do not occur so readily as in the case of figures of surfaces. But that they may occur is shown by the children's drawings of 4 B and 7 B. In other respects the children's drawings of the linear figures differ from those of adults chiefly through

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Fr. Jodl, *Ästhetik der bildenden Künste*, Stuttgart u. Berlin, 1917.

their greater independence of the laws of eurhythmia. Strict symmetry is comparatively uncommon; groups or whole lines are the commonest schemata. A complete absence of method in the designs is not uncommon in children, but it is rare in adults. As for surfaces, the more developed 'sense of lines' in adults shows itself chiefly in more exact reproductions. The power of observation in adults is better developed, and the capacity for concentration is greater. What has been said above of linear figures holds good, in the main, for surfaces also; construction shows itself in grouping around axes, in symmetrical arrangements, in divisions into congruous or similar parts; in short, in a general tendency towards unification. Where construction or assimilation does not take place, the figure is forgotten or not understood[1].

It is difficult to decide, on the basis of these investigations, to what extent proportionality asserts itself in the construction. The drawings were too simple. Even from a general point of view it is difficult to judge whether proportionality is present or not. Its rules cannot be so clearly expressed as, *e.g.*, those of symmetry. But the fact that proportionality works side by side with the other laws of composition makes it probable that in the construction of the primary impressions it is also involved. The two-fold validity of the laws of form as rules for aesthetic composition and transforming principles has been shown by the comparison just drawn with some of these laws, and can hardly be seriously doubted. It might be added that Bühler has given fairly conclusive evidence of the existence of a direct impression of proportion.

V.

Construction has been studied in our experiments with regard to the transforming influence it exercises on our primary impressions. A survey of the results we have arrived at can be divided into two parts. In the first place we will consider the contents of the general principles of construction recurring in different schemata, and secondly we will describe the formative process in terms of mental function.

I. We have found that construction can be defined, quite generally, as comprehension of new impressions of form, which, by means of associations by similarity, assimilate earlier mental experiences. Further we have found that the associations are chiefly concerned with relational resemblances. This we were able to observe already in children. As has been mentioned above it is naturally impossible for them to observe and to recollect the confused intricacies of the lines. If we have a mobile object, *e.g.* a man, the child can see its contour from different sides, and the lines become difficult to fix. The same holds good, though in a smaller degree, for immobile objects, the child itself being always more or less mobile. The relations between the parts of the objects, on the contrary, are comparatively unchangeable. Straight or approximately straight lines may be compared with these relations as regards constancy. The simple relations and the straight lines thus form a frame that recurs in almost every visual perception, and our experience soon teaches us to use them when we try to comprehend complex forms. The simple relations are in the main, of three different kinds: relations of resemblance, of direction, and of magnitude.

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[1] W. Poppelreuter, *Die psychischen Schädigungen durch Kopfschuss im Kriege 1914–16*, I. Leipzig, 1917. Also above (p. 231).

The impression of proportion, so carefully described by Bühler[1], belongs to the latter group. It is extraordinarily direct, but where great exactness has been demanded, Bühler's observers, when comparing the proportions of rectangles, have had recourse to the relation between the vertical and the horizontal side of the same corner (p. 178). Bühler himself does not take sufficient notice of this circumstance[2]. We are compelled to compare the proportions of the vertical and horizontal sides of rectangles almost at every step. It would be curious, indeed, if this did not take place with a certain directness. Besides, Bühler seems to overlook the fact that all perceptions of magnitude are not exactly similar. We must distinguish between (i) the perception of extent, which is due to factors such as the size of the retinal image and the idea of the greater proximity of the object and is of purely integrative nature, and (ii) the perception of relative magnitude, which is founded on a comparison of two objects. The impression of proportion belongs to the latter category. The fact, that we have an immediate impression of 'bigness' or 'slenderness,' must not induce us to regard it as belonging to the former class. This perception of extent, which, in accordance with Watt's terminology we will, at least provisionally, call 'integration,' together with the likewise immediate perceptions of direction and contour, is a *conditio sine quâ non* for all perceptions of form. But the construction depends on perceptions of relations. The C-figures proved very puzzling to our observers; generally they were not understood. Yet the figures have both direction, extension and outline. But the relations between their parts are not simple enough. The assimilation with earlier experiences, the construction, cannot find connexion with simple relations.

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[1] *Op. cit.*

[2] V. Benussi, "Die Gestaltwahrnehmungen," *Ztschr. f. Psychol.* 1914, LXIX., points out that Bühler's work is imperfect in so far as he does not explain the position of the relations in his investigation.

The most frequent relation of direction is that of horizontality and verticality. A psychophysical investigation of the same kind as the one Bühler has undertaken for the measurement of the impression of proportion would be interesting with regard to the impressions of horizontality and verticality also. The differential limen would probably show that our judgments of these relations are very exact. For the rest the same that has been said above of the impressions of magnitude holds good of the impressions of direction also; we must discriminate between absolute integrative and relative perceptions of direction.

Resemblance makes itself felt as a tendency to divide the complex into congruous or similar parts. (Cf. the construction into rows and symmetry.)

It is only natural that the straight line, being the simplest exponent of the integrative perceptions of extent, direction, and contour taken together, should play such a fundamental part in the formative process. The simple relations of magnitude, direction, and resemblance are also expressed in the clearest way by means of straight lines.

After this theoretical discussion of the perceptions of relation, we can return to the genetics of construction. We noted that the children did not so regularly as adults construct the linear figures according to the principles suggested by them. The process of construction is not, as yet, quite mechanised; the relations that offer themselves are accidental, founded on such earlier perceptions of form as, with a superficial relational resemblance to the new impression, unite strong reproductive tendencies. The numerous cases where the same schema recurs several times with new details added, are illuminative. Yet it seems more proper in these cases to speak of perseverative tendencies of the schemata in question. And in order to apply this schema in the usual way through associations by similarity, nothing is required but that the relations between the parts of the figure exposed should be approximately of the same kind. Associations by similarity seldom depend on a single detail only. In adults such a form of construction is particularly uncommon. This seems to depend, in some way, on the regularity just pointed out. In the case of surfaces the possibilities of finding associations by similarity are greater. If we compare such different associations with one another, we find that those belonging to the same figure are comparable only in so far as we keep to the relations. Of these the children judge fairly accurately if the relations are simple enough.

Although the construction in adults is, as we have seen, more dependent on the laws of eurhythmia, there is no reason to believe that the continuity of development has anywhere been interrupted and the construction essentially changed. For eurhythmia is nothing but a regulation of relations. Experience seems to have shown that some relations are more common than others. They have stronger tendencies to reproduction, and, if the relations between the parts of the new complex resemble them, an assimilation or transformation takes place. (Symmetry *e.g.* we see wherever we may happen to turn our eyes.) But the new perception need not of course, necessarily, show a complete formal congruity with an earlier one. In the former certain simple familiar relations are fixed which are corrected by the image. Thus arises the peculiar transformation, assimilation, or synthesis found in the construction. The simple relations, which we might with Meinong[1] call 'principal relations,' are the cause of the regularity of the construction. They are subjected to the laws of eurhythmia. The general law of construction might be briefly formulated thus; Construction consists in finding out, by means of associations of similarity, the relations between the parts of a complex.

In my opinion this law holds good in ordinary as well as in tachistoscopic conditions.

One circumstance of extreme importance for the psychology of relations and complexes remains to be touched upon. Can we consider the relational similarities to be direct and independent causes of reproduction, or do they act only in so far as they are contained in the form as a totality? This question makes it necessary for us to adopt a definite attitude with regard to the problem of the 'forms of synthesis.'

The characteristics of the 'form of synthesis' have been described in the clearest way by Gelb[2] and by Höfler[3]. Gelb identifies 'form of synthesis' with the sum of relations. Why introduce a new quality (*Gestaltqualität*) when it can be explained by means of phenomena already known? Höfler does not deny the importance of the relations but holds that the elements of the complex *plus* the relations do not represent a simple addition but a synthesis, the result of which is the form of combination. If I have rightly interpreted Meinong's strange terminology[4], he seems to be of much the same opinion. As Bühler remarks, the views of Gelb and Höfler are not absolutely irreconcilable. Meumann's and Albien's and our experiments show that the perception of a figure, or as Meumann expressed it, "the forming of a clear idea of a figure," requires a special process. We have added that this process consists in finding out the relations between the parts of the figure exposed and is of a peculiar nature. I am inclined to think that the result of this process, 'the form,' bears a characteristic synthetic stamp. This view is corroborated by Bühler's investigations into the perception of proportion. Moreover, Rubin's[1] careful study concerning the difference between the perception of a surface when the background is conceived as forming the figure, and when the figure itself is present to our consciousness, shows that the sum of relations composing a complex produces so peculiar a perception that a special term is wanted for the designation of it. For this we will use the simple word 'form.' The German term *Gestaltqualität* seems to me to have been employed too uncritically, the perceptions of motion, of contour, of extent, of magnitude, of proportion, etc. having, all without discretion, been called *Gestalten.* My own views as to the perceptions of motion and of contour have already been set forth. I have also tried to maintain a distinction between perception of magnitude and of extent, in accordance with my general opinion that 'form' consists in perceptions of relations which must be distinguished from certain integrative perceptions, as for instance those of extent, motion, contour etc. It is possible that some further distinctions will be necessary. Our definition of the integrations does not lay claim to other than provisional validity. It may be added that the integrative perceptions are more constant than the perceptions of form. Under certain conditions the perceptions of motion and contour are always produced, but form may vary with the relations that are fixed in each separate case. Four points arranged in a certain way—this example has become classical in the literature on the subject—may be perceived as a square, a cross, etc.

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[1] A. Meinong, "Ueber Gegenstände höherer Ordnung u.s.w." *Ztschr. f. Psychol.* 1899, XXI.

[2] A. Gelb, "Theoretisches über Gestaltqualitäten," *Ztschr. f. Psychol.* 1911, LVIII.

[3] A. Höfler, "Gestalt und Beziehung-Gestalt und Anschauung." *Ztschr. f. Psychol.* 1911, LX.

[4] Meinong calls contemplation "Fundierung idealer Gegenstände." These "idea objects" are the relations. By "fundierter Gegenstand" he means "form of synthesis."

As has often been emphasised by various writers we cannot observe all the relations at the same time, form being, as we have seen, founded on certain principal relations. By help of these we construct. My view with regard to the problem of form, as it has been developed above, justifies the following answer to the question asked on p. 32. As the perceptions of form must be allowed to possess certain 'complex characteristics,' it is not very likely that those associations of similarity that mediate the construction depend on resemblance of isolated relations; on the contrary the resemblances seem to bear upon the form as a whole where certain principal relations predominate.

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[1] E. Rubin, *Synsoplevede Figurer I.* Copenhagen, 1915.

Seifert[1] has pointed out that the 'need of construction' (*Gestaltungsdrang*) can be a real experience (pp. 70–71), especially, if the construction is in some way suppressed. In fact, the constructive process seems to take place with the automatism of a well-regulated mechanism; in case of any obstruction to its free course, our consciousness is immediately alarmed. Dr Kaila, as observer, remarked: "I almost feel how I construct. It is an extraordinarily concentrated intellectual process, an 'ideational coordination' that takes place. I cannot find any trace of volition."

We have shown that construction, from being a process in which a general law of association was clearly traceable, has become more and more stereotyped and has developed into a mechanism. This mechanism is governed by the same law, but its application has become so schematic that it admits of being limited and fixed by the formulae of eurhythmia. We are scarcely mistaken if from a greater regularity, psycho-physically we infer a greater mechanisation, psycho-physiologically. There are parallel phenomena in the motor regions of mind. There we know of a number of mechanisms that have been created in the course of individual development. These motor coordinations are conveniently adapted to certain stimuli. Both as regards purpose and development the process of construction seems to furnish analogies with them. Following E. Kaila[2], I will term it 'ideational coordination' (p. 23) in which is emphasised the functional resemblance to corresponding motor phenomena.

Similar views seem to be held by Poppelreuter[3]. In opposition to the theory of primary centres of sensation and secondary centres of perception he assumes the existence of "a great number of specific complicated mechanisms of perception" (p. 77)[4]. Throughout the whole of Thorndike's[5] psychology a corresponding line of treatment is to be found.

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[1] F. Seifert, "Zur Psychologie der Abstraktion u. Gestaltauffassung." *Ztschr. f. Psychol.* 1917, LXXVIII.

[2] E. Kaila, *Ueber ideatorische Koordinationen*, Helsingfors, 1917.

[3] W. Poppelreuter, *Die psychischen Schädingungen durch Kopfschuss im Kriege 1914–16*, I, Leipzig, 1917.

[4] He has also investigated such disturbances as may occur in this mechanism and arrives at the conclusion that the most usual abnormity consists in "a false reproduction of the relations of symmetry" (p. 134). This is in perfect harmony with our views as to the nature of Form.

[5] Edw. L. Thorndike, *Educational Psychology*, New York, 1915.

Man's habits and abilities as well as skill, character and temperament depend on original or acquired groups of connexion, *i.e.* mental functions, responding to situations or to elements of situations. A 'situation-response' of this kind is what we seem to have in the process of construction; we understand through schematisation.

As for the construction itself, it can also, by introspection, be distinguished from the primary impression. Thus Dr Kaila, on two separate occasions, was able to recognise a distinct interval between the sensation and the following construction. Very often the observers are conscious of the fact that their drawings differ considerably from the primary impression, which they say they have forgotten. But the contrary is not uncommon. The impression received may be so rapidly formed that the observers are certain that their drawings correspond essentially to the given stimuli, although, on comparison, great differences have been found. The interval between the sensation and the construction seems to have a correspondence in different localisation. This supposition is confirmed by some experiments concerning recognition.

For this purpose a set of figures was used which was comprised of drawings that showed a certain schematic resemblance to one another, although they were made to differ in details. The observers often said that they had seen the same figure two or more times in succession. Recognition is thus dependent on identification not of two sensations but of two schemata, *i.e.* constructed impressions. In the same way we must account for the fact that very characteristic figures are recognised even when turned upside down. On the other hand it is not altogether impossible that recognition may be based on identification of two sensations. On one occasion (I myself being the observer), when the same figure was shown for the second time, recognition took place immediately and with such force that parts of the figure which I knew ought to have been there did not appear at all and the drawing I made differed considerably from the previous one of the same figure. That the figures were identical, I felt quite sure, although it was impossible for me to give form to that primary impression which had caused this judgment. One comment which almost all the observers made, was that the primary impression is very soon forgotten. Perhaps also the construction is a kind of unconscious 'situation-response' by which we try to *retain* our impressions through formulating them.

In the same way as the motor coordinations leave room for individual differences, thus causing different persons to respond somewhat differently to the same stimulus, so also the ideational coordinations may vary within certain limits. For they are, as we have seen, suitable adaptations —which implies that they can be adapted to a certain extent.

Some observers are entirely dependent on the given stimulus. They construct little and cautiously and rather leave out unnoted parts than resort to construction. They often require more than one exposure and are still uncertain about the more complicated figures. On the other side there are observers who transform their primary impressions and treat the elements they have noted rather freely. Earlier experiences are used to a great extent. The result may sometimes be rather surprising. To this category belong most children[1] and some adults. Maybe we have here to do with degrees of construction. In the former case there is more observation than construction, in the latter *vice versâ*. The two types can, it seems, be identified with Messmer's objective and subjective types. Katz[2], who has made experiments with figures, has arrived at the same conclusion, but he, in place of the terms 'objective' and 'subjective' wishes to introduce 'peripheral' and 'central.' As a matter of fact, these terms show more adequately the dependence of the former type on the peripheral stimulus, and of the latter on central processes. The two types are exemplified in Fig. 3, 1 representing the central, 3 the peripheral type. In the former the observer has constructed the figure symmetrically, in the latter he has aimed at a more adequate representation of it.

It may prove of practical importance to maintain the distinction between these types. But as our experiments had not, in the first place, this conclusion in view, and as what has been said above is chiefly intended to be a corroboration of Messmer's, Meumann's and Albien's, and especially Katz's observations, it would need verification. But it may be maintained that we have not really to do with two essentially different types but rather with two different kinds of response to a stimulus; the same person may respond both subjectively and objectively. The former supposition however seems to me more acceptable. The instructions given to the observers directed them to make their drawings as exact representations of the figures as possible, and thus one might expect to find the peripheral response predominant. However, as in each observer we have been able to detect a comparatively constant tendency either towards the peripheral or the central response, it seems to me that there is an actual difference of type.

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[1] O. Messmer, "Zur Psychologie des Lesens u.s.w." *Arch. f. d. ges. Psychol.* 1904, II.

[2] D. Katz, "Ueber individuelle Verschiedenheiten bei der Auffassung von Figuren." *Ztschr. f. Psychol.* 1913, LXV.

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A CRITICISM OF WRIGHTSON'S HYPOTHESIS OF AUDITION.

BY H. HARTRIDGE.

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THE essential features of Wrightson's hypothesis which distinguish it from other displacement theories of hearing, such as have been advanced by Rutherford, Watt, de Toule and others, is the claim that there are impulse points in any train of sound vibrations, the time intervals between which approximate closely not only to those occurring in the individual tones which together set the air into vibration, but also to their summation and difference tones.

Wrightson claims that it is the recognition of these time relationships by the brain which causes the appreciation of the individual tones in a musical chord; the perception of overtones, harmonics, summation and difference tones being similarly effected.

The existence of these time relationships are postulated by Wrightson on the following evidence:

If three mathematical curves are prepared as shown in Fig. 3 of Wrightson's book one representing the sine curve of a musical note A, the second the sine curve of another note C, and the third the curve obtained by summing A and C mathematically, and if now a pair of callipers be set to the wave length of the note A and be then tried consecutively on each crest, trough, and crossing point of the compound curve, it will be found that the feet of the callipers will fit certain of the distances on the compound curve. For example on Fig. 3 (p. 26)[1] such distances are shown marked A_1–A_1, A_2–A_2, A_3–A_3, etc. In a complete cycle in one case three such distances are indicated, and in another case four. If now the whole process be repeated with either the wave length of C or with that of the summation tone, difference tone or harmonics of A and C, then in each case a number of coincidences will be found between the feet of the callipers, and certain distances between the crests, troughs, and crossing points of the particular compound sound-wave curve under examination. These distances on the curves represent time intervals, and it is Wrightson's suggestion that the brain in recognising these intervals causes us to perceive the different constituent tones which make up a musical chord.

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[1] *The Analytical Mechanism of the Internal Ear.* Macmillan, 1918.

Wrightson now goes on to state (p. 30) that no mathematical exactness is postulated between the time periods in the compound sound-waves and those of its constituents, but that they may be considered sufficiently accurate for the theory "as the ear, in judging of musical intervals does not depend upon absolute accuracy of vibration ratio, a fact well known to practical musicians." This want of exactness is readily detected in the diagrams which Wrightson gives, thus taking the first (Fig. 3) as an example, it will be found that the distance A_2–A_2 is smaller than the wave length of A while A_3–A_3 is larger.

Boring and Titchener[1] have pointed out that taking the above analysis at its face value the following criticism can be advanced against Wrightson's hypothesis:—(*a*) the summation and difference tones are both an octave too low; (*b*) in addition to the pure tone the ear should also hear the octave below it, which the ear does not appear to do. The reader will conclude from this that even if the fact is ignored that the mathematical analysis is not exact, it still further prejudices itself by leading to false deductions. The proof must be now given that it is the analysis itself which is at fault.

Supposing that the compound curve in Fig. 3 be again turned to and that the callipers be set not to the wave length of either A or C, but to a purely arbitrary distance anywhere between these two values, then if this distance be laid off as described above it will probably be found that it is several times repeated in one cycle. Thus I took a definite series of lengths between 2 and 21 millimeters and counted the number of times that each was approximately in coincidence with distances between troughs, crests or crossing points in half a cycle of the compound curve shown in Fig. 3. The results I obtained are shown in the table below.

Arbitrary length	No. of coincidences in ½ cycle	Arbitrary length	No. of coincidences in ½ cycle
2 mm.	1	12 mm.	1
3	3	13	3
4	2	14	3
5	1	15	3
6	3	16	1
7	1	17	3
8	4	18	2
9	1	19	3
10	2	20	1
11	3	21	1

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[1] Boring and Titchener, *Amer. J. of Psychol.* 1920, XXXI, 101–113.

By averaging the number of coincidences in the above table it is found that any of the arbitrary lengths tested were represented on the average 2·1 times per ½ cycle.

The following table shows the number of coincidences which Wrightson found for the wave lengths of the notes.

Note	No. of coincidences in ½ cycle
A	1½
C	3
2 × *A*	3
2 × *C*	1½

The average number of coincidence works out at 2·25. From the coincident distances which Wrightson marks in his diagram I should exclude (*a*) A^3–A^3 as too large and (*b*) the 2nd C^2–C^2 as too large and this would bring the average down from 2·25 to 2 which is very nearly the value that I found, viz. 2·1, for purely arbitrary distances. Since these arbitrary distances are on the average represented with the same frequency as those which are related to the constituent tones in a chord, with their summation and difference tones, it is hard to understand how the organ of hearing can pick out the pertinent distances and exclude all the others which are equally represented.

I next counted the number of coincidences between some arbitrary lengths and the curve compounded of four different musical tones shown in Fig. 7, p. 29 of Wrightson's book, and compared the average value with that obtained for Wrightson's constituent tones:

Arbitrary length	No. of coincidences per ½ cycle
15·1 mm.	3
21·1	3
26·1	2·5
29	2·5
31	2·5
31·5	3
Average	2·75
Wrightson's lengths	
C	3
E	3
G	2
B	1·5
Average	2·4

The above figures show that as before arbitrary lengths are represented at least as often as those which Wrightson regards as essential to his theory. This, at first sight, astonishing result was partly explained by finding that in the compound curve in Fig. 3 the total number of distances in one cycle varying between 1·5 mm. and 44 mm. in length was approximately 400. Assuming that all distances between 1·5 and 44 mm.

are equally represented each distance would vary from the next by approximately ·1 mm. If then we assume an inaccuracy of setting of say ·1 mm., then any arbitrary distance between 1·5 mm. and 44 mm. will be represented 3 times (one exact fit, one ·1 mm. too large and one ·1 mm. too small) on the average.

Thus we see the explanation of Boring and Titchener's remarks. "It is strange," they say, "that the theory, if we take the diagrams at their face value should thus insist on proving itself. Moreover there is something disconcerting about a theory that will come right on any assumption." This mathematical foundation, which Wrightson had laid for his theory, therefore fails since the brain cannot be expected to pick out certain time intervals and construe them as musical notes, when apparently all time intervals between certain limits are equally represented.

A further test of the basis of Wrightson's hypothesis was now made by combining together three sine curves of the wave lengths 12, 16 and 24 cms. and plotting graphically the compound curve thus obtained. From this graph the rough positions of the maximae, minimae and crossing points were ascertained, and using these values as guides their exact positions were found by calculation. The distances between the different crests, troughs and crossing points in the compound curve were now tabulated between the limiting values of 0 and 48 cms. and were found to number 169 in all. Of these 111 fell close together into 23 groups, there being on the average 4·84 lengths in each group, the average divergence of any length from the mean length of its group being under 1 per cent.

These groups of distances were found to have the following mean values in cms.: 3·67, 5·93, 8·89, 11·01, 11·95, 14·23, 21·69, 22·73, 25·26, 26·35, 27·79, 29·33, 30·36, 31·3, 32·66, 33·58, 36·10, 37·10, 39·10, 40·75, 42·00, 43·30 and 45·00. Now the wave lengths of the tones originally combined for the basis of this calculation were 12, 16 and 24 cms., these should have difference tones of 4, 8 and 12 cms. and summation tones of 28, 36 and 40 cms. All these lengths according to Wrightson should be represented by groups of distances in the compound curve. When however the mean group values are compared with these lengths, it will be observed, that two of the lengths only are represented, viz. the 12 cms tone by a group at 11·95 cms., and the 36 cms. summation tone by a group at 36·1 cms.

There are therefore 6 tones not represented as they should be, and of the groups present 21 should not be there and would represent tones inharmonic with those that should be present; these unnecessary tones would be also in nearly every case inharmonic with one another. If then the brain recognises the presence of these groups and interprets them as tones, as Wrightson supposes, nothing but discord could result.

It would appear that this criticism, taken in conjunction with that advanced by Boring and Titchner, removes from Wrightson hypothesis almost all those features which differentiated it from the telephone theory of Rutherford, and it therefore falls under the criticisms advanced against the older theory. The supposed mathematical basis of Wrightson's hypothesis proves to be illusory.

(*Manuscript received* 4 *July* 1921.)

SOME EXPERIMENTS ON AESTHETICS.

By LYNETTE FEASEY.

(*From the Cambridge Psychological Laboratory.*)

I. *Aim of the Investigation* (p. 253).
II. *Series I:*
  (*a*) *Methods* (p. 254).
  (*b*) *Results: influence of colour and surface; Bullough's 'perceptive types'; the psycho-galvanic response* (pp. 254–258).
III. *Series II:*
  (*a*) *Methods* (p. 258).
  (*b*) *Results: the 'picture' and 'pattern' types; relation to Bullough's types; additional types; influence of colour and form; the psycho-galvanic response; the emotional basis of the aesthetic judgment* (pp. 258–270).
IV. *Summary of results* (pp. 270, 271).

I. AIM OF THE INVESTIGATION[1].

THESE experiments were originally undertaken with a view to discovering whether the basis of the aesthetic judgment could be rightly said to be emotional, by means of a comparison of the aesthetic judgment with the emotional reaction evoked by the object on which that judgment was passed. The psycho-galvanic reflex was used to measure the emotional reaction. The results obtained by this comparison were, however, entirely ambiguous and it was chiefly the incidental results that proved interesting.

Two series of experiments were carried out: in the first, simple rectangles were used; in the second, arrangements of geometrical figures. Twenty subjects took part in the first series, and twelve in the second. All were adults, the majority being women of undergraduate standing at Cambridge.

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[1] The writer wishes to express her indebtedness to Mr F. C. Bartlett who not only suggested the problem but also gave constant help and advice throughout.

II. SERIES I.

(a) *Methods.*

The materials used throughout this series were five rectangles of the following sizes (the size of the base being given first): rectangle 1, 4″ × 3″; rectangle 2, 3″ × 3″; rectangle 3, $2\frac{1}{2}$″ × 3″; rectangle 4, $1\frac{15}{16}$″ × 3″; and rectangle 5, 1″ × 3″.

These five rectangles were used in each of six experiments. In Exps. 1, 2 and 3 the *outlines* of the rectangles only were used. In Exps. 4, 5 and 6 rectangular *surfaces* were used. In Exps. 1 and 4 the rectangles were all white. In Exps. 2 and 5 the rectangles were coloured but were all of the same colour, a pale green. In Exps. 3 and 6 the rectangles were of different colours: (i) dark brick red, (ii) very dark grey, (iii) unsaturated blue, (iv) orange, (v) pale green. Thus Exps. 4, 5 and 6 repeated Exps. 1, 2 and 3 respectively, except that rectangular surfaces were used instead of the outlines only.

Two methods were used in each experiment. In method I, the subject was given the five rectangles and she arranged them in order of preference; in method II, the rectangles were exhibited one at a time and the subject was asked to say whether she liked it or not; the emotional reaction to each rectangle being measured by means of the psycho-galvanic reflex and compared with the judgment on the given rectangle.

In order to avoid a mere repetition by the subject of the judgments passed, each subject was not asked to carry out all the experiments by both methods. A subject who performed Exps. 1, 3 and 5 by method I, performed Exps. 2, 4, and 6 by method II.

(b) *Results.*

Subjects naturally differed widely in their preferences, as also in the decidedness of their preferences. One or two were unable to state any preferences at all, and others experienced considerable difficulty in doing so, particularly when the rectangles were uncoloured. Others had no hesitation whatever. The general order of preference may be found by totalling for each rectangle the numbers which give the position of that rectangle for each subject. Then, since 1 means that the rectangle is placed first, the rectangle with the lowest total will take the first place in the general order of preference. The order of the rectangles is found to be: 1, 4, 2, 5, 3, *i.e.* the rectangle 4″ × 3″ is liked best, and the rectangle $2\frac{1}{2}$″ × 3″ least.

This order is the order for all the six experiments. If, however, we consider the first three experiments alone we have the order of preference for the rectangular outlines, and can compare this with the order for the rectangular surfaces, as found from the last three experiments. These two orders are:

for the outlines:—4, 1, 2, 5, 3;
for the surfaces:—1, 4, 2, 3, 5.

Thus though there is a difference in the order according as the outline or the whole surface is given, this difference is not a very marked one: rectangles 1 and 4 are in both cases preferred to rectangle 2, and rectangles 1, 4 and 2 to rectangles 3 and 5. The fact, however, that there *is* a difference, is supported by the subjects' introspections. When the outlines were replaced by the surfaces, one subject, for example, said "They give a totally different impression now."

We find a very much more strongly marked difference between the order of preference when the rectangles are coloured and that when they are all white, or all of the same colour.

Exps. 1 and 4 (all white)	give the order	:—	1, 4, 2, 5, 3.
Exps. 2 and 5 (all same colour)	,,	,,	1, 4, 2, 3, 5.
Exps. 3 and 6 (different colours)	,,	,,	1 and 5 (equal) 3, 4, 2.

In Exps. 3 and 6 rectangle 2, which was placed third in Exps. 1 and 4, takes the lowest place, owing to its dark grey colour, and rectangle 5 is liked best because its colour is liked. These results seem to indicate a much greater interest in colour than in shape in these experiments: the combination of colour with the shapes being able completely to upset the order of preference assigned to the shapes alone. This greater interest in the colours than in the shapes is also shown in the introspections. Many of the subjects remarked that they were much more interested in the colours than in the shapes; and one said "You do not think of the shapes when the colours are there." By many of the subjects the reasons given for the order assigned to the rectangles in Exps. 3 and 6 refer almost entirely to the colours: and the coloured rectangles appear to be treated simply as specimens of certain colours. There are, of course, exceptions. Some subjects really do consider both colour and form in fixing the order of preference, and make such remarks as that "dark colours are better for big things." One or two allow their preferences for the shapes to outweigh their colour-preferences, but these are decidedly in the minority.

Rectangle 4 has, approximately, the proportions of the Golden Section rectangle. It is of interest, therefore, to notice that it takes a high place, though not the highest, in the order of preference, except in Exps. 3 and 6 where the shape was not liked sufficiently well to outweigh the dislike of its orange colour. If this rectangle had been placed 'the other way on,' so that the breadth was greater than the length, it is quite possible that it would have taken a still higher place. Indeed, several of the subjects remarked that they would have liked it better so placed. If this is so, the popularity of the Golden Section rectangle cannot be due to the mere physical proportions of the lines.

The remarks made by the subjects during the experiments, and the reasons given by them for their preferences, show that the four 'perceptive types,' which Mr Bullough found in his experiments on the aesthetic appreciation of colours and colour combinations[1], and Dr Myers in his experiments on listening to tones[2], appear also in the aesthetic appreciation of simple shapes such as rectangles. Examples of the four types are as follows (a subject may be said to belong to a certain type when she gives a preponderance of judgments of that type):

(i) *Objective.* It seems possible to make three subdivisions of this class—

(*a*) The subject may merely criticize the proportions, the rectangle being described as neat, compressed, straight, too long and narrow, regular and so on.

(*b*) The rectangles were frequently judged according to their usefulness for certain purposes, for example, for picture frames. Then we have such judgments as that the rectangle is "quite useless" or "the best shape for a photograph."

(*c*) Mr Bullough points out[3] that appreciations of the objective type "appear to be special cases, of a large class of appreciations based upon what might be called 'conformity to type.'" The object is judged by some preconceived "type or standard which the subject has set up for this class of thing." Certain judgments occur which seem to be explicitly based on this principle: such judgments, for example, as "it looks more like a rectangle," "it is definitely a rectangle," "it is a convenient orthodox shape."

(ii) *Subjective.* Here the subject's attention is directed towards herself and her own feelings rather than towards the object, the rectangle.

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[1] This *Journal*, 1908, II, 406–463.

[2] This *Journal*, 1914, VII, 68–111.

[3] *Loc cit.* 451.

(*a*) Sensual: *e.g.* "it is 'trying' to look at," "it gives you a cramped feeling."

(*b*) Affective: *e.g.* the rectangle is characterized as monotonous, undisturbing, too familiar, or as appealing to the imagination.

(iii) *Associative.* The associations which determine the judgments may be either

(*a*) Impersonal, as "it is like doors in a doll's house," "like fortress windows, it suggests the Middle Ages," "too like a Tate sugar box," or

(*b*) Personal, as "it is too geometrical, the kind of thing we always had to work out problems with at school." One subject disliked the square because "I never could draw squares." Another liked a rectangle because it suggested the frame of a particular picture which was valued.

(iv) *Character.* Judgments of this type are rare but a few do occur. This type appears in such judgments as "it looks generous," "it is a generous sort of creature," "it suggests liberalness," "horrid mean little thing." One subject gave as her reason for disliking squares that "squares have no individuality of their own," and added "it is much easier to be regular than to show individuality." The same tendency to treat the rectangles as if they were persons appears in such remarks as "it looks as if it were trying to be a square," "it looks as if it were pretending to be a square."

Most subjects give judgments of more than one type; but, roughly speaking, subjects tend to give judgments of the same type for both colour and shape in these experiments. For example one subject who gave some character judgments on the shapes such as "generous" and "mean," described the colours as "opulent," "fierce," "wild," "downright." Another who described the shapes as neat, compressed, etc., characterized the colours as dark, dull, brilliant, and so on.

The comparison of the emotional reaction with the aesthetic judgment, gave, as was said above, quite ambiguous results. In only four of the twenty subjects was there any suggestion of a correspondence between the amount of the galvanometric deflexion and the degree of aesthetic appreciation. These four subjects did indeed show a rough correspondence. The chief difficulty of the method, assuming that the psycho-galvanic reflex really expresses the amount of the emotional reaction, is the practical impossibility of knowing how much, if any, of the emotion registered is the effect of the given stimulus, and how much should be assigned to simultaneous stimuli. All that we can say, then, is that while in certain subjects there is some evidence of a correlation between the aesthetic judgment and the emotional reaction, in the majority of cases there is no evidence whatever of any such correlation and no conclusion whatever can be drawn as to a possible emotional basis of the aesthetic judgment.

III. SERIES II.

(*a*) *Methods.*

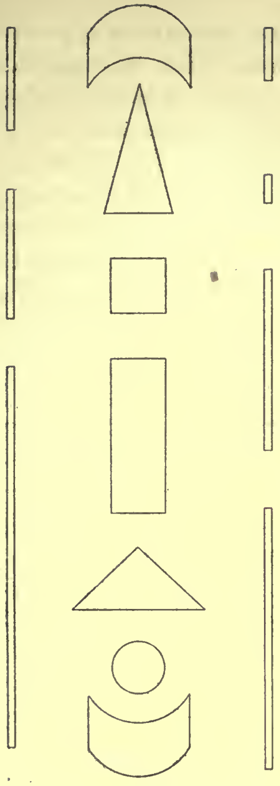
In the second series of experiments a number of geometrical figures, a circle, a square, lines, triangles, etc., were used. The figures were cut out in tin and two sets of them were used, one set of fourteen figures being uncoloured while the other set of fourteen were coloured by means of differently coloured papers stuck on to the tin.

Again two methods were employed. Method I was constructive: each subject was given first one set, then the other set of figures, and asked to arrange them on a piece of grey cardboard 18″ × 12″, in the way which she thought looked best. Thus 24 different arrangements were made, two by each subject. In method II the galvanometer was again used. The arrangements made by all were shown to each subject and the judgment passed on each of the 24 cards was compared with the emotional reaction to the same card. The subject was also asked which of each set of cards, coloured and uncoloured, she liked best. Nothing was said by the experimenter as to the presence of the subject's own two arrangements among the cards, but if the subject herself made no remark, she was asked at the end of the experiment whether she had recognized her own card.

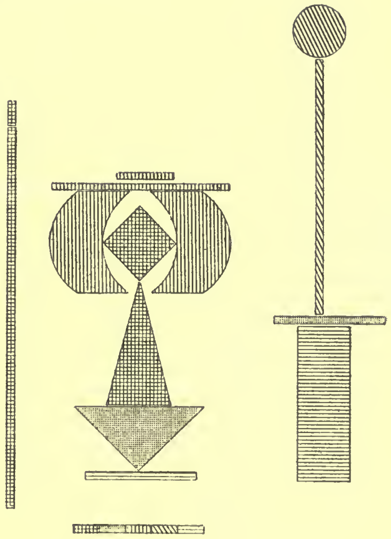
(*b*) *Results.*

The most interesting result was the demonstration of two quite distinct attitudes on the part of the subjects towards the cards. Some subjects made purely formal geometrical arrangements of the figures, aiming solely at symmetry and balance; while others arranged the figures so as to make a picture of an object or scene, such as a church, a flower, a village scene. The former will be called subjects of the 'pattern-type,' and the latter, subjects of the 'picture-type.' The clearest example of the pattern-type is Subject V, and of the picture-type Subject VI; cards made by these two subjects are reproduced as *a* and *b* on the opposite page.

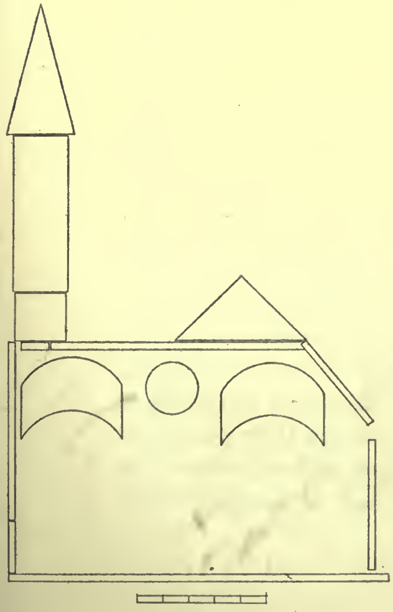
A merely casual inspection of the cards suffices to show the presence of these two types. Almost all observers, for example, recognize at once the different types to which Subjects V and VI belong. But it is not safe to trust entirely to the cards to show to which type a particular



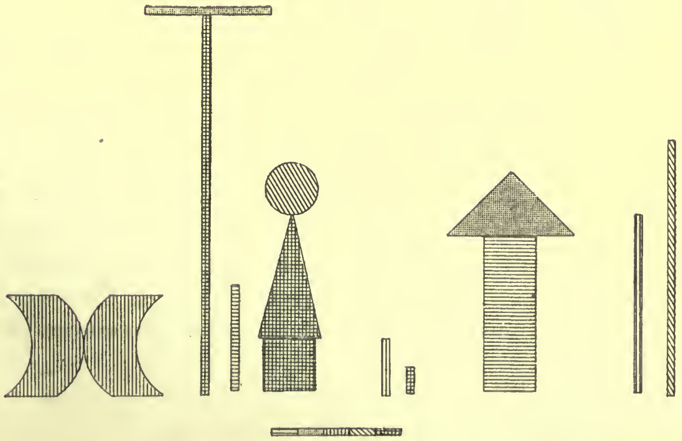
a



c



b



d

subject belongs, as whether any card appears as a 'pattern' or a 'picture' depends partly on the observer's own type. The subjects' introspections provide a more reliable guide. One or two quotations may be given. The following remarks were made, during the construction, by one subject clearly of the pattern-type—"Symmetry, I suppose, is the idea. I will have something rather large in the centre to place the other things round. By way of contrast I will have these two triangles here. I wish the big triangle had been made more symmetrical. I will put a big line here to take away the feeling of its being unsymmetrical. I am trying to get a central axis down the middle." Some subjects of this type did, indeed, aim at some particular design, for example a diamond shape, but this was much less common.

In striking contrast is the following quotation from one of the picture-type subjects: "I am trying to construct a pattern in my mind; I am thinking of it in terms of architecture. These are two domes: it is a piece of Moorish architecture and the lines are the slender columns. I am not thinking of it abstractly as a figure but definitely as some fragment of a building, a gateway or something like that. Yes! it's going to be an absolutely bizarre Oriental gateway. Now it's part of an Indian temple: the figures are mystical signs that had to be worked in in an arbitrary way and this is how they were worked in. It's a cabalistic door."

Some subjects, of course, did not work out the idea in so much detail; *e.g.*, one merely said, "I tried to make the whole look like a flower." As one would expect, the subject's interests, permanent or temporary, often played a large part in determining the object constructed. The subject from whom the above quotation is taken, for example, showed all through the experiments an interest in the East and finally told the experimenter that she was going to India the following year. Another subject, who made a picture of a flower, was interested in botany and so, as she said, "the coloured figures suggested flowers at once."

Of the twelve subjects who took part in this series of experiments, three were definitely of the picture-type, four were equally definitely of the pattern-type, while the other five belonged less definitely to one or other type or were of different types in the construction of the two cards. The two types differed somewhat in their methods of construction. Pattern-type subjects had certain definite principles which guided them in the construction: they were therefore surer of what they wanted to do. Subjects of the picture-type, on the other hand, did not follow such principles and therefore seemed more uncertain how to begin. Usually they made one or two random arrangements of a few of the figures until by chance one such arrangement suggested an idea, after which the construction went on much more confidently. Subject VI said, "I started with no idea: then the top gave the idea of a steeple and then I tried to make it more realistic." When given the coloured figures, she said, "I can't get any inspiration at all for this one." The change from uncertainty and hesitation to confidence and enjoyment when at last the 'inspiration' came was most marked.

The two types are found not only in the construction experiments, but also in the observation of the arrangements made by others. Whether any given card appears to the observer as a pattern or a picture depends on both objective and subjective factors: it is determined partly by the actual positions of the figures, and partly by the observer's own type. Ten of the twelve subjects, including some who were most definitely of the pattern-type, took Subject VI's uncoloured card to represent a church. On the other hand, the fact that one of the subjects did not even see this card as a picture of a church shows the difficulty which a pattern-type subject may find in realising the picture-type attitude. This subject merely said, "The person who made this seems to have had some sort of idea in her head, a factory or something like that." Again, one card which was variously interpreted by subjects of the picture-type as "a silhouette over a garden wall with a tower, some trees and the moon," and as "a village scene, with a sign post and a church," was intended by the subject who made it to be a formal design. One picture-type subject, on being told that this card had not been intended to represent anything at all, exclaimed, "But the black is so obviously a church!"

Two examples may perhaps be given (*c* and *d* on page 259) to illustrate the different judgments passed on the same card by subjects of the two types:

(i) *Card X* (*c*) *Coloured.* Pattern-type. "Yes, that's rather nice. The colours are nicely balanced and the proportions are good. It is nicely placed on the paper."

Picture-type. "I was rather disturbed by that. It is a flower in a pot with a cheeky young flower at the right showing how much taller it is. There are no leaves. The black is a measuring rod."

(ii) *Card XII* (*d*) *Coloured.* Pattern-type. "It is poor. The lump of red knocks you down absolutely and there is nothing whatever to balance at the other end."

Picture-type. "I was awfully tickled by that and made a long story about it. The short lines are children. It is Noah and his children. The red is a huge butterfly out of the Ark. It is most interesting."

Subjects belonging very decidedly to one type often tend to like only those cards which seem to them to conform to their type, and sometimes even to regard that type as the only reasonable one. One picture-type subject condemned all the cards which she could not interpret as pictures as "lacking in imagination." Similarly Subject V, who is markedly of the pattern-type said of Subject VI's uncoloured card, which she saw as a church, "That is ingenious but awfully stupid really." Of the same subject's coloured card she said, "I don't like it as much as the very regular ones, the proper sort of thing." But this is not invariably the case. A subject may see most of the cards as patterns and yet like best the few which she does see as pictures, while another may see most of the cards as pictures and yet she may regard these merely as amusing and like the patterns best. Most of the subjects, however, decided at once that the cards fell into two classes and were prepared to judge each class on its own merits. One subject when asked which class she liked best said, "It is difficult to say; they are so very different."

Generally speaking, a subject who was of one type in construction was of the same type in observation. There were, however, some striking exceptions. Subject VI, for example, was of the pattern-type in observation.

In the first series of experiments it was found that the four perceptive types demonstrated by Mr Bullough in the aesthetic appreciation of simple colours and colour-combination and by Dr Myers in the appreciation of tones appear also when simple shapes such as rectangles are used. With the more complex material used in the second series it is not possible to place all the judgments under one or other of these four heads. It seems necessary to recognize seven classes of judgments.

(i) *Objective judgments.* These are judgments based on such principles as balance, order, regularity, symmetry etc., *e.g.* "nice and symmetrical," "badly balanced," "too spread out," "uneven," "too scattered." In this class are found also judgments depending on the presence or absence of unity; *e.g.* "there is no correlation between the parts," "all the parts fit in together," "nothing has anything to do with anything else," "it is all connected up."

In the objective type, again, are found judgments referring to the usefulness of a design for certain purposes, *e.g.* "that would be a nice design for a jumper," "that would make a jolly border for a nursery."

(ii) *Subjective judgments.*

(*a*) Sensual, *e.g.* "that makes me feel cool," "it gives you an airy feeling because of the spaces," "it makes me feel queer."

(*b*) Affective, *e.g.* "dull," "uninteresting," "it disturbs me," "worrying," "disappointing," "disturbing," "exciting," "amusing."

(iii) *Associative judgments.* As with all kinds of material, associations play some part in determining the aesthetic judgments passed on the cards. Examples are "that's like a goal post, it reminds me of games," "that is a rough sketch of a village scene, it called up a village scene at home," "the top part reminds me of a lantern I once saw at a dance."

It might perhaps be suggested that whenever a subject is of the picture-type the judgments must be held to be determined chiefly by associations, as they are determined by the objects suggested by the shapes. But it seems scarcely possible to regard the fact that subject VI's uncoloured card suggested a church to most of the subjects as a case of association any more than one attributes the fact that a drawing of a church is recognized as such to association. It is often difficult to decide whether a judgment such as "That's awfully jolly. It's just like a pigeon cote," is to be called an associative pattern-type judgment or a true picture-type judgment. Everything depends on whether the card is taken to be really a picture of a pigeon cote, church, or flower, or whether it is really regarded as a formal arrangement of figures which suggests other objects. Examples such as "That's a rough sketch of a village scene: it called up a village scene at home," show that picture-type judgments are not necessarily associative, but may themselves call up associations, just as a drawing of a church may suggest a church one knows. Judgments such as "it reminded me of a weather-cock," appear to represent a transitional stage between the pattern and the picture-types.

(iv) *Character judgments.* Judgments such as "quaint," "it is daft, it looks flippant," "ambitious," "strange," "queer," "prosaic," "ordinary," "stupid," "it hasn't much character," "it has absolutely no character, it is typically stupid," "it is striving after peculiarity," are judgments of the kind we pass on people and therefore represent Mr Bullough's 'character type.' In this class also we must put judgments attributing personality not to the card but to the objects supposed to be represented, *e.g.* "it is a flower in a pot with a cheeky young flower at the right showing how much taller it is."

(v) *Judgments referring to the artist.* Judgments of the character-type are closely related to judgments on the person who made the card. It is not always easy to decide whether the judgment "it is stupid" or "ambitious" really means—"the card is stupid or ambitious," or whether it means—"it was stupid or ambitious to make a design like that." Undoubtedly they are in many cases true character judgments. There are, however, many judgments which explicitly refer to the artist, *e.g.* "they have tried to make it regular but the pieces wouldn't admit of it," "that is a neat way out of the difficulty," "it is careless; the pieces are merely thrown on as if it were done in a hurry." There are frequent commendations of the designs as "cleverly done," "very ingenious," "very original," and many references to success or failure of the artist's aim, *e.g.* "it is a failure," "it is muddled-looking, it hasn't achieved what it set out to do," "it has achieved something, made something quite obvious what it is," etc. Judgments of this class pass over into character judgments when the adjectives are transferred from the artist to the card.

The occurrence of so many of these judgments referring to the artist probably finds an explanation in the conditions of the experiments. Each subject had first to construct two designs herself and was usually very anxious to see those made by the other subjects. Hence when these were shown to her, her attention was liable to be drawn from the card to the person who made it, particularly where the subject was much impressed by the card. Such judgments appear, then, to represent a social factor in aesthetic judgment.

(vi) *Judgments referring to the suggestiveness of the design.* There are a number of judgments which may be classed together, which seem to have affinities with more than one of Mr Bullough's 'types,' *e.g.* "very imaginative," "very suggestive," "it stimulates the imagination," "it makes one think of something else directly," "it suggests all sorts of quaint things," with the corresponding negative judgments, *e.g.* "distinctly lacking in imagination." These are obviously related to the association type: a design is commended because it suggests "a lot of quaint things," etc. Differently expressed they approach either the subjective- or the character type judgments; the judgment "it stimulates the imagination" resembles the subjective judgment as the subject is chiefly concerned with the effect of the card on herself, while the judgment "imaginative" may quite well be a true character judgment.

(vii) *Judgments referring to the presence or absence of meaning.* Lastly, there are a number of judgments referring to the presence or absence (usually the absence) of meaning in the design: *e.g.* "that doesn't mean anything," "it doesn't convey anything," "the bottom part looks so curious stuck down there, it has no meaning at all," "I don't like that; it's meaningless. The others either represent something or are definite designs that could be used for something," "I cannot see any reason in it at all." These judgments are not confined to subjects of the picture-type: they are not condemning a design merely because it does not represent some concrete object. One subject said, "I don't like them where they don't convey anything." When asked whether this meant she only liked the pictures she replied, "Oh no! They may convey something even where they are not pictures. I don't like them when they are untidy." This reference to 'untidiness' suggests that the absence of meaning may underlie judgments of dislike based on the absence of unity, order, balance, etc. The presence of meaning appears to be implied in the appreciation of designs which stimulate the imagination or suggest ideas. Although, somewhat to one's surprise picture-type subjects very rarely make any reference to meaning, positive judgments of this type seem to be implied very frequently: designs which appear to be pictures are usually liked best because they are more readily appreciated as having meaning. Many subjects indeed appeared to think that the fact that the card represented some object was a quite sufficient reason for liking it. Thus these judgments referring to meaning probably do not represent a distinct class of judgment but merely make explicit an essential condition of any object's being liked. Anything that thwarts the 'effort after meaning' is disliked.

Summarising, we may say that the same four types found in the aesthetic appreciation of colours and tones and simple forms occur also in the appreciation of more complex designs, but that all the aesthetic judgments cannot be put under one or other of these four heads. We must recognise three other classes of aesthetic judgments: those referring to the artist, which represent the social factor in aesthetic appreciation and are related to the character type; those referring to the suggestiveness of the design, which have affinities with the associative, subjective and character judgments; and those referring to the presence or absence of meaning which appear merely to make explicit what is implied in all the judgments[1].

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[1] In his paper already referred to, Dr Myers makes some important elaborations of the four perceptive types suggested by Mr Bullough and it is of some interest to consider how far the present results are in agreement with those of Dr Myers. Dr Myers recognises three subdivisions of the *intrasubjective* type (Mr Bullough's physiological or subjective type) viz.

(i) judgments referring to changes in the cognitive aspect of the subject's consciousness, This classification of the aesthetic judgments cuts across the division of the judgments into pattern-type and picture-type: that is, one cannot say that a subject who belongs to the objective type (*i.e.* gives a preponderance of objective judgments) will be necessarily either a pattern-type or picture-type subject. The table given below shows roughly the relations between the two classifications. As it is not always easy to say definitely to which type a subject belongs, the judgments, not the subjects, have been classified; and the table shows what percentage of pattern- and picture-type judgments fall under each of the seven heads given above. A judgment is called a 'picture-type judgment' when it is passed on a design which is regarded as a picture. A judgment condemning a design *merely* because it is a pattern and not a picture is, however, called a negative picture-type judgment.

The most noticeable feature revealed by the table is the fact that while 63 % of the pattern-type judgments are objective, only 11 % of

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(*a*) leading him to describe the tones as strident, velvety, etc.;
(*b*) exciting definite tactual, motor or organic sensations.
(ii) judgments referring to changes in the emotional state, mood or feeling attitude of the subject.

Corresponding to these two subdivisions of the intrasubjective type are the judgments classed above as respectively 'sensual' and 'affective.'

(iii) judgments referring to changes of a conative nature in the subject's consciousness as seen in
(*a*) impulses, *e.g.* to turn the head;
(*b*) the endeavour to determine the meaning or use of a sound.

Very few of judgments of this type were found in the present experiments. One subject, however, said that a certain rectangle made her "want to scream" (cf. (iii) *a*); and judgments of type (iii) *b* seem to be implied in such statements as "I can't make that out at all," "I can't think what the red thing can be."

Dr Myers also recognises under the *objective* type
(i) the appreciation of the object as having meaning or use;
(ii) the consideration of the sound in relation to the subject's standard of purity, pitch, etc., which a satisfactory sound should attain.

This latter type of judgment is definitely included in the objective type in Series I and though not occurring in a definite form in Series II seems to be implied frequently in judgments which are based solely on such principles as regularity, *i.e.* the subject seems to think that a design is satisfactory if it attains a certain standard of regularity.

Judgments based on the appreciation of the object as useful for certain purposes have in the present experiments (as also by Mr Bullough) been included in the objective type. Series II, however, suggests that judgments based on the appreciation of the object as having meaning (using 'meaning' in its widest sense, and not as equivalent to usefulness) are not confined to the objective class and they have therefore been taken to constitute a separate type (type 7). I understand that Dr Myers now regards judgments depending on the appreciation of the object as having meaning or use, not as a subdivision of the objective type, but as a distinct type to which he gives the name of the 'pragmatic' aspect.

the picture-type judgments are objective. The class claiming the largest proportion of the picture-type judgments is the subjective. Judgments referring to the 'suggestiveness' (type 6) we have seen to be closely akin to the subjective- and character type judgments, whereas character type judgments are nearly related to the subjective judgments; indeed Mr Bullough regards them as a further development of subjective judgments. If, then, we may be allowed to group together types 2, 4 and 6, the respective proportions of subjective and objective judgments would be as follows:—

TABLE I.

Type	Pattern %	Picture %
1. Objective	63·1	11·1
2. Subjective	17·1	37·1
3. Associative	1·1	3·7
4. Character	10·2	17·3
5. Referring to Artist	5·3	14·8
6. Referring to Suggestiveness	—	13·6
7 Referring to Meaning	3·2	2·4
	100	100

TABLE II.

Type	Pattern %	Picture %
Objective	63·1	11·1
Subjective	27·3	68·0

If under the term 'subjective' we had included judgments referring to the artist (which usually are closely related to the character judgments), we should have had a still more striking result. These results seem to justify the statement that the pattern-type is pre-eminently an objective type, while the picture-type is pre-eminently of the subjective (or character) type. There are, of course, exceptions: *e.g.* Subject V, quite definitely a pattern-type subject, gave several character judgments, describing the patterns as "stupid," "lacking in individuality," etc. Further, because picture-type subjects tend to give judgments of the subjective or character type, we cannot assume that their judgments are necessarily of higher aesthetic value; the pure subjective judgment appears to be of lower aesthetic value than the objective, while the character judgments are of higher aesthetic value. The pattern- and picture-types are perceptive not aesthetic types, and it is not easy to decide which of the two attitudes is the more nearly related to the true aesthetic attitude.

In the first series of experiments it was found that the subjects had a much greater interest in the colour, than in the form of the rectangles; colour-preferences quite outweigh the preferences for shapes. It is interesting, therefore, to find that with the more complex material used in Series II the relative importance of colour and form is reversed. This is shown both in the construction of the cards, and in the observation of those made by others. In the construction, where a satisfactory arrangement of both colour and form is found impossible, it is the colour scheme that is sacrificed. This is even more markedly so with picture-type subjects than with pattern-type subjects, as is shown by the introspections. Picture-type subjects make very little reference at all to the colours. In the observation of the designs made by others the same feature occurs. An inspection of the remarks passed by all the twelve subjects on the twelve coloured cards reveals only 33 references to colour, many of these being quite incidental remarks, and practically all of them also accompanied by remarks on the shapes. Of these 33 references to colour, only six are picture-type judgments. A card may be taken to represent "a village scene," "a pagoda," "a silhouette over a garden wall" with no reference to the colour scheme and apparently with little or no thought of the improbability of the colours. For picture-type subjects form is even more coercive than for pattern-type subjects.

In Series I the shapes were so simple that they were apt not to be considered at all, the coloured rectangles being regarded merely as specimens of certain colours. With increasing complexity of the material, form appears to become increasingly important and we may conjecture that with still greater complexity, for example in the case of pictures, form would assume still greater importance relatively to colour.

Subjects of the two types show, as we should expect, a somewhat different attitude to colour. Pattern-type subjects aim at balance and harmony of colours and usually effect a compromise between symmetry of form and harmony of colour. They make such remarks as "The black is the background as it clashes least. I tried to help out each with the others by putting black between." Subjects of the picture-type, where they considered the colour scheme at all, aimed not so much at harmony of colours as at a realistic representation of the object. When given the set of coloured figures—in which the circle was green—one of them said, "I shan't be able to make anything out of this, I'm afraid; they've made the sun green." Subject X, for example, in her coloured design, is not so much concerned with the question as to whether black and green go well together as with the fact that the green would do for a plant and the black for its pot. The coerciveness of the form for subjects of this type appears in the fact that even if the colours had been interchanged, *i.e.* if the line had been black and the rectangle green, the line would still have had to represent the plant, and the rectangle the pot.

The use of the two methods in this series of experiments, the method of construction and the method of judgment, made it possible to attempt to discover what influence, if any, the fact of construction has on aesthetic appreciation . Since it appears to be a matter of everyday observation that one tends to be partial to what one has made oneself, we might expect a subject to tend to pass a more favourable judgment on those designs which she has made herself than on the others. It is somewhat surprising to find that only one of the twelve subjects liked her own cards best, though others remarked that they had "a certain affection" for their own, or that "if it had been anyone else's" they should have "detested" it. These remarks show a tendency on the part of some subjects to feel a partiality for their own designs merely because they *are* their own: but this tendency is not sufficiently strong to make them like their own best. On the other hand a subject may not like her own at all; we find such remarks as, "That's mine, I don't think much of it at all," "that's my hideous thing," "mine doesn't compare at all well with the others." All the subjects recognized their own cards, some giving big emotional reactions (as measured by the galvanometer) to them; but there is no clear evidence that the fact of having constructed a particular design necessarily determines a favourable aesthetic judgment on that design, although in some cases it certainly does so, to some extent at least.

In the first series of experiments, as stated above, no evidence was found of a correlation between the aesthetic judgment and the emotional reaction, as measured by the psycho-galvanic reflex, except in four of the twenty subjects. In Series II there was a similar absence of correlation. This absence of correlation is sufficiently well indicated by the fact that in the large majority of cases the deflexion shown by the galvanometer for the card which the subject liked best was *less* than the mean deflexion for the set: in no case was it markedly above, and in many cases it was very markedly below the mean deflexion.

This absence of correlation between the deflexions (representing the emotional reactions) and the aesthetic judgments cannot, however, be taken to *disprove* the emotional basis of the aesthetic judgment. In the first place, the difficulties of the method were such as to render it impossible to conclude that there really is no correlation between the aesthetic judgment on a stimulus and the emotional reaction to that stimulus. In the majority of the subjects it is quite impossible for the experimenter to tell how much, if any, of the reaction is a reaction to the particular stimulus exhibited, and how much is due to simultaneous stimuli, material or mental. Many of the subjects were quite obviously reacting to other stimuli, large deflexions taking place between the exposures of the cards, and not only were these apt to mask the reactions to the given stimuli, but they emphasize the fact that the deflexion which appears to be the reaction to the given stimulus may really be partly, at least, due to simultaneous stimuli quite without the range of the experimenter's observation. These practical difficulties appear to force one to the conclusion that the psycho-galvanic reflex, useful as it may be for diagnostic purposes, is not at present a suitable method for exact investigation.

Secondly, even supposing that it were safe to assume that the deflexion shown by the galvanometer accurately recorded the emotional reaction to the given stimulus, we could not conclude, from the absence of correlation, that the aesthetic judgment was not based on emotion. The judgment might be determined not by the *amount* of the emotion aroused by the object, but by the quality of the emotion: it might, for instance, be based on one particular emotion,—the so-called 'aesthetic emotion' may actually exist.

Yet another alternative appears to be open. It may be that the aesthetic judgment sometimes has, and sometimes has not an emotional basis. For example, Subject X, one of the most reliable of the subjects, that is, one about whom the experimenter could be reasonably sure that the deflexion showed the reaction to the given stimulus, gave big emotional reactions to certain cards which suggested Eastern objects, and these same cards evoked very favourable judgments; *i.e.* the emotional significance of the card for the subject seems to have determined a favourable aesthetic judgment. On the other hand, she gave no reaction at all to two cards which she liked very much indeed but which did not suggest Eastern objects and so had not the emotional significance of the others. The results obtained in the first series of experiments would not be opposed to the view that in the case of some individuals the aesthetic judgment is based on emotion while in others it is not; and the results obtained from Subject X in Series II at least suggest that the aesthetic judgment may sometimes be determined by the emotional significance of the object and sometimes not. It is at least a possible alternative left open by the results of these experiments that there may be both an intellectual and an emotional aesthetic judgment. Such a view would be opposed to the view that there is a peculiar 'aesthetic emotion': it would hold that there is no one condition which an object must fulfil if it is to be judged 'beautiful'; a position of some interest for philosophical aesthetics. Certainly all experimental aesthetics bears witness to the markedly different ways in which an object may appeal to different individuals and yet be liked or judged beautiful by each: for example the difference between the objective and subjective or character types, and between the pattern- and picture-types; for it is difficult to assert confidently that any one type is the only one to reach the true aesthetic attitude. If it could be shown that sometimes the aesthetic judgment has, while sometimes it has not an emotional basis, it would be interesting to know which type, objective or subjective or character, pattern or picture, gives emotional aesthetic judgments and which intellectual. But on this question, of course, the present experiments throw no light at all.

IV. SUMMARY OF RESULTS.

1. The order of preference for coloured rectangles differs markedly from that for uncoloured rectangles, a fact which points to a greater interest in colour than in form where the material is simple. There is a similar difference where rectangular surfaces are substituted for outlines but not nearly so marked.

2. The Golden Section rectangle holds a high place where the rectangles are uncoloured, but its popularity is not sufficiently great to outweigh the dislike of its colour when the rectangles are coloured.

3. The four 'perceptive types' which Mr Bullough demonstrated in the aesthetic appreciation of simple colours and colour-combinations and Dr Myers in the aesthetic appreciation of tones, appear also in the aesthetic appreciation of simple forms.

4. The chief result of the experiments with arrangements of geometrical forms is the demonstration of two distinct attitudes, that regarding the designs merely as formal arrangements of figures, and that regarding them as representations of objects or scenes. Subjects adopting these attitudes have been said to belong to the 'pattern-type' and the 'picture-type,' respectively. These two types appear both in the construction of the designs and in the observation of the designs made by others. The pattern-type appears to be more objective and formal, the picture-type more subjective and imaginative.

5. In the observation of the designs, whether a particular design

appears as a pattern or a picture is determined both by objective and subjective factors.

6. Mr Bullough's four 'perceptive types' occur also with the more complex material, but all judgments cannot be brought under one or other of the four types. Three other classes are found, viz. judgments referring to the artist, those referring to the 'imaginativeness' of the design, and those referring to the presence or absence of meaning.

7. Roughly it may be said that the majority of pattern-type judgments are of Mr Bullough's 'objective' type, while the majority of picture-type judgments are of his 'subjective' or 'character' type.

8. With the increasing complexity of the material, form assumes an increasing importance relatively to colour. This is more pronounced in subjects of the picture-type than in subjects of the pattern-type. The two types differ somewhat in their attitude to colour, the one aiming chiefly at a realistic arrangement of the colours, the other at harmony of colour.

9. The fact of having constructed a particular design does not necessarily determine a favourable aesthetic judgment on that design.

10. No evidence was obtained to support the view that the aesthetic judgment is based on emotion: there was no general correspondence between the aesthetic judgment and the emotional reaction as measured by the psycho-galvanic reflex. In view of the practical difficulties of the method, particularly that of knowing how much of the reaction is due to the given stimulus and how much to simultaneous stimuli, the results must be considered ambiguous. Moreover it is not impossible that the aesthetic judgment may depend not only on the amount but also on the quality of the emotion, and that there may be intellectual as well as emotional, aesthetic judgments. The practical difficulties of the interpretation of the deflexions suggest that the psycho-galvanic reflex, useful as it may be for diagnosis, is in its present form, at least, not suitable for exact psychological investigation.

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LEFT-HANDEDNESS AND THE REVERSAL OF LETTERS.

By L. G. FILDES and C. S. MYERS.

I. *Experiments* (pp. 273–276).
II. *General results* (pp. 276, 277).
III. *Conclusions* (pp. 277, 278).

I.

The brief series of experiments here described was conducted by one of us (C.S.M.) in 1913 on a markedly left-handed boy, whom we will call E., between six and seven years of age. His maternal grandfather, his maternal uncle, and a son of that uncle are also left-handed. The object of these experiments was to throw light on E.'s peculiar use of, and apparent preference for, the 'reversed' (*i.e.* mirrored) forms of letters and numbers. At the time E. knew some, but not all, of the letters of the alphabet, and all the ten unit arabic numbers. He had just begun to be taught to write with his right hand; but he found the greatest difficulty in doing so, owing to the coarseness and the want of co-ordination of the movements of that hand. For these reasons, and especially because he began to stutter, he was soon allowed to write with his left hand only; whereupon the confusion of the positions of letters and the disturbance of speech rapidly disappeared.

The experiments are obviously incomplete, but in view of the present interest in the subject[1], their publication seems justified. They fall into three main groups, each group dealing with a special aspect of the general inquiry. The aim of each of these groups, and the results obtained therefrom, will be described in turn. Finally, the general results and conclusions suggested by the whole of the observations will be summarised.

Group I. The experiments in this group were planned to discover how far the presentation of numbers and letters (*a*) in the 'ordinary' position, (*b*) 'reversed' (mirror fashion), and (*c*) 'inverted' (upside-down or recumbent), affected E.'s ability to read them. With this object, on

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[1] Cf. H. Gordon, "Left-handedness and Mirror Writing..." *Brain*, 1920, XLIII, 313–368.

March 25, the following series of numbers (E. took a natural interest in arithmetic) was shown him:

6 1 *2* 4 *6* 7 9 *3* *5* 2 *4* *7* 8 *9* 3 5

of which those here printed in italics were drawn 'reversed.'

All were named by him with equal ease; no delay or hesitation being perceptible in naming the reversed numbers in the series. The numbers ordinarily written and in their reversed positions were described by E. as "the same, turned different ways." He could, however, be induced to *choose* the rightly placed numbers, except in the case of 5; here he preferred the reversed form and considered it 'right.'

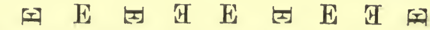
Later (June 17) four series of *letters*, differing in length, were shown, in the ordinary, reversed and inverted positions, and the reading times were taken. These times are given in seconds, in the following table:

Number of letters in series	4	4	10	24
Ordinary position of letters	3	4	10	34
Reversed ,, ,,	3	3	11	34
Inverted ,, ,,	6	5	—	—

Thus, the recognition of letters, like that of numbers, was effected with approximately equal ease whether the material was presented ordinarily or in reversed form. Indeed, E. once remarked that "it is easier to read in the glass"! Inversion of the letters made recognition more difficult; this difficulty was felt by E. who observed: "The upside-down ones are the hardest."

Group II. The aim of this group of experiments was to ascertain how often the 'right' position would be chosen visually in the case of certain letters of the alphabet, already fairly known—b, C, D, E, h, J, K, L, S—when each of them was shown to him in different positions. For this purpose E. was given practice from March 29 to April 3, in learning to draw them with his left hand and to name them, until he could easily recognise them in their ordinary positions. At the same time two 'artificial' letters, one of which was called 'goo,' and the other 'ree,' were similarly taught him.

On April 4, May 27 and June 4, E. was required to "choose the most correct letter" in different lines, each showing a letter in varying positions, as, for instance, in the line following:



On April 4 he repeatedly and consistently chose the reversed forms of b, h, and 'ree,' as being the 'right' positions. Later, however, on May 27 and June 4, after first selecting the reversed forms of E, C, D, S, J, K, L, he chose both the ordinary and reversed forms of J, S, C and E. These forms, ordinary and reversed, were termed by him "the same thing." Indeed, he seemed quite unable to detect any difference between the two positions (normal and mirrored) of E or D, until it was suggested to him that "one looks one way, the other the other." Even then he could not decide which way these letters *ought* to look, so that he finished by saying: "I should choose both." Usually the inverted (the upside-down and the recumbent) letters were rejected; he called the recumbent forms of K and E, and , "upside-down."

On April 6 he was asked to write the letters h, 'goo,' b and 'ree' with his left hand (*i.e.* as they were learnt); he wrote h and 'goo' correctly, l for b, and reversed 'ree.' He considered l the "same" as b[1]: "it only needs a —." His reason for reversing 'ree' is interesting: the drawing was begun correctly, but was altered to the mirrored position, because "it *looks* better; all the other letters face this way."

On June 4, the letter Z, previously unknown to E., was correctly learnt by drawing it blindfold with guided left and right hands, alternately. On the following day, he showed the same confusions as before; specimens of Z, whether ordinary, reversed, or recumbent, were "all zeds," when he was asked to make a visual choice; though ultimately, on his being pressed to be careful, he rejected the recumbent (which had been his first selection) as being an 'en.' Moreover, after E. had correctly learned the unseen form of an 'artificial' letter (known to him as 'shoo') on June 4, 6 and 14, by repeatedly drawing it blindfold with the guided left hand, the *reversed* form was *visually* chosen several times on June 20 as the most correct, whereas on June 15, without practice since the previous day, he had drawn the *correct* form *blindfold* both by the left and by the right hands. So, too, the drawing of Z was *reversed* on the introduction of the *visual* factor: E.'s *blindfold* drawing of it on June 5 was Z both with the right and with the left hands, but he drew it *reversed* with either hand *when his eyes were open*[2].

Group III. An attempt was now made to ascertain how far E.'s tendency to visual choice of the reversed position of a letter was affected by the method of learning. With this object ten letters were taught him, (*a*) two letters, visually, (*b*) three letters, visually and with right-hand drawing, (*c*) three letters, visually and with left-hand drawing, (*d*) one letter, blindfold and with right-hand drawing, (*e*) one letter, blindfold and with left-hand drawing, until they could be reproduced correctly—blindfold, in the case of (*d*) and (*e*)—and their names were known.

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[1] In the case of b and h, he was taught the hand-written, not the printed, form.

[2] In learning 'shoo' (which consisted of four short lines, each at right angles to the preceding one) by blindfold drawing, evidently E. first mastered the number rather than the direction of the successive movements. Thus, on June 6, before the day's practice, he reproduced it as four lines successively at right angles, but making a letter quite different in form from that taught him.

This group of experiments was carried out from September 17 until October 5, various letters being practised every day; and on September 23, 25, 27, 28, 29, October 3 and 4, certain tests were interpolated. These tests were similar to those used in the experiments of Group II. At each test E. was asked to pick out the 'right' letters, pointing to his choice.

Unfortunately the results cannot be formulated quantitatively inasmuch as (i) most of the letters used had been at least partially learnt before the experiments began, and no precise information is available as to the methods or the effects of such learning; (ii) the number of repetitions needed for learning differed according to the letter; (iii) too few experiments could be made with methods (*d*) and (*e*) to have any value. But so far as they go, the data indicate the distinct (from 25 to 50 per cent.) superiority of method (*a*) over any of the other four methods, and the superiority of method (*b*) over method (*c*). That is to say, visual choice was more correct after purely visual learning (though even here there were 30 per cent. of wrong choices), and was more adversely affected, in the direction of preferred reversal, by left-hand than by right-hand drawing practised with the eyes open. Blindfold drawing led rather to confusion of upright with recumbent positions.

Two points, observed during these tests, are noteworthy. (i) In choosing the letter C (practised blindfold with left-hand drawing), E. was uncertain of the 'right' position, until he was allowed to run with a pencil over the variously placed letters in the test. With the aid of such left-hand tracing, the 'right' forms of the letter were chosen; whereas on subsequent right-hand tracing the reversed forms were chosen. (ii) Inversion of the test-sheet led to changes in E.'s choice—generally to choice of the corresponding reversed position. This E. explained as due to the fact that he thought that when the paper was inverted the letters must "look the other way."

II.

The foregoing experiments show that:

1. E. could read ordinary and reversed letters and numbers with about equal ease, but read inverted letters more slowly.

2. He had the greatest difficulty in deciding visually "which way a letter should go." He would frequently—invariably in the earliest ex-

periments—*choose* the reversed positions of letters shown him which he had previously learnt with open eyes by (especially left-hand) drawing them, although he could *reproduce* them correctly.

3. The purely visual method of learning letters was found to give the greatest number of subsequent correct visual choices, but even then only about 70 per cent. of the choices were correct.

4. The *reversed* position of a previously unseen letter or form, which E. had learnt correctly to draw *blindfold* with either hand, was preferred not only (i) on the visual presentation to him of a choice of positions, but also (ii) in subsequent drawing with either hand when the eyes were *open*.

5. After such blindfold learning of an unseen letter, repetitions of the movements by the learning hand traced over the letter visually presented in different positions enabled a correct choice to be made; whereas the reversed form was preferred when the tracing movement was performed by the opposite hand.

6. Right-hand drawing practised with the eyes open seemed to favour more correct visual choice than left-hand drawing.

7. A letter correctly learnt by one hand blindfold was not (at all events, necessarily) reversed when subsequently drawn blindfold by the other hand.

III.

These results seem to warrant the following interpretations:

1. E.'s choice and writing of a reversed letter instead of an ordinary letter are the outcome both of visual and of manual influences.

2. A child's early visual experience is probably little concerned with the *absolute* position of seen objects. His attention is first drawn to their form, and his powers of recognition are not gravely disturbed, whether that form once learnt, be re-presented to him in the 'ordinary' or 'reversed' positions or even in the 'recumbent' or 'upside-down' positions; he apprehends the letter, as it were, in a definite visual 'schema'—through the inter-relation of its parts within the whole.

3. Whereas the visual learning of a letter rapidly permits of a simultaneous image of its parts, manual (blindfold) practice at its reproduction affords for some time at first a memory of the successive movements required to draw the letter. But finally, a *motor* 'schema' may be obtained which enables the child to reproduce the letter correctly (blindfold), even with the opposite hand.

4. But before this stage is reached, and while the visual and motor schemata are being integrated, considerable confusion must arise from

the tendency for one hand to perform the reversed (mirrored) movements learnt by the other. Especially must this confusion occur in the case of a left-hand child, like E., whose right and left hands are being both employed in writing. Since, for example, the letter E, when learnt with the right hand, is at this stage most readily written Ǝ with the left, it is evident that in a left-handed child, taught to write with his right hand, and writing with his left when unobserved, a special factor is operative which tends to present him with the wrong (*i.e.* the reversed) form more frequently than in the case of a right-handed child, and so must lead to utter impotence to decide which is the 'right' form of the letter. In the case of such a left-handed child, this special factor may cause him actually to prefer the reverse to the direct forms when presented to him, especially after he has learnt to draw them correctly with his left hand; it may even lead him, when drawing with his eyes open, to reverse a letter which he has already learnt blindfold, with either hand to draw correctly.

5. The better results of right-handed than left-handed drawing with the eyes open, in the case of a left-handed child, are too difficult to interpret here.

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FACTORS IN MENTAL TESTS.

An abstract of an experimental investigation.

BY E. J. G. BRADFORD.

(*From the Psychological Laboratory, King's College, University of London.*)

IF it be granted that the various forms of intelligent behaviour may be related to one another in virtue of the units of behaviour of which they are composed, it seems reasonable to suppose that mental and physical tests should lend themselves to being similarly related, in virtue of the units of behaviour which are involved in successful reaction thereto. The purpose of this investigation was to demonstrate the possibility of a quantitative relation being established between groups of such tests. The task of examining tests with a view to relating them into a system is rendered difficult, however, by many causes, some of the more obvious being (i) the labelling of tests with psychological labels derived from introspective study alone; (ii) the relationship assumed to exist between 'ability' and 'capacity'; (iii) the differential improvability (educability) shown by the subjects tested; (iv) the individual variability of the subjects due to causes which are not under the control of the subject or of the experimenter; (v) the interpretation of the reliability coefficients obtained; (vi) the need of a theory concerning the mode of combination of the units or factors.

Evidence bearing both on the main problem and on the difficulties cited above is furnished by experiments which were carried out with six approximately equal groups of children (three groups of boys and three of girls) totalling in all 224 subjects. Six tests were given on each of ten occasions during one month. The tests used were—Opposites (O.), Backward Alphabet (B.A.), Forward Alphabet (F.A.), Schema (S.), Ring (R.), Ring Motor (R.M.). The last three tests have been described respectively as the Spot Pattern, B. and A. tests in a paper by Dr Reaney[1]. Two control tests were given twice each toward the end of the month—the Dot Chain (D.C.) which consisted of crossing out dots arranged irregularly in a column, and the Handwriting Speed (H.W.) which consisted of writing out the alphabet as rapidly as possible.

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[1] This *Journal*, 1914, VII, 235.

1. Tests demanding a marked degree of concentrated attention give no correlation amongst themselves, *e.g.* R. and B.A. give an average of ·12 ± ·08. Of 450 correlations calculated between the different applications of the six tests only 14 negative ones are as large as their P.E., suggesting that factors correlate positively or not at all. If this result be true generally then the low correlation mentioned above cannot be ascribed to the influence of factors acting in opposition to an assumed 'attention factor'; it is low probably because attention is specific to the activity involved in the test and varies with it.

2. The average correlation between initial and final ability is only ·66; the correlation between initial ability and capacity is considerably lower. An average improvement equivalent to 100 per cent. in the mean performance was shown after ten attempts. The amount of improvement varies considerably among individuals and is the chief influence tending to lower the correlation between initial and final ability.

3. The average correlation between improvability (educability) and initial ability is zero, but with final ability it is ·59. The elimination of the influence of differential improvability is not achieved by an amalgamation of all the applications of a test; continued applications will tend to increase the reliability up to the point where the individual limits of capacity are reached, at which point individual variability alone will influence the reliability.

4. The existence of improvability necessitates the measurement of individual variability about a progressive mean; this measure[1] of variability can be regarded as an inverse measure of the consistency of performance. The average correlation between individual variability and improvability is − ·22. In other words it may be said that consistency and educability correlate to the extent of ·22. Owing to the influence of individual variability, the reliability coefficient between proximate applications of the tests seldom exceeded ·85.

5. Both improvability and variability tend to lower the reliability coefficients and to mask the true relation existing between the tests. This relation appears to be constant, irrespective of the gradual increase in the reliability coefficients, as an inspection of the following table will show.—

Tests correlated	Initial application	Final application	Tests correlated	Initial application	Final application
B.A.—F.A.	·66	·66	R.M.—R.	·58	·57
F.A.—O.	·40	·49	R.—S.	·32	·32
B.A.—O.	·33	·34	R.M.—S.	·25	·26

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[1] Cf. "Measures of Variability," *Amer. J. of Psychol.*, XXVII, 234 ff.

The above uncorrected average values represent a quantitative relation which is not fully assessed but needs correction for the variability of the subjects tested. The reliability coefficient was treated as the personal equation of the group tested, and a correction formula elaborated which gave results almost identical with the Spearman correction formulae.

6. On the basis of correlation values and introspective evidence, certain tests were judged to be relatively simple and others complex. It was assumed that the correlation between the simple and complex tests could be likened to the correlation between two series of dice throws, one of which series was wholly included in the other, the size of the coefficient depending on the relative magnitude of the uncommon factor[1]. The factors were expressed as percentage values, thus:

$\text{D.C.} = 100\,M$ $\text{R.} = 80\,M + 20\,P$
$\text{S.} = 100\,P$ $\text{R.M.} = 48\,M + 52\,P$
$\text{F.A.} = 100\,R$ $\text{B.A.} = 42\,R + 58\,D$
$\text{O.} = 80\,R + 20\,M$ $\text{H.W.} = 54\,R + 46\,M$

The factors Motor, Apprehension (Schematic), Recall and Dissociation (a doubtful factor) are represented respectively by the letters M, P, R and D.

The validity of the assumption that the 'mental' factors (if they may be so termed) may be treated after the manner of dice, is confirmed by the correlations between the complex tests. The theoretical coefficients calculated from the above formulae correspond closely with the actual correlations obtained after correction for variability. In the table below, column A gives the actual correlation corrected for variability, and column B gives the correlation calculated from the derived formulae.

Tests	*A*	*B*	Tests	*A*	*B*
R.M.—R.	·71	·70	B.A.—O.	·49	·58
H.W.—O.	·68	·69	B.A.—H.W.	·43	·44
R.M.—H.W.	·58	·66	R.M.—O.	·41	·40
R. —H.W.	·57	·51	R. —O.	·30	·31

A general law, regarding the mode of combination of factors, is suggested by these results, viz. *that the extent to which simple tests correlate with similar tests of more complex nature may be used as a basis for determining the degree to which the same complex tests will correlate among themselves.*

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[1] Cf. *J. of Exp. Ped.* v. 297.

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A NOTE ON THE USE OF THE PSYCHOGALVANIC REFLEX.

BY W. WHATELY SMITH.

(From the Cambridge Psychological Laboratory.)

THERE can be little doubt that the psychogalvanic reflex is the most delicate method yet devised for the detection and measurement of affective tone, and inasmuch as the latter appears to be the most important determinant of all mental states, it is clearly desirable to refine, by every means in our power, any technique adapted to its quantitative study.

One of the chief difficulties connected with the use of this method is that of making comparable with one another the reactions observed in different subjects and on different occasions. It might be supposed that the absolute magnitude of the reflex produced by such physical stimuli as pricks, burns, sudden noises and so forth, would afford an indication of the comparative 'emotivity[1]' of the subject concerned, and that this might be used as a 'vocational test' for occupations demanding self-control. But it was soon realised that factors other than emotivity greatly affect the absolute magnitude of the reflex which is, therefore, of small value as a test of that quality.

It is with this question of the comparability of reactions that the following observations are mainly concerned.

The phenomenon appears to be a very complex one and we are at present far from a thorough understanding of its mechanism. There seem to be, for instance, at least two clearly distinguishable forms of the reflex; first, a change in the effective resistance offered by the skin to the passage of an electric current and, second, a generated electromotive force which is independent of any current applied *ab extra.*

Of these two varieties the former is certainly a skin effect, though whether it is due to a change within the skin itself or to a change of polarisation at its surface is not yet clear. It is with this form that I shall concern myself below.

Many methods have been used for studying the reflex. Following Waller, I myself have always used a Wheatstone's bridge and D'Arsonval galvanometer in conjunction with two zinc-plate electrodes, covered with wash-leather and soaked in a concentrated solution of common salt; these were applied to the palm and back of the subject's left hand which thus formed the external resistance of the bridge.

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[1] 'Emotivity' being here used to denote liability to react to an exciting stimulus.

The following factors appear to be involved in determining the absolute magnitude of the galvanometer deflexion produced by a given stimulus:

(i) The intensity of the emotion actually evoked.

(ii) The proportion of it which finds expression through those efferent channels which innervate the skin-mechanisms responsible for the reflex.

(iii) The responsiveness of the skin to such innervation[1].

(iv) The initial resistance of the skin.

(v) The sensitivity of the galvanometer.

(vi) The magnitude of the fixed resistances of the bridge.

(vii) The E.M.F. applied to the bridge.

Of these, (i) is the quantity which we wish to measure, (v), (vi) and (vii) are easily kept constant or, if not, suitable corrections can be made on their account. Of the remainder, (iv) can readily be measured, and I deal below with the appropriate correction for it, but (ii) and (iii) are variables for which, at present, no allowance can be made.

I propose to deal here with the question of what correction should be applied to compensate for variations in the initial resistance of the skin.

This point is of importance for two reasons. First: even if we cannot eliminate all the causes of variation, other than (i) above, between different subjects, it is desirable to remove as many as we can, both with a view to closer study of those which remain and in order to reduce the amount of fortuitous variations to be neutralised by the use of such statistical methods as may be necessary. Second: if we are studying the behaviour of the same subject on different occasions we shall wish to make the results obtained as comparable as possible with respect to (i) and we may for the present assume that (ii), at least, and perhaps (iii) are not likely to vary greatly in the same subject from time to time.

One of the obvious results of a difference in resistance between two subjects will be that a heavier current will be passed through the subject of lower resistance than through the one of higher resistance (assuming the E.M.F. on the bridge to be kept constant).

It is easy to show experimentally that, in general, the greater the current passed through the subject, the greater is the absolute magnitude of his reactions. The question then arises whether this increased reaction is due simply to what I may call 'normal' electrical causes or whether, as has been suggested by Prideaux, the heavier current produces some definite effect on the subject of such a nature as to increase his 'irritability' quite apart from the increased deflexion which would be expected on purely electrical grounds. In other words, can the living subject be treated, so far as differences of initial skin resistance are concerned, as if he were an inanimate resistance whose changes we were observing?

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[1] I think it probable that this factor may, for all practical purposes, be subsumed under (ii) or (iv) or both.

This would be easy to determine if we could apply standard stimuli to subjects of different resistances and measure the deflexions produced. This procedure, however, appears to me impracticable, partly because a stimulus of small emotional import to one subject may arouse intense emotion in another and partly on account of possible and unknown effects due to the factors (ii) and (iii).

These difficulties can, however, be largely surmounted by the use of appropriate statistical methods.

In connexion with some experiments on memory and affective tone described in an earlier paper[1], I had occasion to apply a word-association test of 100 words to 50 different subjects, and to compute the mean resistance of each for the period of the test. I therefore first calculated the coefficient of correlation between the mean galvanometer deflexion and the mean resistance for these 50 subjects. Its value was $-\cdot497$; that is to say, there is a strong tendency, as we would expect, for deflexions to increase as resistance decreases.

On general grounds it seemed probable that initial resistance and the deflexion produced by a given stimulus would be connected by a relation of the form

$$R^xD = K$$

when R is the initial resistance, D the deflexion and K a constant. I therefore calculated the values of the coefficient of variation for the expression, R^xD, for this series of 50 subjects, giving x the values 0, 1 and 2 successively.

The resulting values are:

x	C of V,
0	·656,
1	·572,
2	·704.

These values lie on the curve $V = \cdot098x^2 - \cdot182x + \cdot656$, which has a minimum at the point $x = \cdot925$; $V = \cdot5717$.

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[1] This *Journal* (Gen. Sect.), 1921, xɪ, 236.

That is to say the effect on the absolute magnitude of deflexions, of differing resistances of the subjects, can be more perfectly removed by multiplying the deflexions by $R^{\cdot 925}$*—which is substantially equal to R—than by any other power of R.*

(It may be noted that the improvement effected by using the expression $R^{\cdot 925}\,D$ instead of RD is inappreciable, for the coefficient of variation only changes from ·572 to ·5717.)

As a check on this, I substituted a resistance box for the subject and obtained by direct calibration the deflexions corresponding to a constant percentage decrease in resistances of 1, 2, 3, 4, 5, 6, 7, 8, 9 and 10 thousand ohms. Similar treatment of these gave an optimum value for x of approximately ·9. We may therefore conclude that so far as variations of initial skin resistance are concerned, the subject does behave in substantially the same way as an inanimate resistance, and that the differences observed as the effect of passing a larger or smaller current through the subject are wholly due to normal electrical causes and not to any further effect of the current upon the subject himself.

It is not always necessary to apply this correction; but in cases where it is desirable to do so, the inconvenience of actually multiplying each deflexion by the resistance of the subject can be obviated by any one of the three methods described below.

(i) If we take a subject of resistance 1000 ohms, say, as 'standard' and use for such a subject a galvanometer shunt of x ohms selected so as to give deflexions of suitable size for ordinary stimuli (producing a decrease of resistance of about 2·5–3·0 per cent.), it is easy to obtain by calculation or, preferably, by direct calibration, the values of the shunts which will give the same deflexion for the same percentage decrease of resistance in the case of subjects of resistances 2000, 3000, 4000, etc., ohms. These values can be plotted graphically as ordinates against resistances as abscissae and the shunt appropriate to a subject of any resistance can be read off from the resulting graph.

(ii) Another method is to use no shunt on the galvanometer but to control the magnitude of the deflexions by varying the E.M.F. applied to the bridge by means of a potentiometer. Here again, the best procedure will be to calibrate the apparatus directly by substituting a resistance box for the subject, balancing resistances of 1, 2, 3, 4, 5, etc. thousand ohms on the bridge, reducing each of these when balanced by the same percentage—2·5 say—and adjusting the potentiometer so as to give the same deflexion in each case. Potentiometer adjustments can then be plotted against resistances as before.

(iii) A third method, which has the advantage that it requires considerably less apparatus than either of the foregoing, is to abolish the use of the bridge altogether and to employ a modification of Binswanger's arrangement described in Jung's *Studies in Word Association*, p. 446. The subject, battery and electrodes are here placed in series and no bridge is used[1].

When the subject is at rest and not stimulated, there will, of course, be an initial steady deflexion of the galvanometer. Assuming that the resistance of the latter and of the remainder of the circuit is small compared with that of the subject, and that the galvanometer deflexions are proportional to the current over the range in question, the deflexion will increase by a percentage equal to that by which the subject's resistance decreases.

Thus, for a subject of resistance 5000 ohms, the initial deflexion will be twice as great as for one of 10,000 ohms, and so will the added deflexion corresponding to any given stimulus.

If we interpose a potentiometer between the battery and the circuit, we can always pass the same current through the subject, thereby producing a constant initial deflexion and constant subsequent deflexions for the same percentage decrements in the subject's resistance, whatever the absolute magnitude of the latter may be. This is what is required.

The only disadvantage of this method is that the galvanometer always starts with a large deflexion of which only a small percentage increase is observed as the result of stimuli. In its simpler form it has, however, been successfully used by Binswanger, Veraguth and others, and its simplicity and cheapness are very much in its favour.

As I have observed above, it is not always necessary to apply a correction for the resistance of the subject by any of these means or by direct multiplication. The way in which results are handled and the form in which they are expressed should depend upon the objects of the experiments. Thus, if we are using the reflex merely as a 'complex indicator' in a word-association test—as a preliminary to psycho-analytic treatment, for example—no corrections of any sort need be applied, for all that concerns us is the *relative* degree of emotion evoked by the various stimulus words. If, on the other hand, we are seeking to ascertain which words of a list, or which members of a series of other stimuli provoke most emotion, on the average, in a given class of subjects, it will be best to express the reaction to each stimulus as a percentage of the mean reaction of the subject concerned for all the stimuli. Thus, if ten stimuli are applied to a given subject with the following results:

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[1] An alternative arrangement combining the advantages of this with those of the bridge mentioned has been described by Prideaux (*Brain*, 1920, XLIII. 50–73).

Stimulus	A	B	C	D	E	F	G	H	I	K
Reaction	7	3	9	14	2	11	8	6	12	5

of which reactions the arithmetic mean is 7·7, we should express the results for every such subject as follows:

Stimulus	A	B	C	D	E	F	G	H	I	K
Reaction (% of mean)	91	39	117	182	26	143	104	78	156	65

This eliminates not only variations due to resistance but also the danger of the results being unduly influenced by excessively large or small reactions, of whatever origin, on the part of a single subject.

Whether the arithmetic or the probable mean should be used will depend on circumstances; in nearly all cases the latter is preferable. But if we wish to compare the behaviour of different classes of subjects with respect to the psychogalvanic-reflex in general, it will be necessary to apply the correction for resistance. For the classes may differ by virtue of the factors (ii) and (iii) mentioned on p. 283 above, and this may be important. For instance, if we are comparing normal with mentally deficient persons, it may be, as is suggested by some as yet unpublished experiments by Prideaux, that the latter persons give very small reactions to all classes of stimuli because only a small proportion of the emotion aroused finds expression through the mechanisms responsible for the reflex, or because they have skins of resistance much higher than the normal, or because they really feel less—*i.e.* less emotion is actually aroused.

If the uncorrected results were simply averaged for a number of such defective persons and compared with the similarly treated results for normal persons, we could form no definite conclusions on the subject. Whereas, if due allowance is made for variations in skin resistance, any difference between the size of the reactions given by the two classes of subject can either be ascribed to this cause or, when it is eliminated, shown to be due to one or more of the others.

Finally, inasmuch as the phenomenon consists essentially in a lowering of the resistance of the skin, and as the percentage decrease of resistance appears to be, very approximately, directly proportional to the intensity of the emotion aroused, it is desirable that all results which are published with a view to comparison with those obtained by other experimenters should be expressed in terms of percentage decrement of resistance or, at least, that sufficient data should be given to enable the results to be reduced to these terms. Absolute deflexions are valueless for comparative purposes, as they depend so largely on the particular arrangement of apparatus used.

It may be of interest to note that in the case of the 50 subjects mentioned above to whom I applied a word-association test, the mean resistance for all subjects was 4400 ohms and the mean deflexion 7·32 mm. This corresponds, with my apparatus, to a decrement of resistance of about 2·3 per cent.

(*Manuscript received* 10 *October* 1921.)

A COMPARISON OF SPEED WITH ACCURACY IN THE LEARNING PROCESS.

(*A Report to the Industrial Fatigue Research Board.*)

BY MARY STURT.

(*From the Cambridge Psychological Laboratory.*)

I. *Aim of the investigation: conflicting views on the relative importance of speed and accuracy in habit-learning* (pp. 289, 290).
II. *Description of the experiment: two groups of subjects, the one working for speed, the other for accuracy; tests for insuring equal mental ability of the two groups* (pp. 290–294).
III. *Results: profound differences in the two groups when typing sensible matter; effects of reversing the instructions* (pp. 294–297).
IV. *Discussion: error of Gilbreth's views; pedagogical fallacies; initial and final insistence on accuracy* (pp. 297–300).
V. *Conclusions.*

I. AIM OF THE INVESTIGATION.

THIS experiment was undertaken with the object of obtaining evidence concerning the question, whether, in learning habits involving muscular activity, it is better to insist from the start on accuracy or on speed. This point has only recently become a subject of controversy. Formerly it was assumed that the 'accuracy method' was the only feasible one, and that an almost exaggerated attention to details at the start was the only way to achieve skill finally. Such a view is well expressed in the following advice given by Southward[1] to those learning hand-compositing:

> The rule for apprentices should be—first study accuracy and an easy position and regard speed as a secondary consideration. There is more danger in going too fast than too slow.

It is clear from the whole of the above passage that Southward would insist on the beginner working slowly at first, because he believes that only by close attention to a movement can the beginner succeed in making it correctly, and without correct movements it is impossible to obtain speed subsequently. The ordinary school emphasis on accuracy and neatness seems to be due partly to an underlying belief in the *moral* value of tidy work and in the necessity of 'cultivating an accurate habit of mind,' but partly also to the conviction that inaccuracy, once tolerated, can never be removed.

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[1] Southward, *Modern Printing*, 1915, p. 160.

Quite recently, in his *Applied Motion Study*, Gilbreth has delivered a frontal attack of much violence on this long-accepted position. He points out that slow movements are inevitably different *in form* from (the 'same') fast movements, and contends, therefore, that if the beginner starts by learning movements slowly in order to make them *accurate*, he is simply *not* learning the *fast* movements that are finally required.

To this question of speed he attaches great importance, returning to it several times:

> Right motions must be insisted on from the beginner's first day of work. Standard speed of motions must be insisted on from the learner's beginning on his first day, if least waste in learning is to be the first consideration[1].

And again:

> The learning process is the proper repetition of the desired method at the standard speed. The proper insistence is upon the right methods at standard speed first and the quality of work produced second[2].

He explains that by standard speed he means the speed at which the maximum of work can be produced. If his methods are followed, he feels sure that accuracy will ultimately be attained. If this position is to be accepted, it clearly involves a method of teaching totally different from that in general use at present, and it is of considerable importance to industry.

II. Description of the Experiment.

The general plan of the experiment was that two groups of subjects, as nearly equal in ability as possible, should learn some activity, one group by the 'accuracy method' and the other by the 'speed method,' and that then the results should be compared. Typewriting was chosen as the work to be learnt as it seemed easier to get subjects for this than for a 'useless' activity, such as some motor test. The subjects were two groups of ten each, all girls, between the ages of 12 and 14, attending two Cambridge schools, R. and P.[3]. The R. group learnt by the speed method, the P. group by the accuracy method. Owing to the fact that different instructions were given to the two groups (see below), it was essential that members of one group should not discuss the experiment with members of the other, and for this reason the groups were obtained from different schools. Each group of ten was selected from a much larger group at its school, with the object of obtaining two *equal* groups. The facts considered in making this selection for equality were (1) age, and (2) performance in several mental tests. (The social status of one group seemed identical with that of the other.) These data are set out in Tables I and II, the tests used being as follows.

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[1] *Op. cit.* p. 109.

[2] *Op. cit.* p. 121.

[3] I must thank the Cambridge Borough Education Committee for facilities for the experiment, and the headmaster and mistresses of the schools for their great kindness throughout.

(1) *A Substitution Test* (digits for letters). This test was given twice; on the first occasion (*A* in Tables I and II) a longer time was allowed for it than on the second (*B* in Tables I and II). The score used was the actual number of substitutions made in the time allowed. The figures for this test in Tables I and II are these scores.

(2) *The Woodworth and Wells Opposites Test.* The score was the number of opposites written in one-and-a-half minutes, 40 words being used in the test. The figures for this test in Tables I and II are these scores.

(3) *A Trabue Completion Test.* Each blank filled in correctly was scored = 1. The figures for this test in Tables I and II are the total scores.

(4) *An Immediate Memory Span Test.* The material consisted of a number of sentences, written sufficiently large to be exposed to the whole group at once. Each sentence was shown for 10 seconds. The score was the total number of words correctly reproduced. The figures for this test in Tables I and II are these scores.

(5) *A Spelling Test.* The material consisted of a selection of 50 words from Burt's test[1]. The score was the number of words correctly spelled. The figures for this test in Tables I and II are these scores.

Of these tests, the *substitution* test was used because it has been considered a good test for typewriting capacity[2], though it is doubtful if this is so. The *opposites* and *completion* tests were used because they are among the best 'general ability' tests. The *immediate memory span* test was used because the function it brings into operation seemed important for typewriting. The reason for the *spelling* test was that errors in typewriting are often due simply to faulty spelling, so that if one group were here clearly superior to the other it might have a certain advantage over it. It will be seen by comparing the figures in Tables I and II that this was the one test in which the groups did not appear to be entirely equal, the

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[1] See Ballard's *Mental Tests*, pp. 157, 158.

[2] See *Industrial Fatigue Research Board Report*, "On Vocational Guidance," section on Typewriting (now in press).

P. group being apparently slightly superior to the R. group in spelling capacity. Partly to prevent this difference from interfering with the results, the copy material chosen for teaching was such as involved a minimum of spelling difficulty.

TABLE I. R. GROUP.

	The tests						
Subjects	Substitution		Opposites	Completion	Memory	Spelling	Age
	A	*B*					
S. E.	379	210	27	23	94	42	13·2
I. C.	360	225	29	29	87	33	14·2
K. C.	355	173	26·5	27	95	28	13·3
D. H.	351	147	36	31	91	24	14·2
G. G.	264	140	31	19	100	43	13·5
E. E.	340	159	40	31	96	46	13·8
G. D.	311	167	34	30	96	38	14·2
K. M.	296	144	30	28	95	38	14·4
D. B.	278	149	28	26	87	31	13·4
I. A.	256	106	26	23	99	26	13·6
Total	3190	1620	307·5	267	940	349	137·2
Av.	319	162	30·75	26·7	94	34·9	13·85
M.V.	38	25·4	3·6	3·16	3·4	6·5	2·7

TABLE II. P. GROUP.

	The tests						
Subjects	Substitution		Opposites	Completion	Memory	Spelling	Age
	A	*B*					
M. S.	360	154	34	27	96	33	12·10
R. W.	350	158	28	25	93	46	13·10
D. L.	335	154	29	28	100	46	13·5
O. F.	328	155	30	27	95	45	13·6
G. S.	305	194	35	20	95	38	13·6
E. C.	389	210	37	21	90	38	13·4
V. H.	307	137	29	28	100	37	13·6
I. C.	296	147	39	31	96	45	13·10
B. M.	268	122	31·5	31	91	30	13·5
M. T.	257	124	34	24	85	42	13·5
Total	3195	1555	326·5	262	941	400	134·7
Av.	319·5	155	32·65	26·2	94·1	40	13·55
M.V.	32·9	19	3·2	3	3·5	4·8	2·1

These tables show that, in ability to perform the tests used, the two groups of children are as nearly as possible equal. The greatest difference is in spelling, and even there it is not very large. In age, too, the groups are nearly identical. If, therefore, one group proves superior in typewriting to the other, this may reasonably be accounted for by supposing that it learnt by the superior method.

The teaching was individual, all the children using the same typewriter. Each subject was given about two half-hours teaching a week, R. (which formed the group taught for speed) on Wednesdays and Fridays, P. (the accuracy group) on Tuesdays and Thursdays. No attempt was made to teach by the touch method; but clearly this does not affect the broad results of the investigation and the problem at issue. The children were allowed to look at the key-board, though they were told that they should try to learn to work without looking. Each lesson began with the typing of the alphabet, after which the subject copied a passage of connected prose (as much as possible in the lesson period) usually taken from a simple translation of the *Odyssey*. The fingering taught was that given in Mrs Clough's book on typing. Gilbreth's pictures of 'champion' typists at work were used to show the proper position of the hands, and the movement of the fingers. This was the same for all. In addition, the speed group were told that of the two essentials for a typist—speed and accuracy—speed is the more important. They were given marks, and now and then prizes, *simply for speed*, and they were never shown their old papers nor were their mistakes mentioned. In the case of the accuracy group the procedure was reversed. They were told to aim at accuracy, were marked on this, and they were *constantly shown their papers with their mistakes corrected*, and nothing was said about speed.

The experiment began on November 2, 1920, and continued with breaks for holidays till May 6, 1921. During this period each child had had about 16 hours' practice. The characters of the performances of the two groups were by this time clearly defined, and as a continuance of the same methods seemed unlikely to produce any important change, the instructions given to the groups were *reversed* in order to see how permanent the results were, and whether the earlier teaching would render the children unable to acquire the characteristic that had been previously neglected. After this change in method, 11 lessons were given to each group, extending from May 10 to June 30, each child receiving about six hours' teaching.

In scoring it was thought better to use key-strokes (letters, spaces, etc.) as the basis rather than words; as the former method was both more accurate and easier to work. The alphabet was typed at the beginning of each lesson. As it contains 51 such strokes, the speed and accuracy of typing the connected prose were calculated on this basis of 51 key-strokes so as to make all records as nearly comparable as possible. The speed given in the table of results below is the time taken to type 51 strokes (the whole time any subject typed at a lesson having been recorded, as also the total number of strokes made and the time for 51 strokes calculated from this); and the accuracy, the number of mistakes made in 51 strokes, was calculated in the same way. Errors in spelling, spacing, capitals, and punctuation were counted as mistakes.

III. RESULTS.

The main results of the investigation are shown in Table III and the curves corresponding to it (Figs. 1–3).

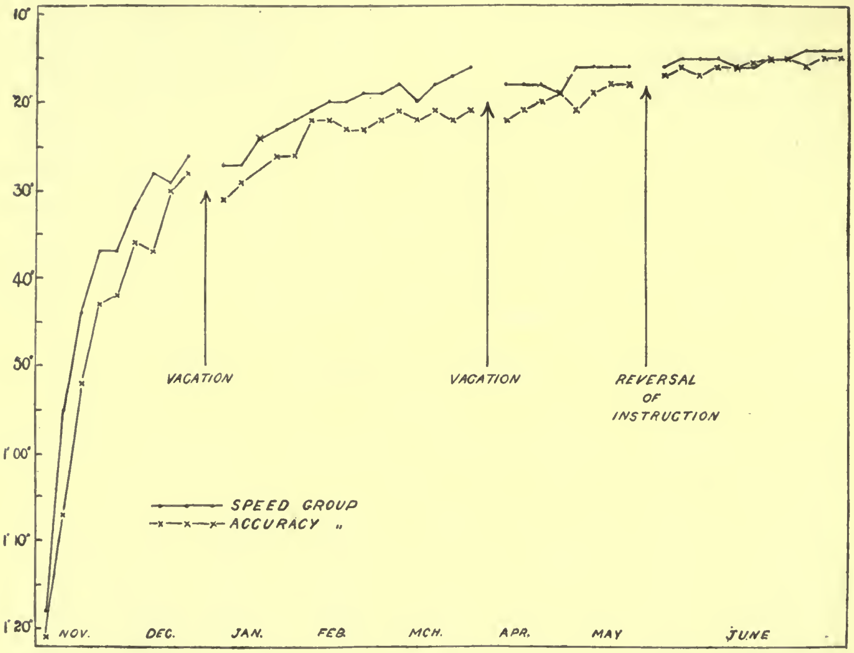

Fig. 1. Average speed of each group in making 51 key-strokes in typing the alphabet (at the beginning of each lesson) at the different stages of the experiment. The higher the curves the better the performance.

The times given in Table III (representing *speed* of work) are the average times taken by the subjects of each group for 51 key-strokes, independently of whether the strokes were correct or incorrect. The two last columns give the average numbers of mistakes (incorrect strokes) made by the subjects of each group per 51 key-strokes.

Let us first consider the typing of the alphabet. It will be seen from Fig. 1 (and the corresponding part of Table III) that the accuracy group takes slightly longer than the speed group to type the alphabet, though

TABLE III.

Date	Time taken to type alphabet (51 key-strokes)		Time per 51 key-strokes taken to type connected prose		Number of mistakes per 51 key-strokes (connected prose)	
	Speed group	Accuracy group	Speed group	Accuracy group	Speed group	Accuracy group
Nov. 9	1′ 18″	1′ 21″	—	—	—	—
,, 11	55″	1′ 7″	1′ 42″	2′ 7″	1·27	1·25
,, 16	44″	52″	1′ 28″	1′ 45″	1·35	1·12
,, 18	37″	43″	1′ 20″	1′ 31″	·95	·75
,, 23	37″	42″	1′ 12″	1′ 34″	·93	·62
,, 25	32″	36″	1′ 7″	1′ 23″	·83	·57
,, 30	28″	37″	1′ 6″	1′ 23″	·89	·52
Dec. 2	29″	30″	1′ 5″	1′ 29″	1·13	·59
,, 8	26″	28″	1′ 4″	1′ 15″	·82	·47
Vacation						
Jan. 13	27″	31″	1′ 1″	1′ 20″	·83	·19
,, 18	27″	29″	58″	1′ 11″	·98	·39
,, 20	24″	24″	59″	1′ 2″	·69	·35
,, 25	23″	26″	54″	1′ 8″	·84	·25
,, 27	22″	26″	53″	1′ 4″	·78	·35
Feb. 1	21″	22″	52″	1′ 5″	·79	·25
,, 3	20″	22″	49″	1′ 11″	·83	·30
,, 8	20″	23″	49″	1′ 11″	1·00	·28
,, 10	19″	23″	46·5″	1′ 1″	·78	·28
,, 15	19″	22″	45″	1′ 4″	·94	·20
,, 17	18″	21″	46″	1′ 3″	1·04	·29
,, 22	20″	22″	43″	1′ 5″	1·07	·24
,, 24	18″	21″	44″	1′ 1″	·92	·28
March 1	17″	22″	42″	58″	1·03	·30
,, 3	16″	21″	39″	1′ 1″	1·05	·15
Vacation						
April 8	18″	22″	43″	58″	1·11	·26
,, 13	18″	21″	43″	56″	1·14	·25
,, 15	18″	20″	42″	59″	·87	·15
,, 20	19″	19″	42″	55″	1·16	·17
,, 22	16″	21″	39″	48″	1·01	·12
,, 29	16″	19″	42″	58″	1·14	·38
May 4	16″	18″	39″	57″	1·44	·25
,, 6	16″	18″	38″	52″	1·13	·20
Reversal of Instructions						
May 13	*16″*	*17″*	*41″*	*44″*	*·63*	*·33*
,, 18	*15″*	*16″*	*44″*	*43″*	*·47*	*·33*
,, 20	*15″*	*17″*	*39″*	*44″*	*·50*	*·18*
,, 27	*15″*	*16″*	*41″*	*40″*	*·49*	*·24*
June 1	*16″*	*16″*	*41″*	*41″*	*·43*	*·29*
,, 8	*16″*	*16·5″*	*38″*	*39″*	*·42*	*·29*
,, 10	*15″*	*15″*	*39″*	*39″*	*·47*	*·25*
,, 15	*15″*	*15″*	*40″*	*39″*	*·41*	*·24*
,, 17	*14″*	*16″*	*42″*	*43″*	*·47*	*·28*
,, 22	*14″*	*15″*	*39″*	*41″*	*·41*	*·32*
,, 24	*14″*	*15″*	*37″*	*37″*	*·33*	*·31*

NOTE TO TABLE III.

The dates in this Table are those for one group only: the true dates for the other group are in each case one day later or earlier than the given dates. But for convenience of tabulation the dates for the two groups are made the same, as this in no way affects the results. As was explained above, the speed group had its lessons on Wednesdays and Fridays, the accuracy group on Tuesdays and Thursdays.

On the first lesson day only the alphabet was typed, the remainder of the lesson being taken up with explanations. This accounts for the blanks in the Table for date November 9.

the difference between them practically disappears after the reversal of instructions. This approximate equality between the groups in typing the alphabet seems to show that, as far as the mechanical part of the typing went, the groups were equal; such difference as was shown during the earlier part of the learning process being due to the difference of methods.

From all the other results, however, it is clear that the two methods of teaching produced very different results. Almost from the beginning the two groups began to draw apart, and this difference became more marked as time went on. For instance, on December 10, when the groups had had ten lessons (amounting to about five hours' practice) the speed group was 17 per cent. superior as regards speed, and the accuracy group was 74 per cent. superior as regards accuracy. These differences increased. By May 4, after 33 lessons or about 16 hours' practice, the speed group was superior in speed by 38 per cent. and the accuracy group in accuracy by 450 per cent.

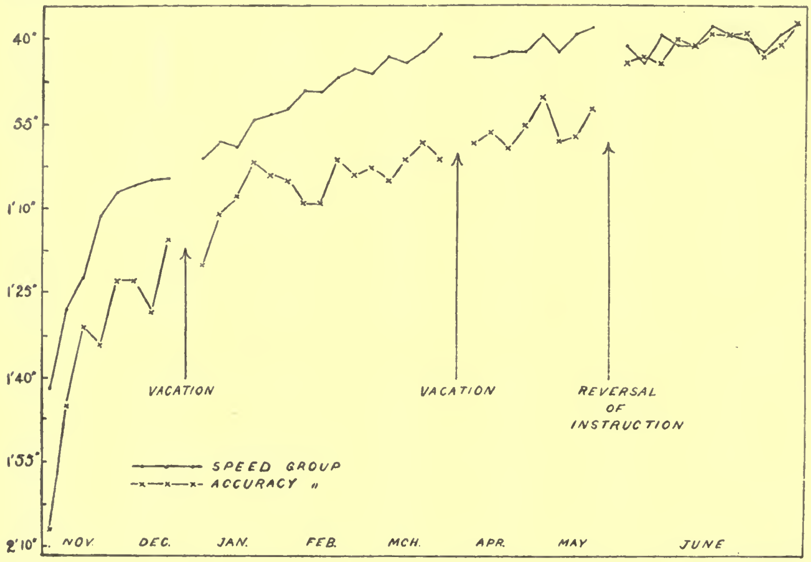

Fig. 2. Average speed of each group per 51 key-strokes of connected prose at the different stages of the experiment. The higher the curves the better the performance.

It was at about this point that the method of teaching was changed. Both groups were now treated alike, and encouraged to attend both to speed and accuracy, though with each group most stress was naturally laid on what had hitherto been neglected. The result, as can be seen, was that the two groups drew practically level, the accuracy of both groups rapidly approximating, and the difference in speed disappearing. The fast group did not, however, attain quite the same level of accuracy as the accuracy group until just at the end of the experiment. The speed of the two groups remained the same throughout this final period. It should be noted particularly that the speed group acquired at once almost the accuracy of the accuracy group *without appreciable loss of speed*, and similarly, that the accuracy group acquired at once the speed of the speed group *without appreciable loss of accuracy* (cf. Figs. 2 and 3).

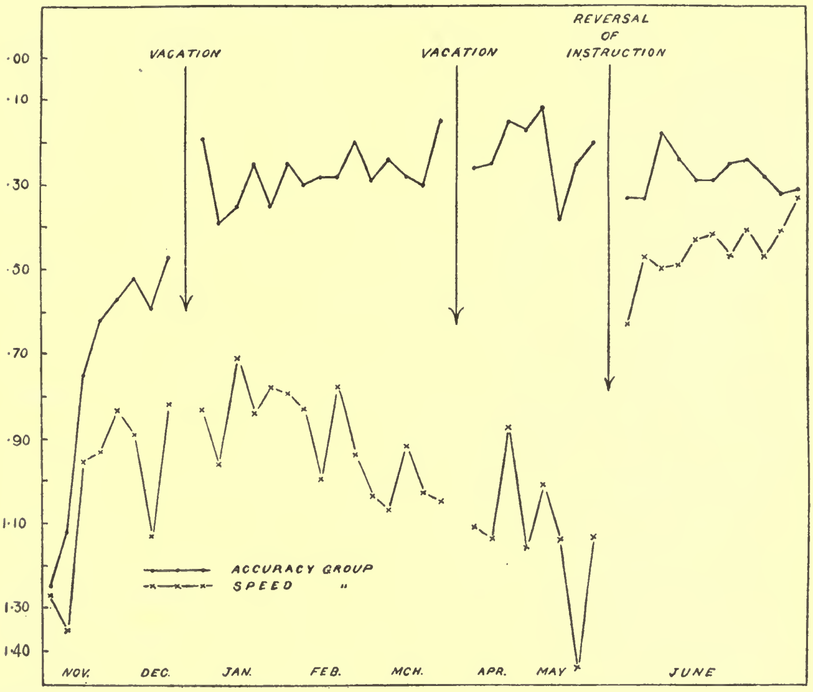

Fig 3. Average number of mistakes (incorrect key-strokes) per 51 key-strokes made by each group in typing connected prose at the different stages of the experiment. The higher the curves the better the performance (the greater the accuracy of the typewriting). Ordinates indicate number of mistakes per 51 key-strokes.

IV. DISCUSSION.

From the foregoing results it is perfectly clear that so far as typewriting is concerned Gilbreth is wrong in his contention that learning motions slowly is no help to the making of them quickly after they are learnt. The rate at which a beginner *is allowed* to work need make no difference whatever to the rate at which he *can* work after a certain amount of practice. When required, children who for six months have been allowed to work as slowly as they like, can acquire the speed of a group that has been exhorted from the beginning to work as fast as possible. The improvement is immediate, and the records of the two groups remain to the end so similar that they may be considered identical.

It is nearly as clear that the talk about the deplorable results of 'careless haste' is equally unfounded. Children who have been making over eighty mistakes in a page of typing, can, in the course of three lessons, reduce them to nine, and that with only the very smallest decrease in speed. But, at the same time, it must be noted that accuracy will not improve of itself as speed will. The group that was trained in accuracy gradually improved in speed: but the group that was trained in speed did not constantly improve in accuracy—on the contrary, after an initial improvement it gradually became less accurate (cf. Fig. 3). To effect an improvement in accuracy it needed a fresh stimulus directed to this particular point; and to the experimenter it seemed that the *rate* of the improvement in accuracy depended mainly on the degree of influence she had over individual children, some responding with a great improvement in the first or second lesson, others, who were noticeably slow and apathetic about the whole of the teaching, only improving gradually. The cases in which carelessness has become a 'habit' are doubtless those in which there has never been any adequate stimulus to accuracy.

There is another point in connexion with teaching method. The curves for accuracy fluctuate, in general, more than the curves for speed. This is suggested by Figs. 2 and 3. Table IV gives exact figures for one section of the curve. Here the scores (for both speed and accuracy) for the eight lessons from April 7 to May 6 have been averaged, and the daily deviations from these eight-day averages calculated as percentage of the eight-day averages. The results are as follows:

During this period, then, the average fluctuations in accuracy are, in the accuracy group, four times, and in the speed group twice, those of speed. This may be due to temporary causes, such as distraction or fatigue, having a greater influence on accuracy than on speed. A method of teaching, in any case, which is based upon insistence on accuracy must involve more constant rebuke and be more unpleasant to the learner than one which makes speed its aim; yet, if the quality of the *immediate* product is the chief concern, this greater variability as regards accuracy renders constant attention to it necessary. If, however, the quality of only the *final* product is the important matter, the period of learning may be made much more pleasant for the pupil (and for the teacher too) by neglecting fluctuations in accuracy, since accuracy by the way seems not to be essential for final accuracy.

TABLE IV.

Showing deviations of the daily group average from the group average for eight days in both speed and accuracy; the deviations being expressed as percentages of the eight-day averages.

	Accuracy group % deviation from average for			Speed group % deviation from average for	
Date	Speed	Accuracy	Date	Speed	Accuracy
April 7	4·7	17·6	April 8	4·9	1·5
,, 12	1·0	10·6	,, 13	4·9	1·2
,, 14	6·5	30·5	,, 15	2·5	22·4
,, 19	0·7	23·7	,, 20	2·5	2·9
,, 21	13·4	45·0	,, 22	4·9	9·9
,, 28	4·7	23·7	,, 29	2·5	1·2
May 3	2·9	11·9	May 4	4·9	28·3
,, 5	6·1	9·7	,, 6	7·3	·3
Total	40	172·7		34·4	67·7
Average	5	21·6		4·3	8·5

Certain features of the experiment illustrate serious difficulties in the way of carrying out Gilbreth's suggestion, even if his general contentions were correct. He assumes that before beginning to teach habits involving (or consisting of) muscular activity, the best motions have been discovered and motion models set up. In typewriting, at all events, owing to the large number of combinations of movements, the latter is impossible, and, from this experiment, it seems unlikely that the same movements are desirable for all individuals; *e.g.* many of the children seemed unable, through weakness, to use the little finger of the left hand. Moreover, many of the children seemed to find great difficulty in learning to make, or to omit to make, some particular movement. In the speed group this difficulty was more noticeable than in the accuracy group, apparently because the effort to work fast prevented them from paying adequate attention to the details of their movements. Near the end of the experiment, notes were taken of the fingering and the position of the hands of the children; of the four who were best, three came from the accuracy group, and of the four worst, three were from the speed group. On the average too, the movements of the accuracy group were considerably more 'correct.' This result is only what might be expected in learning a habit involving varied and difficult movements. Unless some extremely stringent system of discipline is in use, it is impossible, if speed is demanded from the start, to insure absolute correctness of movement. Of course, the value of 'correctness' of movement is uncertain; but it would probably appear in advanced stages of practice.

V. CONCLUSIONS.

1. If movements such as are employed in typewriting are learnt slowly at first, the length of the learning process is not thereby increased; and it appears highly probable that the same speed can be obtained finally as when speed is insisted on from the start.

2. If during learning the attention is directed solely to accuracy, the speed will gradually improve. If attention is directed solely to speed, the accuracy tends to diminish.

3. Between the two methods employed—(*a*) exclusive attention to speed, and (*b*) exclusive attention to accuracy—there seems little to choose when considered simply as methods of learning. The choice must depend on other considerations, *e.g.* on how soon it is desired to use the product.

4. Without special apparatus it seems unwise to demand speed from the *very* beginning, as it becomes more difficult for the learner to acquire 'correct' movements (assuming these to be ultimately valuable).

5. It seems probable that a high quality of work can be obtained finally, without insisting on a high quality constantly throughout the learning process. This would appear to have a very general bearing on methods of teaching.

PUBLICATIONS RECENTLY RECEIVED

Suggestion and Autosuggestion. By CHARLES BAUDOUIN. Translated from the French by EDEN and CEDAR PAUL. London: George Allen and Unwin. 1920. pp. 288. 15*s.* net.

This very entertaining book shows once again that successful practice in psychotherapy has very little to do with whether the practitioner has clearly thought out the theoretical basis of his work. The underlying principle throughout is that every idea tends to find active expression. When the process leading to such expression is subconscious, there is suggestion (p. 26). But 'idea' is never really defined at all, and consequently can be used in all kinds of ways, and with many different shades of significance. We are told that 'idea' is simply equivalent to 'representation'; but this does not help us very clearly to understand what can be meant by such statements as: "The idea of a sensation of pleasure or pain, the idea of a feeling, tends to become this pain, or this feeling." The difficulty is not rendered any easier when we read: "the idea of a sentiment is already the sentiment." It is suggested in one place (pp. 42–3) that an idea is some kind of a *schema*, and in another (p. 78) that it is merely a tendency. In fact, Baudouin's terminology in this respect is extremely muddled. When we turn to other matters there is the same difficulty. Baudouin shows immense enthusiasm, but he mistakes certainty for clearness. What, for example, can be made of the 'law' "In the conflict between the will and the imagination the force of the imagination is *in direct ratio to the square of the will*"? The really interesting thing is how such formulations come to have their undoubted practical value.

The book is always interesting, often acute, sometimes illuminating; but very few readers who are not carried away by enthusiasm will admit that as a theoretical formulation it can be regarded as satisfactory.

The Psychology of Thought and Feeling. By CHARLES PLATT, Ph.D., M.D. London: Kegan Paul, Trench, Trübner & Co. 1921. pp. x + 290. 7*s.* 6*d.* net.

This is best described as a 'nice' book. It contains nothing of importance that is new, but it is written with a good knowledge of modern psychology, and is interesting throughout.

Reports of the Industrial Fatigue Research Board. No. 11, *Preliminary Notes on Atmospheric Conditions in Boot and Shoe Factories.* By W. D. HAMBLY and T. BEDFORD. pp. 69. 3*s.* net. No. 13, *A Statistical Study of Turnover in Munition and other Factories.* By G. M. BROUGHTON, E. M. NEWBOLD and E. C. ALLEN. pp. 92. 3*s.* net.

The first of these two Reports contains nothing of immediate psychological interest; it "deals with the practical use of the Kata-thermometer as an indicator of atmospheric conditions from the physiological aspect," and gives many series of readings, showing how the instrument may be employed to measure the cooling power of the air. The second which includes enquiries into the labour turnover of three factories engaged in making biscuits, leather goods and drugs, will prove of great value to industrial psychologists as an attempt to measure labour wastage by a uniform systematic procedure. Evidence is adduced that the high leaving rate in the early months of factory service was not restricted to munition factories during the war but is still in other factories a source of serious economic loss and is very largely due to such often avoidable causes as ill-health, incompetence and discontentment.

Psychotechnik und Taylor-System. Von K. A. TRAMM. Erster Band. Berlin: J. Springer, 1921. S. vii + 140. M. 24; geb. M. 29.

The author is an engineer, apparently in the service of the Berlin tramways. His descriptions of motion study work apply especially to the training and selection of employees in this occupation. Most of them are elaborations of the methods of Gilbreth. In several instances the author gives no reference to the source of his illustrations; in others the work of Moede, Piorkowski and Amar has been evidently drawn upon. The book will prove of considerable interest and value to engineers and others who are unacquainted with the progress of industrial psychology.

Man's Unconscious Passion. By Dr WILFRED LAY. New York: Dodd, Mead and Co. 1921. pp. 246. 10*s*. 6*d*.

This book deals with the dangerous results of unconscious incestuous passions. The theory is ingenious, but the treatment unscientific in the extreme; no evidence whatever is brought forward to support the statements which are made. Unconscious passion itself is defined in two different ways and no attempt is made to make the two definitions mutually compatible.

The Depths of the Soul. By Dr W. STEKEL. London: Kegan Paul, Trench, Trübner & Co. 1921. pp. 216. 6*s*. 6*d*.

This is a well-translated, readable book, in which the author discusses in popular language a number of human affections and motives which are common in every-day life. The interest of the book consists in the fact that the topics are treated from the standpoint of psycho-analysis.

The Care of the Adolescent Girl. By PHYLLIS BLANCHARD, Ph.D. London: Kegan Paul, Trench, Trübner & Co. 1921. pp. xxi + 201. 7*s*. 6*d*.

This book discusses the special difficulties attending the mental transition from childhood to womanhood in the light of the work of Freud, Jung, and Adler. The cases with which the discussion is illustrated are largely taken from the author's psychological experience in this field. In spite of some vagueness and possible exaggeration of statement at times the book contains a useful summary and illustration of the views and facts with which it deals.

Die differentielle Psychologie: in ihren methodischen Grundlagen. Von Dr WILLIAM STERN. Dritte Auflage. Leipzig: J. A. Barth. 1921. S. x + 546. M. 63; geb. M. 72.

This is a reprint of the edition of 1911 which was exhausted several years ago. It contains, as an *addendum*, an account of the recent progress of the psychology of individual differences and a bibliography of nearly 400 books and papers on the subject which have appeared between the years 1911 and 1921.

Periodic Variations in Efficiency: as shown in mental and physical tests together with some weather effects. By Archibald G. Peaks. Baltimore: Warwick & York. 1921. pp. 95.

This is a study of general periodic changes in human life, and especially of the rhythms of growth through which youth passes, while developing into manhood. The author gives a summary of work already done upon the subject; together with a criticism of the means of investigation employed by his forerunners. In the experimental part of his work, the author deals with the question of a seasonal periodicity (i) in muscular strength, and (ii) in mental ability. A prolonged series of tests was made upon schoolboys; the Smedley dynamometer being used to test variations in muscular strength, while for demonstrating any periodic variations in mental ability, a primary memory test was given. The results appear to show that as regards the physical strength of growing boys, there is a period of increase from September to about the middle of December; this is followed by a period of depression between January and March, while a renewed period of growth in strength begins in March and continues till June. The curve for mental efficiency tends to resemble that for physical strength. It shows a rise from September to the end of the year with another increase from March to May, the drop between these periods of increase also appears, but is less marked than is the case of muscular efficiency.

NOTES ON RECENT PERIODICALS

The American Journal of Psychology, Vol. XXXII.

No. 1, January, 1921.

The American Journal of Psychology (G. Stanley Hall and E. B. Titchner).
Gives a history of the Journal on the occasion of Prof. Stanley Hall's retirement from the editorship.

Early Psychological Theories of Herbert Spencer (G. B. Denton).
Discusses Spencer's theories of 'faculties,' 'fatigue' and 'attention,' as given in his *Philosophy of Style.*

The Comparative Influence of Majority and Expert Opinion (H. T. Moore).
Shows by experiments that expert and majority opinion have equal sway over the individual in Morals and Music, but that in matters of linguistic preference the chances are ten to seven in favour of majority prestige.

The Number-Forms of a Blind Subject (R. H. Wheeler and T. D. Cutsforth).
Describes how the blind subject never visualises numbers, but translates colours into numbers in vocal-motor fashion.

Some Problems in Regard to Alimentary Sensitivity (J. G. Campbell).
Hunger is perceptual and appetite involving a different conscious state from hunger. Sensations of fullness are due to stretching of the muscular coat. The stomach certainly and the duodenum probably have thermal Sensibility.

Pleasantness and Unpleasantness in Relation to Organic Response (P. T. Young).
There is no organic-kinæsthetic *sine quâ non* of affection. Unpleasantness is organically positive and active, while pleasantness is negative and passive.

An Experimental Study of Kinœsthetic Imagery (A. H. Sullivan).
Kinæsthetic images of memory are uniform, simple and lacking in body, whereas kinæsthetic sensations are varied and complex.

Affective Tendency as Conditioned by Colour and Form (M. Yokoyama).
As far as relative pleasantness is concerned colour and form are mutually independent in producing affective results, even though they are simultaneously operative.

Brentano and Wundt. Empirical and Experimental Psychology (E. B. Titchner).
A comparative study of the two writers, Brentano's psychology being essentially a matter of argument, and that of Wundt being essentially a matter of description.

A preliminary Study of the Range of Visual Apprehension (S. W. Fernberger).
Shows that the statistical limen, that stimulus value for which 50 per cent. correct judgments are given, is the most reliable measure of visual apprehension.

Emotions and Instincts (H. C. Link).
The notions of 'emotion' and 'instinct' as represented in controversies centering round James-Lange theory are obscure. McDougall's identification of instinctive responses with a "group of emotions arbitrarily selected" is untrue to fact.

The Relation of the Pleasantness of Colour Combination to that of the Colours seen singly (M. F. Washburn, D. Haight and J. Regensburg).
The unpleasantness or pleasantness of a colour combination is derived not merely from summation of the affective tones of its components, but from another factor dependent on the combination itself.

No. 2, April, 1921.

Wilhelm Wundt (E. B. Titchner).

An appreciative review of the career of Wilhelm Wundt.

The Child Mind (H. J. Mulford).

Since mind is the expression of the brain, the development of the child mind is only a development of consciousness in the child brain.

On the Relevancy of Imagery to the Process of Thought (C. Comstock).

If imagery is present as part of the contents of thought, it is *ipso facto* relevant to the thought.

An objective Interpretation of Meanings (J. R. Kantor).

Meanings are definite responses to stimulating objects in their proper setting, differing from differential reactions in that they condition a succeeding act.

A quantitative Investigation of the Effect of Mode of Presentation upon the Process of Learning (F. J. O'Brien).

The modality of imagery which a learner employs is determined by his ideational type, and only secondarily by the mode of presentation.

Voluntary Control of Likes and Dislikes; The Effects of an Attempt Voluntarily to change the Affective value of Colours (M. F. Washburn and S. L. Grosse).

Extreme judgments of preference are harder to change voluntarily than moderate ones and judgments of extreme unpleasantness are decidedly harder to change than judgments of extreme pleasantness.

Journal de Psychologie normale et pathologique (XVIII^e Année).

No. 7, July, 1921.

Doctrines et méthodes psychologiques de l'Inde (P. Masson-Oursel).

The metaphysical assumptions of Indian Psychology are contrasted with those of European Psychology. This is followed by a *resumé* of the essentials of (1) Brahmanistic Psychology of the Vedas and Brahmanas, (2) Buddhist and Jain Psychology and Metaphysics, and (3) Hindu Psychology, religious and philosophical. Finally an account is given of the psychological analysis of the individual which is common to all branches of Indian thought.

Un fait de rire (G.-H. Luquet).

An incident in the war introduces a discussion of laughter as a liberator from painful situations. The kinds of stimuli and situations which produce laughter are distinguished. Special attention is paid to the muscular response to such relieving stimulation.

L'interprétation du rhythme du cœur dans certains rêves (M. Déat).

Three dreams occurring to a subject in a fatigued state present markedly rhythmic characteristics which are ascribed to the rhythm of the heart-beat, changes in the former rhythm being correlated with changes in the rhythm of the latter.

A report of a meeting of the Société de Psychologie is added with papers by P. Sollier on *The Nemo-muscular phenomena which accompany the processes of rapid 'weaning' from morphine,* and by R. Stumper on certain points in the psychology of ants.

Psychological Monographs, Vol. XXX.

No. 1, Whole No. 134.

A Tentative Standardisation of a Hard Opposites Test (Marie Hackl Means).

The aim of the writer was to obtain a standard 'Hard Opposites Test' suitable for application to college students. Stimulus words were selected, and graded in order of difficulty by statistical methods. Each of these words had a number of possible responses, and a response was assigned a value by averaging the values given in each case by five independent judges. A list of these stimulus words in order of difficulty was arranged as a group test, and copies were sent to a large number of colleges and universities. In this way records from 1628 students were obtained, from which norms for college students were established. These standards are presented in terms of percentiles for each class of students, including graduates; and for each sex.

The results of the investigation tend to show that success in the test depends on native ability rather than on years of schooling, since there is a great overlapping among the classes, and also since the lowest scores gradually rise from year to year while the highest scores remain constant.

No. 2, Whole No. 135.

The Influence of the Time Interval on the Rate of Learning in the White Rat (Joseph M. Yarbrough).

The objects of these experiments was threefold:

(1) To investigate the influence of the time interval upon the rate of learning;

(2) To determine the readiness with which a given temporal association will function backwards, and the most favourable time interval for this functioning; and

(3) To consider the claims of the rival theories of simultaneous and successive presentation in relation to the rate of learning.

The experiments were carried out in an improved problem box. The animals were required to learn first a negative response to pain; then to transfer the response to an auditory stimulus introduced in a definite temporal relation to the pain stimulus; and finally, to substitute the response from the auditory to a visual stimulus. The number of trials necessary to learn each problem was recorded.

The results show that the number of trials necessary to learn the association does not increase proportionally with increase in the time interval between the presentation of the pain and auditory stimuli. Also association backwards is very little more difficult than forwards when a continuous mode of presentation is used, but a time interval of one second probably makes backward association impossible. The writer concluded that the simultaneous mode of presentation is slightly superior when the exposure time *of the stimuli* per term is unequal; but if the exposure time is equal the better mode of presentation depends on the material used.

Journal of Experimental Psychology.

No. 1, February, 1921.

The Inspiration-Expiration Ratio During Truth and Falsehood (Harold E. Burtt).

When a person is consciously lying, in general, certain of his emotions are aroused, and the deceit may be indicated by a measure of emotional expression. The inspiration-expiration ratio may be used with fair success as an indication of deceit, but systolic blood pressure forms a better criterion. Both measures may be to some extent vitiated by voluntary control.

An Experimental Study of Motor Ability (F. A. C. Perrin).

Even the simplest motor test brings into play a variety of factors which ordinary experimental conditions do not attempt to analyse or isolate. In particular there can be no satisfactory study of motor accomplishment apart from a consideration of how motives operate.

Comparative Simple Reactions to Light and Sound (F. L. Wells, C. M. Kelley, Gardner Murphy).

The relation between reaction time to sound and light is dependent on the magnitude of the stimuli compared, and in addition may differ very considerably for different individuals. There is greater individual difference in reaction time to sound than to light.

Experimental Study of the Stimulus Error (Samuel W. Fernberger).

The different results obtained from the well-known experiments according as the subject's attention is directed upon the character of the stimulus or upon an analysis of his own responses, are set forth, and the significance of both attitudes discussed. It is claimed that the experiment shows the futility of a statistical treatment of a purely behaviouristic study, apart from introspective control.

Zeitschrift für Psychologie. Band 87.

Heft 1 und 2. Juni, 1921.

W. Köhlers physikalische Theorie der physiologischen Vorgänge die der Gestaltwahrnehmung zugrunde liegen (Erich Becher).

An account and criticism of W. Köhler's theory of Form. Köhler has attempted to account for the fact that the perception of a whole is other than the mere sum of the perceptions of its parts. He deals first with a physical theory of form, and secondly with the physiological events concomitant with the perception of form in audition, vision, and taste. He then proceeds to establish a relation between the first and the second.

Studien zur zentralen Transformation der Farben (Georg Marzynski).

When shadows are cast on a white ground the patch thus formed is matched with grey, and yet the white ground shows through the shadow in spite of the fact that the same amount of light stimulation can be shown to be operative in both cases. The possible objective and subjective causes of this are dealt with. Kindred problems of the relation between shadowed and dimly-lighted patches, and the matching of shadows are noticed. The bearing of Weber's law on the problem is particularly considered, and an account is given of some shadow experiments.

Über die Vorstellungswelt der Jugendlichen und den Aufbau des intellektuellen Lebens (E. R. Jaensch).

The author has edited a collection of papers on the objective world of children and the building up of the intellectual life, of which three are here presented by Edith Gottheil, E. R. and W. Jaensch, and Alfred Jösser.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY

GENERAL MEETING

October 15, 1921. The Origin of Affective Tone, by C. S. Myers.

December 10, 1921. Tests of Practical Ability, by Margaret McFarlane.

SECTIONAL MEETINGS.

Education Section.

October 4, 1921. A Basis for Experiments in Æsthetic Appreciation, by Margaret Bulley.

November 8, 1921. The Problems of Intelligence and Intelligence Tests, by J. A. Green.

Medical Section.

October 26, 1921. The Affect in Dreams, by W. H. R. Rivers.

November 23, 1921. The Castration Complex, by Douglas Bryan.

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VOLUME XII APRIL, 1922 PART 4

THE DEVELOPMENT OF THE KNOWLEDGE OF TIME IN CHILDREN.

BY E. C. OAKDEN AND MARY STURT.

§ 1. *Introduction and tests employed* (pp. 310, 311).
§ 2. *The 'questions' test* (pp. 311–316).
§ 3. *The 'order of date' test* (pp. 316–318).
§ 4. *The 'temporal absurdities' test* (pp. 318–321).
§ 5. *The 'temporal completion' test* (pp. 321–323).
§ 6. *The 'pictorial identification' test* (pp. 323–327).
§ 7. *The 'temporal order of historical characters' test* (pp. 327–329).
§ 8. *The 'temporal memory' test* (pp. 329–333).
§ 9. *The 'making an appointment' test* (p. 333).
§ 10. *Summary of experimental results* (p. 334).
§ 11 *General educational implications of the tests* (pp. 335, 336).

PREVIOUS investigations in 'time-sense' have been concerned mainly with ability to estimate and reproduce intervals of time; this reproduction being made without reference to the conventional scheme of time measurement expressed by our system of minutes, seconds, etc. Such investigations were intended to throw light on the origins and primitive forms of the time experience. In the following experiments, on the other hand, our aim has been to inquire into the knowledge and appreciation of time as measured by instruments of precision and arranged in a conventional scheme. Knowledge of this 'conventional time,' as possessed by the educated adult, is the result of a gradual process of learning—not due to some innate faculty or acquired suddenly in the course of infancy or childhood. The learning proceeds partly by contact with adults, who already know the use of the different time-words, and partly through definite teaching in such subjects as history. Indeed, the comprehension of time as measured by conventional means is a condition of the full appreciation of many school subjects, especially those with any historical elements. Therefore, our aim in the following investigations into children's knowledge of 'conventional time' is mainly educational.

§ 1. *Introduction and tests employed.*

The growing sense of time in children has been studied through their vocabulary—a method which, though probably the best yet tried, suffers from the disadvantage (especially in the youngest children) that the inability to use a word correctly does not always mean that the experience connoted by the adult use of the word is lacking. The subjects of our own experiments were chosen from the schools; they do not therefore include any children under four years of age. This objection has therefore less weight in the present case. What we have attempted to trace in these children is the growth of that sense of time which gives us the power to think in dates, to make and keep appointments, and to form plans involving temporal factors. It is obvious that in making such plans adults, at least, think directly in conventional units of time, *e.g.* minutes and seconds, and not in subjective sensations of duration.

On these problems little work seems to have been done; yet children's ignorance and incapacity in such matters are notorious. Binet's observations on the subject are well known[1]. In his 1906 scale he ascribed to children of 6 years the knowledge whether it is morning or afternoon, and to those of 9 years the knowledge of the day of the week, the month, day of month, and year. At 10 he supposes children to be able to repeat the names of the months correctly. In his study of reasoning in children[2], Burt comments on the great difficulty they seem to find in making "time syntheses," but he does not consider the matter further. The other studies of children's knowledge of time, *e.g.* those of Decroly and Degan, deal with children under the age of 5.

Our own aim being, as we have said, primarily educational, we have attempted to provide experimental evidence bearing on (*a*) the methods of history teaching, and (*b*) the age at which certain aspects of it could most profitably be begun. This seemed to involve an investigation into

(i) the child's understanding of ordinary time-words and symbols such as are in use in every-day life;

(ii) his power to form the conception of a universal time scheme extending into the past and future, and his ability to use the dates by which such a scheme is symbolized;

(iii) his knowledge of the characteristics of definite epochs in the time scheme and his ability to place these epochs roughly in the correct order;

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[1] *Development of Intelligence in Children.* Eng. Trans. 1916, pp. 206–217.

[2] *J. of Exp. Ped.* Dec. 1919.

(iv) the matter and methods used by the child in thinking about historical data;

(v) the importance attached by children to time in comparison with other elements in their experience.

The knowledge tested under the first two heads is clearly an indispensable preliminary to any profitable study of history, if history is to mean more than the mere description of ways of life without reference to chronology or causation. The third and fourth heads rather concern what has been assimilated, and have more bearing on the methods that can be employed in teaching than on the general question of the growth of the concept of time. The last head directly concerns selection and emphasis of material for teaching at different stages.

The tests used were:

A. A list of questions intended to illustrate (i) and (ii) above.

B. Lists of historical characters with dates, dealing with (ii) above.

C. An absurdity test, two completion tests, and the arrangement of people in order, in their centuries and in epochs. The last of these tests had two forms: (*a*) the names of people were given; (*b*) pictures were shown. All these were used to illustrate (iii) and (iv) above.

D. A memory test and the composition of a letter, dealing with (v) above.

§ 2. *The 'questions' test.*

This test was always given individually and orally when given to the younger children. To one set of older children, ages 8–10, it was given in class as a group test, and the answers were written.

The following is a list of the questions asked:

1. What is your age?
2. What is the date of your birthday, (*a*) month, (*b*) day of month?
3. Is it morning or afternoon? Why?
4. What day of the week is it?
5. How long would it take you to walk round this room?
6. What season is it? Why?
7. What month is it?
8. What day of the month is it?
9. What year is it?
10. In what year were you born?
11. What day is it at [*e.g.* Newmarket] now?
12. What time is it now?
13. What time is it for your mother at home?
14. Robin Hood lived in 1187. (*a*) Would your mother be alive then? (*b*) Would your grandmother?
15. Would Christ be alive then?

16. How long is it since the [*e.g.* Easter] holidays?
17. What time does School begin?
18. What time does School end?
19. Shall you come to School on Saturday?
20. How long will it be till holidays begin again?
21. (*a*) When you are [*e.g.* 10], shall you be older or younger? (*b*) Will your mother be older, younger, or the same?
22. How long have you been talking to me?

In answer to Q. 5 two estimates were required, the second (called 5*b*) after the child had actually walked round the room. Q. 13 was always introduced by—"Has your mother a clock at home?" and "Is it right?" It was actually asked in the form, "What time would she see if she looked at it now?" Q. 14 was asked thus, "Have you heard of Robin Hood?" If the answer was "No," the experimenter said, "He was a robber and lived in the forests. He lived in the year 1187. Was you mother alive then?"

In marking, four symbols were used. R = right; W = wrong; – = no answer; ! = an absurd answer. The marking of most of the questions presented no difficulty, but a few questions need special mention.

Qs. 8 and 9 had to be quite right. The children were told the date in school.

Q. 12. Five or ten minutes' latitude was allowed.

Qs. 16 and 20. Estimates of more than a week out counted as wrong.

Q. 22. An answer of five or ten minutes counted right (the tests actually took about 7 minutes); a greater or less time, *e.g.* 2 or 15 minutes, was counted wrong.

Such answers as the following were counted as 'absurd':

Q. 5. Five minutes or over.

Q. 6. Any answer that was quite wrong, *e.g.* "Nearly July, because the apples are on the trees." Any answer that was reasonable, *e.g.* "Summer, because it is so hot" (the day being a particularly hot one in March) was counted as right.

Qs. 7 and 9. Any answer apparently senseless, *e.g.* 3.

Q. 12. A time more than two hours out, or some repeated phrase that seemed to mean nothing.

Qs. 16 and 20. All periods much too long or too short, *e.g.* 5 days instead of about 2 months, or 6 or 12 months instead of 2 months.

Qs. 17 and 18. Any time more than one hour out.

Q. 22. Any time more than half-an-hour out.

The following Table shows the percentage of children answering each question rightly (R), absurdly (!), and failing to answer (–), at each age.

The percentage of children answering wrongly (W) is not shown, but can readily be calculated. The questions are arranged in order of increasing difficulty.

TABLE I.

Age	4			5			6			7			8			9			10		
No. of children	14			12			15			13			25			25			6		
% answering	R	!	–	R	!	–	R	!	–	R	!	–	R	!	–	R	!	–	R	!	–
Number and nature of question																					
1. Age	100	.	.	100	.	.	100	.	.	100	.	.	100	.	.	100	.	.	100	.	.
19. School on Saturday ...	78	.	.	91	.	.	100	.	.	100	.	.	96	.	.	100	.	.	100	.	.
3. Morning	67	.	*7*	83	.	.	100	.	.	100	.	.	100	.	.	100	.	.	100	.	.
ⓐ-24. When 10 you older ...	50	**43**	.	75	.	.	80	.	.	100	.	.	92	.	.	100	.	.	80	.	.
2. Birthday month	50	.	*29*	50	.	*50*	73	.	*20*	100	.	.	96	.	.	100	.	.	100	.	.
4. Day of week	21	.	*50*	73	.	*9*	87	.	*7*	92	.	*8*	97	.	.	88	.	.	100	.	.
14. R. H.: mother	50	.	.	66	.	.	67	.	*13*	77	.	.	92	.	.	92	.	.	100	.	.
13. Time for mother	50	.	*29*	75	.	.	60	.	*7*	84	.	*8*	84	.	.	92	.	.	100	.	.
ⓑ-21. When 10 mother older ...	14	**43**	*21*	42	.	.	87	.	.	84	.	.	88	.	.	92	.	.	67	.	.
18. What time return from school	7	**35**	*43*	33	**25**	*16*	54	.	*15*	82	.	.	91	.	.	100	.	.	100	.	.
2. Birthday: day of month ...	7	.	*86*	25	.	*75*	27	.	*60*	69	.	*31*	96	.	.	100	.	.	100	.	.
11. Day in other town	14	.	*21*	58	.	.	60	.	.	54	.	*16*	64	.	*7*	84	.	*8*	86	.	.
7. Month	7	**7**	*78*	8	.	*66*	20	**13**	*33*	77	.	*27*	83	**4**	*8*	100	.	.	100	.	.
6. Season	7	.	*78*	50	.	*42*	31	**8**	*54*	27	**9**	*53*	83	.	*8*	96	.		100	.	.
14. R. H.: grandmother ...	35	.	*7*	58	.	.	40	.	*7*	38	.	*8*	63	.	*12*	89	.		67	.	.
6. Season. Why	14	.	*78*	63	.	*35*	30	**10**	*40*	9	.	*64*	74	.	.	82	.	*4*	100	.	.
17. Time school begins	7	**43**	*29*	16	**25**	*16*	46	.	*7*	45	.	.	53	.	.	96	.	.	83	.	.
9. Year	.	**14**	*86*	.	**8**	*91*	7	**13**	*80*	46	.	*46*	84	**7**	*4*	92	.	*4*	100	.	.
15. R. H.: Jesus Christ	14	.	*7*	33	.	*16*	54	.	*7*	54	.	*8*	28	.	*4*	12	.	.	30	.	.
8. Day of month	.	**7**	*93*	.	.	*66*	7	.	*73*	31	.	*54*	48	.	*8*	80	.	.	57	.	.
16. How long since holidays ...	14	**7**	*71*	8	**33**	*25*	27	**27**	*13*	31	.	*16*	34	**14**	*4*	36	**8**	*8*	23	.	*14*
22. Time been talking	.	**7**	*78*	25	**33**	.	20	**7**	*13*	46	.	*8*	27	**3**	.	24	**8**	.	29	.	.
20. How long till holidays ...	.	**14**	*64*	.	**16**	*33*	31	**15**	*31*	.	**16**	*38*	30	**6**	*8*	36	**8**	.	71		.
10. When born	.	**7**	*93*	.	.	*100*	.	.	*100*	18	.	*82*	47	**6**	*13*	56	**12**	.	43	.	.
12. Time now	.	**35**	*64*	8	**33**	*16*	33	**7**	*20*	38	.	*46*	38	.	*10*	12	.	.	14	.	.
5. How long to walk round (*a*)	.	.	*86*	.	**33**	*8*	.	**27**	*7*	.	.	.	22	**7**	.	8	**12**	.	14	.	.
5. " " (*b*)	.	**21**	*64*	.	**12**	.	7	**27**	*40*	.	**8**	.	10	**13**	.	4	**16**	.	.	**14**	.

In estimating the ages at which the various questions can be answered, it is perhaps best to consider the columns giving absurdities and failure to answer. The 4-year-old children know very little, and their absurdities are mainly in answers to those questions which involve giving the time of day. At 5 years of age the greater number of absurdities is to be found in the answers to questions on duration; while from 7 onwards the absurdities have largely disappeared, thus showing, on our system of marking, a certain general knowledge of the terms involved. The 10-year-old group is small and came from a class in which the average age was 9; it therefore consisted of children below the average intelligence of the class. In spite of this, the 10-year-old group is on the whole better than the 9-year-old group, and shows a fairly close approximation to the adult knowledge of time.

There seems to be a slight change in the order of difficulty of the questions at different ages. This may well be due to the smallness of the groups investigated. Accidental factors have undoubtedly affected the position of certain questions[1].

To arrange the questions in order of difficulty, and to determine thus the order of the growth of knowledge, are difficult. Some facts, however, seem clear. The duration questions are clearly the hardest, so much so that they are beyond the power of most adults to answer correctly. *E.g.* Q. 5 was only answered correctly by one adult out of seven tested, and he was an amateur photographer practised in estimating short periods of time. The difficulty of Qs. 16 and 20 for the children seemed to be partly due to their inability to perform the necessary calculations, such as would be made by an adult if he could not tell 'off-hand' the length of time before a certain event. It is noteworthy that only one of the younger children made use of organized experience, saying, "I suppose the term will be three months; it usually is."

Next in degree of difficulty may be put the questions involving the time of day. Q. 12 is probably too hard for most adults; the man who can tell the time correctly without looking at his watch being generally regarded as something rather out of the common. It is noteworthy that Q. 17 proves to be much harder than Q. 18. It might be thought that this is due to the children tending to confuse the time of leaving home with that of the actual beginning of school, but for the fact that they give a time that is late quite as often as one that is early. Knowledge of other points seems to grow irregularly; though the tendency is for growth to occur outwards from the more to the less personally interesting, and from frequently recurring cycles to those of longer periodicity.

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[1] Cf. Q. 15; the children of the school which wrote the answers to these questions had clearly been taught that Jesus lives *eternally*, wherefore in almost all cases they said that He was alive at the same time as Robin Hood.

Both the day of the month and the year involve counting and this does not become easy and habitual until a child is about 7 or 8 years old[1]. The season seems to children much less a mark of time than a description of concrete material things that enter directly into his experience. Winter really means for them snow or Spring, flowers. Consequently a wrong season may be given with the right description; on a very warm March day Q. 6 may produce many answers such as, "Summer, because it is so hot." This also explains the greater difficulty children find in giving a reason for their thinking it is morning or afternoon. When a reason was given, and not merely the intuitionist's "I just know," it usually concerned personal activity, often a meal. Such answers as "The sun is setting," occur but rarely.

Perhaps the most interesting results are those bearing on the universal nature of time. Most children realise quite early that time is the same all over the same town (Q. 13 can be answered by 50 per cent. of the 4-year-old children) and also that the sequence of time in the future for others is the same as for themselves (*v.* Q. 21). But even at 10 years of age the fact that time is (practically) the same in different English towns is not realised in all cases. One 8-year-old who knew that it was Saturday in Cambridge, summed up the matter quite finally, "It would be Sunday in London, 'cos it's a different town, and there are towns where it would be Monday." She was pressed for an example. "It would be Monday in Hunstanton, 'cos that is different again." Of course, children seldom express themselves as definitely as this, but in cases where it has been possible to get the child to give a reason at all for his answer, he seems to think as did the child quoted. A difficulty may arise for him from his observation of the fact that different people's time tables vary greatly; 'baby' goes to bed at a time different from 'sister' or 'mother.' Even when 'time' is thought of as being the same within the child's circle of experience, why should it be so in places which are apparently so entirely disconnected from it?

Binet commented on the fact that children found great difficulty in naming the year, a task which appears easy to adults. This knowledge seems to be acquired suddenly, and in a way rather different from that of the other elements of the date. From 4–6 years of age there is practically no knowledge of the year. At 7 about half know, and from 8–10 nearly all. It was, however, far from certain that, because the children could give the year as part of the date, they had any real understanding of a chronological system. When they were asked to give the year of their birth the highest percentage of successful answers was 56 at age 9; but at that age and still more at 10, the children ought to have been able to make the needed calculation if they had understood the principle on which dates were arranged. The other question, 14, intended to test an appreciation of dates, did not seem to give results at all conclusive. It appeared to be Robin Hood's name, rather than the date, that suggested a period remote from ordinary experience. The description given of him as a robber, and the stories of him in the books where he was often described as living in 'the olden times,' probably often determined the right answer when it was given. If the children had made any calculation, even the simplest, there would not have been so much difference between the answers in the cases of their mother and grandmother (Q. 14 *a* and *b*). Apparently, grandmother's experience could be so remote that anything might have happened in those days. One quite clever and well taught child of 8½, who answered practically all the questions rightly, was quite unable to decide about her grandmother. "You see," she said, "Granny lived to be 80." She never made any attempt to obtain an answer from calculation. Another child, aged 6, placed Robin Hood exactly a generation back. "Mother was a baby in his time like I am now, and Granny was a young lady, like Mother is." Many other children said that he lived at a time different from Christ, but when asked whether it was before or after, they replied, "before."

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[1] Ballard, *Mental Tests*, pp. 55, 62, 71.

§ 3. *The 'order of dates' test.*

The 'questions' test showed that children make very little use of dates. This might be due to inability to perform the necessary calculations, or to ignorance of the significance of the dates themselves. We therefore gave the following test in various schools, viz. the two lowest forms of a Girls' High School, X, a Boys' Preparatory School, Y, and two Elementary Schools, A and B, the latter in a slum district. We must thank all these schools for their kindness in allowing us to carry out this and other tests.

On the black board was written:

Attila lived in Hungary in 438 A.D.

Philip lived in Spain in 1585 A.D. (or *Dante lived in Florence in* 1312 A.D.).

Nero lived in Rome in 50 A.D.

This was read out to the children and they were told to write down the names of the people in the order in which they lived, "beginning with the one who lived longest ago—furthest away back in history." The test was performed slowly and additional explanations were given if they were asked for. When the children had done this, a similar test was given containing the names of Plato, Burke and Swift. In its earliest form the test contained five names; but as only 20 per cent. of the children tested, aged 10, were capable of answering, we thought that the number should be reduced to three. The results given are for the shorter form shown above.

TABLE II.

	Attila series		*Burke* series	
Age	No. of children	% answering correctly	No. of children	% answering correctly
8	45	51	45	69
9	75	61	56	71
10	57	67	40	93
11	52	89	40	93
12	40	85	37	92
13	28	96	25	100

Ability to perform the test varied considerably at the different schools. School Y was slightly superior at all ages, though the children were rather hurried; B was much the worst.

The higher scores in the second test are mainly due to the fact that school B did not take it; but as all the schools show a slight improvement in it, a few of the errors in the first test may be due to the unfamiliarity of the task. The majority of the errors, however, show real ignorance. They were mainly inversions; and when reasons were demanded, the most usual reply was, "Philip comes first because his number is bigger." This error was, however, complicated by the fact that some children did not know when the number *was* bigger, *e.g.* in the problem below 1898 was often thought to be bigger than 1901. Indeed some children seemed to judge of the value of a date by its last figure[1]. Other mistakes arose through ignorance of 'B.C.' one child thinking that it came after 'A.D.' but on the whole there were fewer mistakes due to this than might have been expected[2]. The utter confusion of a few of the children can be gathered from the following answers to questions. M.D., aged 10, school A: "Nero lived 50 years ago, Dante 1312, therefore Dante lived first." M.E., 9, A: "Look at Dante and Nero, whose date is later?"

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[1] This is partly explained by the fact that this school adopts the modern idea of dealing with *small* numbers in arithmetical calculations. Numbers greater than 1000 are therefore unfamiliar.

[2] Cf. 'pictorial identification' test later (p. 323).

"Dante's." "Who lived longest ago then?" "Dante." P.M., 10, A: "Why do you think Attila lived after Philip?" "I just guessed, I had nothing to guide me."

The majority of children seemed vague as to the point from which years are enumerated; and even where 'B.C.' was understood, did not realise whether this point is in the future or past.

It was suggested by one teacher that the children failed because of the unfamiliar names and the remoteness of the dates. We therefore gave a problem to some of the same children which was free from these objections. It ran:

> John was born in 1898, Mary was born in 1901. Who is the older?

The results of this test were better than those of the other. With the same children they were:

Age	% answering correctly the *Attila-Burke* series	% answering correctly the *John-Mary* problem
8	48	67
9	57	70
10	50	71

If this difference is due to the cause suggested, it would be another example of the difficulty that children find in generalising their time-knowledge.

§ 4. *The 'temporal absurdities' test.*

The two tests above described show that only from about 9 years of age can the majority of children be assumed to possess a knowledge of the conventional scheme of time-marking used in every-day life and in history. Even at that age it is quite unsafe to assume that much more than a half of the class understands the principle on which chronology is based, or what is intended to be conveyed when the date of some historical character is given.

But dates are merely the scaffolding of history, and an understanding of them is quite distinct from a knowledge of the characteristics of the different periods which they denote. It seems probable that the power to understand this depends partly on age, even apart from the effects of different methods of teaching. We were told by an experienced teacher that she had given up telling stories about the remote historic past to children of 7 because they seemed quite unable to imagine a state of things when there were no trains or when the servant's 'night out' did not have to be considered. At 8 she found that they could manage this.

Given this power to think of the past as different from the present, there still seem to be at least two stages in the development of historical knowledge. The first may be called the 'negative' stage; at this stage the present is distinguished from the past by the qualities which the latter *lacks* rather than those which it possesses. Each child sums up the *positive* characteristics of the past, as it appears to him, in some short formula such as wearing sandals and skins or worshipping idols. He relegates whatever seems to him to be beyond his own experience, or the possible experience of those with whom he comes into close contact, to the dim regions of 'savagery.'

The second stage occurs only when the child begins to distinguish historical periods, and to form a picture of *successive* epochs approximating to that formed by an adult.

The following absurdity test was devised to examine the child's power of distinguishing a historical epoch from the present; and also his power of using effectively such knowledge as was investigated by the 'questions' and 'order of date' tests.

In 55 B.C. Julius Caesar arrived with his troops at Dover. They had had a stormy crossing, and now, as the sun was sinking, they splashed through the surf to the land. The Britons on the shore were singing at their $\overset{\text{A}}{\underline{\text{mid-day meal,}}}$ and did not notice the noise the Romans made until the enemy forces were only a quarter of a mile away. $\overset{\text{B}}{\underline{\text{A day's march}}}$ brought the Britons to the spot. The next day, Wednesday, the $\overset{\text{C}}{\underline{\text{30th February,}}}$ the Romans caught a British prisoner. He told them the British troops were discontented as they wished to go home at once to reap their $\overset{\text{D}}{\underline{\text{autumn harvest.}}}$ Caesar was greatly encouraged and forced a battle the next day. At dawn he offered a sacrifice. Taking off his $\overset{\text{E}}{\underline{\text{top-hat}}}$ he stood before the Altar and prayed "O, $\overset{\text{F}}{\underline{\text{Lord Jesus,}}}$ may this day, $\overset{\text{G}}{\underline{\text{Friday,}}}$ by others regarded as unlucky, prove fortunate to us." The Romans won a great victory due to their superiority in $\overset{\text{H}}{\underline{\text{gun fire.}}}$ The British chieftain was taken prisoner and shown in Caesar's triumphal procession three years later, $\overset{\text{K}}{\underline{\text{58 B.C.}}}$ His grandson is $\overset{\text{L}}{\underline{\text{still living}}}$ in a remote corner of Scotland.

This test was given with the instructions: "Here is a story. In it are many things which are wrong, nonsense or absurd. You are to underline all the things that you can find wrong or absurd, and to write in the margin why you think them so." An example was then given[1]. The actual test was given to school Y (cf. p. 316) throughout[1], to two forms of school A, ages 8–11, and to forms I, II and III of a Bristol Central School, ages 11–14. The results may thus be tabulated:

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[1] It was taken from the absurdity test given in Ballard, *Mental Tests*, p. 38.

Percentage of children scoring each point.

Age	8	9	10	11	12	13	14		
No. of children	26	51	71	55	65	66	24	Total	Average
A	27	47	49	78	86	79	88	454	65
B	8	18	30	60	66	48	75	305	44
C	35	25	8	33	37	29	21	188	27
D	27	33	41	64	66	76	50	357	51
E	50	49	54	73	83	86	83	478	67
F	15	16	25	44	48	39	58	245	35
G	23	35	34	54	48	53	46	293	42
H	31	37	59	80	95	94	100	496	71
K	27	20	20	42	43	38	12	202	29
L	42	47	61	89	89	83	83	494	70
Average	28·5	32·7	38·1	61·7	66·1	62·5	61·6		

If one considers the results obtained by children at different ages the most noticeable fact is the sharp improvement between the ages of 10 and 11. Both above and below that age there is but little improvement; at that point the score practically doubles. The results of school Y are markedly superior to those of the other schools, the average score in this case for all ages being 56 per cent., and in all the other schools together being 42·1 per cent. In spite of this, however, the order of difficulty of the points is practically identical. This shows that the test presented the same characteristic difficulties to each group, although the boys of school Y were more competent to deal with them.

If we range the points in their order of difficulty, the easiest first, they run:

H Gun fire.
L Grandson still alive.
E Caesar's top-hat.
A Sun-set and mid-day meal.
D Autumn harvest.
B Day's march.
G Friday following Wednesday.
F Christ worshipped in B.C.
K 58 B.C. 3 years after 55 B.C.
C 30th of February.

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[1] It was impossible to test the top mathematical form at Y. This meant that only two of the top classical form did the tests. As the school is predominantly classical the cleverer 13-year-olds and some of the best 12-year-olds did not do the tests. The absence of these boys influences the results of the 'J. C.' test and 'completion' test.

We can divide these points roughly into three classes. H, L, E emphasize the distinction between the past and the present, and are the easiest. A, D, B come next and have to do with time as measured by reference to natural phenomena or personal activity. The last four points, G, F, K, C, exemplify purely conventional marks of time. If we compare these results with those obtained from the 'questions' test (pp. 311–316), the correspondence is, on the whole, remarkably close. In the 'questions' test the 'day of the month' is one of the hardest—almost the hardest if we exclude the very difficult 'duration' questions; while in the above absurdity test C proves the hardest point. In both tests, points relating to the year and to the connexion of Christ with the beginning of the A.D. period occur next in order of difficulty. Points requiring a knowledge of the different seasons and the power to realise, however vaguely, when a period passes the span of life of a grandfather or grandson, came third in order of difficulty. All this seems to show that there is actually a definite order in which these elements of knowledge are acquired, and that this order can be demonstrated in different types of material.

While this test thus confirms the results of the 'questions' test, it also supports the view that the earliest distinction the child makes, is between the present and a historical past which is as different as possible from that present. This was shown to some extent by the reasons given by the children for marking the various points in the absurdity test. In addition to those points which the experimenter intended to be observed, many others were marked as absurd that were not meant to appear so, *e.g.* dating by our calendar. The comments made show a gradual change in type. Those of the younger children almost always run: "They did not have dates." "The Britons had no troops." The past appears to be considered simply as negating existing practices. As the children grow older, a positive standard of judgment appears. "Caesar had a helmet and therefore could not wear a top hat." Or, "They did not know about dates and days till the English came." In one case a boy gave a long and quite correct account of the Roman calendar. This change is noteworthy, and seems to occur mainly about 11 years of age.

§ 5. *The 'temporal completion' test.*

To investigate further the positive differentiation of epochs we used at first two 'completion' tests. The children were given sentences dealing with the dress and social life of the Romans and of the English in the time of Charles I, and were asked to complete the sentences correctly. The marking was necessarily difficult, but it was all carried out by one person and every effort was made to keep it uniform. The papers were marked in five grades G (good), F +, F (fair), F –, B (bad). To be B the completion had to bear no reference to the period indicated, *e.g.* attributing to the reign of Charles I the mud huts and human sacrifices of the ancient Britons. F showed a passable, though generally vague knowledge of the period, G a really definite one.

The following is one of the blanks used. The other was exactly the same, save that it dealt with the Romans.

1. The occupations of the English in the time of Charles I were ....
....
2. Their food consisted of ....
3. They lived in houses made of...
4. Their chief city was ....
5. When they went to war the weapons they used were ....
....
6. When they were at peace they wore on their bodies ....
7. On their heads they wore ....
8. On their feet they wore ....
9. They worshipped..............and to please him they ....
....
10. The language they spoke was ....
11. We know about them because they have left ....
and ....

The scores for school Y were as follows:

I. Romans.

Age	No. of children	% at each age scoring: F and over*	G	B
8	14	29	0	43
9	21	33	0	57
10	30	44	3	20
11	40	62	22	25
12	34	71	18	6
13	25	68	20	20

II. Charles I.

Age	No. of children	% at each age scoring: F and over*	G	B
8	14	36	0	43
9	20	25	0	55
10	33	42	9	39
11	41	56	17	15
12	36	61	17	17
13	25	76	24	12

* These figures include those scoring F, F +, and G.

The papers were on the whole good. The following is one of the best. It is from the 10-year-old son of a University historian in school Y.

1. The occupations of the English were indulging in village sports, and drinking, singing and merry-making.
2. They lived in large country houses and their tenants in smaller cottages.
3. Their food consisted of eggs, ale and butter.
4. Their chief city was London, but during the civil war, Oxford.
5. In war their weapons were swords and blunderbusses.
6. At peace they wore velvet jackets and trousers, with lace frills.
7. On their heads they wore cocked hats.
8. On their feet they wore top boots or velvet lace slippers.
9. They worshipped Christ and to please him they had church on Sunday.
10. Their language was Old English.
11. We know about them because they have left records of births, deaths and marriages.

The papers marked G were about up to this standard. The same test was given at school A to children aged 10–11. The results were surprisingly bad. No one scored G, the greatest number getting F or F – on the Romans, and B on the English. The reason for this was that the answers were in a large measure identical in the two cases, and the 'sandals' and 'idols' fitted the Romans rather better than the English. The tests were separated by a week's interval, and the difference in period was carefully pointed out. The complete failure of these children in the Charles I test is all the more remarkable as they had been receiving lessons on the social life of England in the time of the Stuarts during the whole term. Practically all the children seemed to have 'lumped' all historical periods together, giving them the general attributes of savagery. We could not be certain, however, whether the children failed in the test through absolute ignorance of the period, or only through an inability to revive their knowledge definitely enough to put it down in writing. The next test was directed to clearing up this point.

§ 6. *The 'pictorial identification' test.*

This test, given in school A to standards II, III, IV and V, was intended to show whether the children could recognize and distinguish different historical epochs, and, if they could, what were the indications they used in so doing. We also wished to see what means the children used to 'date' the different epochs, and what knowledge they had of actual dates. For this purpose a series of three pictures was shown. All were large (about 3′ × 4′), coloured, and historically accurate. As far as possible a definite historical event was avoided, and the scenes were those of the ordinary life of the period. The pictures were:

(1) Charles I with typical Cavaliers and Roundheads.
(2) Ancient Britons.
(3) A Tournament in the time of Richard I.

In connexion with each picture the children were asked to write the answers to the following questions:

I. Who do you think these people were?
II. When did they live? (Answer any way you like.)
III. What things in the picture tell you when they lived?
IV. Give the name of any man or woman who was alive when these people were alive.
V. Give the date at which these people lived. Never mind if you have given it before; give it again. If you do not know it at all, guess it, but then put 'G' by your answer.

Before the test began, a picture was shown as an example and the questions were answered orally by the class.

The results are best considered by taking together the answers to the same question for all three pictures, since by this means any differences due to freshness of acquaintance or to the skill of the teacher may cancel out. As the concentric method[1] of arrangement of the history syllabus is in force in this school, all the children should have learnt something of each of the periods on which they were questioned.

In answer to question 1 some children gave answers which were, as a matter of fact, not correct, but which could easily have been so. There was nothing in the picture to contradict such answers, though to a mind familiar with the pictures of English History, the right answer would assuredly have been suggested. For example, instead of Stuarts, French, Spanish, Roman Catholics, or "Raleigh's men" were given. Such answers were marked 'vague.'

Answers such as "Romans," "Britons," "Danes" or "Crusaders" to Q. 1 for the Charles picture, or "Romans" for the second, or "Danes" or "Napoleon's men" for the tournament picture were of course wrong.

The percentages of wrong (W), no answers (NA), vague (V) and right (R) answers to Q. 1 are given on the next page.

The drop in the 12-years' results is due to the backwardness of the children tested. They would otherwise have been in a higher form. The high scores in the answers to the questions on the Britons are probably caused by the attention that is given to this period in the Elementary Schools, and also by the greater facility with which more remote periods are differentiated. As in the 'completion' test, most absurdities were made in regard to the Stuarts, the period nearest and most like our own.

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[1] The 'concentric method' is that by which the history syllabus is so arranged that the same ground is covered in successive years, but in each year the subject is treated from different point of view, or with wider or narrower scope.

Age	8				9				10				11				12			
No. of children	10				29				55				72				25			
	W	NA	V	R	W	NA	V	R	W	NA	V	R	W	NA	V	R	W	NA	V	R
Charles I	50	20	20	10	24	34	28	14	31	14	31	24	29	15	36	20	28	24	24	24
Britons	0	20	0	80	21	3	7	69	11	7	13	69	5	3	0	92	8	0	0	92
Richard I	40	10	0	50	21	38	10	31	44	14	13	29	59	6	11	33	60	8	0	32
Average %	30	16	6	46	22	25	15	38	28	11	19	40	38	8	15	48	32	10	8	47
% of W and NA	46½				47				40				36				44			
% of V and R	53½				53				59				64				55			

The increase in the number of 'vague' answers is probably due to the child's increase in scope of historical knowledge without at the same time being able clearly to differentiate between national types. It is interesting to notice that this 'vagueness' is highest at 10 years and that it decreases at 11 with an increase in correctness. This agrees well with the more positive knowledge which appeared in the other tests at this age[1].

In answering Q. II the children chose one of four different ways of marking the period:

(*a*) By a date.

(*b*) By an epoch. This was often vague. B.C. was counted as an epoch, as were also "Stuarts," "Stone age," "Middle ages," "Olden days," "Days of Chivalry."

(*c*) By giving the name of a person they associated with the period, *e.g.* "In the time of Charles I," or "When Julius Caesar was alive," or "In Robin Hood's time."

(*d*) By saying definitely, or indefinitely, how long ago, *e.g.* "2000 years ago," or "a long time ago."

The results for Q. II are shown on p. 326.

On these results two questions arise, (i) what means did the children use to mark the period? and (ii) with what degree of correctness did they use these means?

As to (i), the most noticeable fact is the great popularity of the Date or of a Person as a time indication. Of these two the Date is the more popular at the earlier ages, but it is so frequently wrong that it cannot

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V. Results of 'Julius Caesar' test and 'completion' test, pp. 319, 321, *ante.*

have much meaning. Between the ages of 9 and 10 a change occurs and the association between an epoch and a famous person of that epoch becomes the characteristic method of 'dating.' Of the other two methods "Long ago" is popular with the youngest children, but rapidly becomes rarer as they grow older. The Epoch is always rather unusual except at 10. This result may be due to accident, or it may indicate the point at which the change from the indiscriminate giving of dates is ceasing, and knowledge is hardly yet definite enough to allow them to name a Person in the period.

Age		8					9					10				
No. of children		10					29					55				
Percentage of children answering by:—																
		D	E	M	LA	NA	D	E	M	LA	NA	D	E	M	LA	NA
Picture I	2	0 *50*	10 *0*	10 *10*	20 *0*	0	3 *28*	7 *21*	7 *3*	21 *3*	10	0 *18*	7 *15*	24 *15*	14 *4*	9
,, II	2	10 *20*	20 *0*	0 *0*	20 *0*	30	18 *14*	14 *0*	10 *14*	7 *0*	24	27 *0*	33 *0*	4 *7*	7 *5*	18
,, III	2	20 *20*	10 *10*	0 *10*	0 *30*	0	13 *21*	4 *7*	14 *3*	14 *1*	25	4 *7*	11 *11*	29 *0*	4 *24*	11
Average %		10 *30*	13 *3*	3 *7*	13 *10*	10	11 *21*	8 *9*	10 *7*	14 *1*	30	10 *8*	17 *9*	19 *7*	8 *11*	13
Incorrect + correct added		40	16	10	23	10	32	17	17	15	30	18	26	26	19	13

Age		11					12				
No. of children		72					25				
Percentage of children answering by:—											
		D	E	M	LA	NA	D	E	M	LA	NA
Picture I	2	3 *15*	11 *14*	20 *11*	3 *11*	6	0 *12*	4 *4*	76 *28*	4 *0*	4
,, II	2	56 *0*	14 *0*	11 *3*	12 *3*	3	36 *0*	16 *4*	8 *4*	20 *4*	8
,, III	2	7 *7*	11 *4*	35 *21*	4 *1*	10	0 *12*	8 *4*	28 *20*	16 *4*	8
Average %		22 *7*	12 *6*	22 *12*	6 *5*	6	12 *8*	9 *4*	37 *17*	13 *3*	7
Incorrect + correct added		29	18	34	11	6	20	13	54	16	7

D = Date. E = Epoch. M = Man. LA = Long ago. NA = No answer.

Italic figures indicate incorrect answers.

With regard to the accuracy of the answers, there is a steady and rapid improvement in those answers which determine a Date by reference to a Person. Accuracy as regards the Date itself improves greatly between the ages of 10 and 11, but for the other ages is much the same. Accuracy as regards the Epoch is more irregular, being at its best at 10 years of age. These figures depend on the actual number who attempted to answer in these ways and do not show the comparative reliabilities of the different methods of answering. If we take the percentage of accuracy on the totals, the Date gives 47 per cent., a Person 65 per cent. and the Epoch 67 per cent. The wild use of the Date by the younger children recalls the absurd answers given in the 'questions' test to the questions about the time of day by the 4- and 5-year-olds. Evidently, exact designations of time pass through a stage in which such are known but little or no meaning is attached to them.

The answers to Q. III were interesting chiefly as showing a steady increase in ability to note definite marks of a period, and as showing that it was clothes more than anything else that attracted the children's notice.

The percentage of those who indicated definite things in their answer rose from 36 at age 8 to 72 at age 12, and the average percentage of those who noticed dress, either definitely or vaguely, was 80 as compared with 16 who noticed other things. 4 per cent. gave no answer.

In Q. IV there was a steady increase in ability to give a name in connexion with a period. At 9 less than half could give any name, wrong or right, by 11 only 22 per cent. failed to answer. At 9 years of age 18 per cent. of the answers were right, and 30 per cent. wrong; at 11, 49 per cent. were right and 29 per cent. wrong. The remaining percentage in either case gave no answer. It was noticeable that all the names given were those connected with battle or warfare, with the exception of King John (two children), Guenevere (one child), Wolsey (one child). Henry VIII and Charles I were also given. These have decided military connexions.

There did not seem to be any noticeable decrease in the number of guesses which children from 8 years old to 12 years old had to make in answering Q. V. The proportions who did not guess varied between 51 and 92 per cent. Dates which were not absurd were given in from 31–40 per cent. of cases. There was no clear improvement with age.

§ 7. *The 'temporal order of historical characters' test.*

The 'picture' test gave us some idea of children's power to distinguish periods and their method of indicating a point of time by the name of some prominent person or by a date. We next wished to see (i) what methods they would adopt if asked to arrange persons in sequence; (ii) in view of their general failure in the 'picture' test to manipulate dates, and the low degree of consistency achieved, whether an arrangement on a basis of dates would give the same result as an arrangement by some other means; and (iii) what would be done in the case of people whose dates were not known.

A list of five names was therefore given to the children, and they were asked first to arrange the people in the order in which they lived, then to assign them to their proper centuries, and then to give their reasons for the original arrangement. An attempt was made to do this as a group test, but it was found unsatisfactory, and the test was then given individually. The numbers are therefore smaller, but more information was obtained. The names usually given to the children were Julius Caesar, King Alfred, Robin Hood, Charles I, Tennyson, Admiral Beattie. They were varied according to the child's knowledge.

The reasons given by the children for their order of arrangement fell into three main types. In the case of a few of the elementary school children the people were arranged in the order in which they had entered the child's experience, *e.g.* "I put Robin Hood first because I heard of him in the Infant School, and I heard of King Alfred in the first form." This type of answer was never given at school Y. The most usual type was a rough dating by epochs, "Robin Hood lived in the time when there were few towns, he lived in the forests." In its most advanced form this type involved considerable reasoning. One boy from school Y said, "Tennyson comes late in the list. I have been reading his poems and they are not old-fashioned." A third group relied on some external means of arrangement. The simplest of these means is order in the history book. "King Arthur is before Charles because he comes earlier in our book." This can be carried over to Robin Hood who usually does not occur in the books. "He lived about the time of William II, so he is later than Alfred."

The most advanced form of this method is the true arrangement by dates. Only one child, a girl aged 9, was found to use this method fully, though another used it in some degree. There did not seem to be any form of arrangement characteristic of a special age. The differences seemed rather to be due to greater or less knowledge of the special people given; dates being more frequently used when the character happened to come from "our period" than when he did not. Very much the same thing occurred when a variety of the test was tried on three adults who were asked to arrange a fairly difficult list of names. It was found that a genuine historian at once assigned the names to their centuries and then put them in order. Another, who knew very little history, arranged them exclusively on the basis of epochs, though certain epochs had a vague date attached to them, *e.g.* Renaissance, 1400. A third, whose historical qualifications were intermediate, seemed to use a mixed system, relying on dates as far as she knew them, and then using epochs. This use of epochs in the earlier stages of knowledge is quite in accord with the greater ease of the points H, L and E in the 'Julius Caesar' test.

When the children were asked to arrange in order by centuries the names they had already arranged in order, it became clear how little importance the majority attached to dates. This was shown in various ways. There was very little correlation between success in the two tasks. Children who had the order quite or practically right, might get the dates all wrong. But more striking were the differences of order between the two arrangements. Of 14 children tested at school Y, three changed the order, although their previous arrangment was on the page before them. They did not appear to regard the second arrangement as an improvement on the other. At an elementary school, of 16 children tested, aged 8 and 9, eight gave a different order with the dates. Moreover, many of the dates were quite absurd and could only have been given if they had no meaning for the children. One boy, age 9, who must have known St John from scripture lessons, dated him at 1500. Another bracketed Joan of Arc and Lloyd George as both living in 300 A.D. Another gave St John his right date, but put Robert Bruce, Robin Hood and Charles I all earlier.

There was a general tendency to put unknown people early, which applied also to people who were known by name, but with whom the children connected no period. The other well-marked tendency was to group people of the same occupation together. For example, Chaucer and Milton usually occurred in close proximity, Drake and Admiral Beattie, and on two occasions Alexander the Great and Charles I were so grouped! This tendency is, of course, well known and widespread; it is seen in the seven sages of Greece and in other half-legendary characters whose dates are approximated.

§ 8. *The 'temporal memory' test.*

Given a reasonable understanding of the nature of time-words and relations, is it possible to show that time-marks possess less significance for children than other details, and disproportionately less for children than adults? Certainly adult forgetfulness for such details as the time of a meeting is very marked, and children seem to be even more unreliable. We used two tests for an investigation of this point. The first was the reproduction of a story that contained details of various kinds, our object being to see if one type of detail was forgotten more readily than another. The second was the composition of a letter asking a friend to come out for a walk, our aim here being to see if the children would remember to name both the time and place of meeting, or would forget one more often than the other. In the memory test various stories were used, the two main ones being *Archbishop John* and *Chenklo.* In all cases the story was read through once and the children were asked to reproduce it in writing after an interval of from 20 to 30 minutes. They were told in advance that this reproduction would be required. In marking, the stories were split up into what seemed separate ideas, and these were counted as correct even if the wording was not exactly right, or if they were displaced from their proper place in the story. The 'inaccuracy' of the memory of any point was taken as the percentage of times the point was wrongly remembered in proportion to the total number of times it was remembered at all. This percentage is later called the coefficient of inaccuracy.

A B C D E
Archbishop John | was a patron of art | in Denmark. | In the summer | of 1679 |
F G H I
3 | poor boys | came to ask him to help them to learn painting. | They arrived on
J K L M
Sunday | morning | at his palace | at Copenhagen, | and said they had walked | 152
N O P
miles to see him. | The servant | said the Archbishop would be busy all day. | They
Q R R_1 S
must come again | at 6 o'clock in the evening. | They amused themselves in the public
T
gardens | which were only 100 yards | away till the Archbishop could see them. | He
U V W
gave them each £27 | and made them stay in the city | during the month of June, | to
X Y Z α
see the great works of art | preserved in the churches there. | In after years | two of
β γ δ ε
them, | William | and Martin, | became painters famous | throughout Europe. |

1 2 3 4 5
Chenklo | was a brigand | who lived in a cave | on a rock | which towered 1120 ft. |
6 7 8
above the Caspian sea. | He was only 48 | but his hair was already as white as snow. |
9 10 11
17 devoted followers | were ever at his beck and call. | The cave was stored with gold
12 13
and jewels, | but he became ever sadder and more silent. | Twice a year, | once when
14 15 16
the first swallow returned, | once on the 8th of November, | he retired to the topmost
17 18 19
peak of the mountain | and spent two days | in lonely meditation. | None knew his
20 21 22
secret, | but in his last years | he retired to a monastery | and became a devoted servant
of the Church. |

It could not be expected that the time details, however unimportant they might be, should all be found together at the bottom of the list. With stories of this nature various circumstances may affect the position of the different points. *E.g.* a certain romantic interest seemed to attach to Chenklo's hair being white, and this probably affected the number of individuals who remembered his age. In another test the results of which

Points arranged in order of difficulty, beginning with the easiest.

Archbishop John.

I. 18 children, 12–14.

No.	Point	Percentage remembering it correctly	Coefficient of inaccuracy
G	Poor boys	78	0
A	Archbishop John	72	23
H	Ask him for help	72	0
O	The servant told them	72	0
δ	Became famous painters	61	0
P	Archbishop busy	61	0
F	3	56	33
U	Gave each £27	50	25
R	**At 6 o'clock in evening**	39	0
S	*Public gardens*	39	21
α	Two of them	39	0
C	*In Denmark*	33	0
L	*At Copenhagen*	33	0
M	They had walked	33	0
T	100 *yards*	33	33
V	*In that city*	33	0
γ	Martin	33	0
I	**Sunday**	28	0
Z	**In after years**	28	0
ε	*Throughout Europe*	22	0
N	152 *miles*	22	50
B	Patron of art	22	77
D	**In summer**	17	57
J	**Morning**	17	0
W	**Month of June**	17	0
K	*At his palace*	17	0
X	Great works of art	17	0
E	**1679**	11	71
Y	*In the churches*	11	0
β	William	11	71
Q	They must come again	6	0

II. 20 adults.

No.	Point	Percentage remembering it correctly	Coefficient of inaccuracy
δ	Famous painters	95	0
C	*In Denmark*	80	0
G	Poor boys	75	12
M	They had walked	70	0
α	Two of them	65	13
U	Gave each £27	65	7
B	Patron of art	65	19
H	Ask him for help	60	0
A	Archbishop John	60	37
O	Servant told them	55	26
F	3	50	50
γ	Martin	40	20
S	*Public gardens*	35	36
P	The Archbishop busy	35	36
L	*At Copenhagen*	35	12
E	**1679**	35	50
Q	They must come again	30	0
β	William	30	33
J	**Morning**	25	0
K	*At his palace*	25	0
X	Works of art	25	16
Z	**In after years**	25	16
R	**6 o'clock in the evening**	20	0
T	100 *yards*	20	20
W	**Month of June**	15	50
N	152 *miles*	15	80
Y	*In the churches*	10	50
ε	*Throughout Europe*	10	33
D	**In the summer**	10	0
I	**Sunday**	10	0
V	*In that city*	5	0

Points in order of difficulty.

Chenklo.

I. 73 children.

3	*Lived in a cave*	84	0
11	Cave full of gold	81	2
8	Hair as white as snow	73	0
16	*Went to peak of mountain*	66	6
7	**Only 48**	64	18
6	*Caspian sea*	60	15
12	Grew sadder	53	2
21	Retired to a monastery	49	18
2	He was a brigand	41	14
9	17 devoted followers	41	47
17	**For 2 days**	40	12
22	Servant of the church	33	27
13	**Twice a year**	31	18
19	None knew his secret	30	19
10	Followers at his call	29	5
4	*On a rock*	26	14
18	In lonely meditation	25	0
20	**In his last years**	18	53
14	**When the swallow returned**	14	50
1	Chenklo	11	87
15	**8 Nov.**	10	59
5	1120 *ft.*	8	88

II. 8 adults.

9	17 followers	88	0
2	Was a brigand	75	0
11	Cave full of gold	75	0
12	Grew sadder	75	0
13	**Twice a year**	75	0
16	*Peak of mountain*	75	0
21	Retired to a monastery	75	0
3	*Lived in a cave*	63	0
19	None knew his secret	63	0
4	*On a rock*	50	0
7	**He was 48**	50	43
8	Hair as white as snow	50	0
14	**When the swallow returned**	50	43
15	**8 Nov.**	50	43
22	Servant of church	50	20
1	Chenklo	32	50
5	1120 *ft.*	32	53
6	*Caspian sea*	32	0
10	Followers at beck and call	32	0
20	**In his last years**	32	40
17	**Spent 2 days**	25	0
18	In lonely meditation	12	0

Time in heavy type. Place in italics.

were closely similar (and which are therefore not given), an age not connected with any such detail came out bottom. Moreover, some points are more closely connected with the fabric of the story than others. *E.g.* the first five points in *Archbishop John* could hardly have been missed without destroying the story. But allowing for these causes of irregularity, it can be seen from the tables that the details of time and space do form a sediment, as it were, at the bottom of the lists, and that of these two, time holds a lower place than space. The percentage of people remembering correctly the different types of points is:

		Time	Space	Others
Ch.	Children	35·4	48·8	43·3
A. J.	Children	23·1	27	45·5
A. J.	Adults	20	26	54·6
Ch.	Adults	47	50·4	55·2

It can be seen that space has always a superiority over time, and that both space and time are, except in one case, inferior to the other points taken together. In this one case it seems that the slight superiority of space is due to the fascination for children of 'life in a cave,' which heads the list.

The tables of the inaccuracy coefficients show that it is not to time or place as such that inaccuracy mainly attaches, but to all definite names and figures. With few exceptions, all the high coefficients (over 20 per cent.) belong to names or figures. In two of these cases, "patron of art," and "servant of the church," the mistakes were due to a failure to understand the phrases. The other three are marks of time: "in his last years," "in summer," and "the first swallow," and may perhaps be due to inattention to time. However, the numbers who remembered the existence of these points were small. In spite of the inaccuracy in recall, we do not find a sediment of names and figures at the bottom of the lists as we do with time and place. The mistakes in many cases suggest that it was the effort to recall the details *exactly* that proved difficult. The figures were represented by some vague phrase, "some poor boys" taking the place of "three," or "one day" taking the place of "in summer." It was not rare to get an "about" added to the round number, *e.g.* "about 1100 ft." as if the writer were rebuking the experimenter for being unnecessarily precise.

From this test then we can get some experimental evidence of the observed fact that time details are remembered with more difficulty than others, but the evidence does not show that this inability to remember is more marked in children of ages 12–14, the ages tested, than in adults. Such inability would probably have been shown more clearly if we had used younger children, but the difficulty of using the same tests on both groups would then have been too great. If, as was suggested above, time-knowledge begins to assume its adult form at about 11, this would account for the result.

The last test was intended to illustrate the same point, when the problem was not one of memory, but the formation of plans of future action.

§ 9. *The 'making an appointment' test.*

This test gave much more definite results. The instructions to the children were: "Write a letter to a friend arranging to go out for a walk with him." The letters were mostly quite short, and the greater number made a definite appointment to meet. In these cases it was possible to see how many gave both time and place of meeting, and how many forgot one or the other. Of 153 children of various ages tested, 128 gave a definite place of meeting, only 94 gave a definite time. Of the others, however, most gave an indefinite or inadequate time, *e.g.* to-morrow, or Tuesday afternoon. Very few gave absolutely no time indication at all. There is a slight improvement with age up to about 13. Owing to the conditions of the test the 14-year-olds were more stupid than the others. The numbers at 11 years of age are too small to be reliable.

Age	No. of children	% giving Time	% giving Place	Difference place-time
8	12	33	92	59
9	23	61	87	26
10	25	60	88	28
11	7	86	71	−15
12	32	72	94	22
13	32	59	66	7
14	22	59	87	28

Some children gave both time and place.

From these letters it seems possible to imagine how the plan presented itself to the children. In one case they thought of a vacant afternoon to fill, and the letters read, "Can you come for a walk on Saturday afternoon?" In others the object of the walk was more prominently in mind, "Can you come for a walk to Leigh woods?" Of the two the latter was the more likely to lead to the making of a definite appointment; in the former the writer seemed apt to give no further time indication, apparently feeling that what was so clear to him must be equally clear to his friend. We can conclude then, that in forming plans, time is less important to children than space, and that the tendency is very strong for a vague time to take the place of a definite one.

§ 10. *Summary of experimental results.*

From these experiments we can draw some general conclusions as to the growth of children's knowledge of conventional time.

(1) This growth is essentially a slow process, already beginning by the age at which our experiments started—four years—and arriving nearly at the adult level at about 13 or 14 years. The most important period in its development seems to be at about the age of 11, when there appears to be a rapid improvement in all types of time-knowledge.

(2) Children appear to learn first the meaning of the time-words in ordinary use. The order in which these are acquired appears to depend (*a*) on the periodicity of the cycle in which they occur, *i.e.* the parts of the day are learnt before the days of the week, and the weeks before the year; (*b*) on the personal interest involved, *e.g.* the date of a birthday is learnt early, also the occasion of such events as the weekly holiday.

(3) The day of the month always presents great difficulties, probably because of the highly artificial method of numbering and the lack of distinguishing activities for the different days.

(4) Marks of time are closely connected with activities or concrete experiences. *E.g.* the seasons are identified with characteristic natural phenomena, and such absurdities as B (day's march) in the 'Julius Caesar' test are comparatively easy of detection.

(5) Passing beyond the ordinary time-words to an understanding of chronology and the arrangement of historical epochs is a matter of some difficulty. Children find it difficult to arrange dates correctly, and apparently attach little importance to them when required to assign them to persons, or to arrange historical characters under their proper dates. When required to 'date' an epoch they prefer to give the name of some famous contemporary character.

(6) In dealing with historical epochs those which are most remote from our own time are most readily distinguished. This seems to be due to the fact that the earliest distinction is between the present and a past, which is mainly negatively characterized. In this past, subdivisions are at first ignored: they are only attended to after the age of 11 years. Children naturally find it easier to recognize the representation of an epoch, *e.g.* by a picture, than to give a description of one themselves.

(7) Indications of time appear to play a less important part in children's thought than do those of space.

§ 11. *General educational implications of the tests.*

The number of children tested is obviously too small to admit of drawing dogmatic conclusions; but a few hints as to history syllabuses, and methods can perhaps be gleaned, although it must be realised that these experiments tested the results of teaching, as well as natural interest and preference.

In the first place, it seems that up to 11 years of age in the elementary school, *i.e.* up to about standard IV or V, conventional names of time-periods, especially dates, have little meaning for children unless they are definitely explained and correlated with the child's own activities; *e.g.* when a week, or a year, or two years, or twenty years, is mentioned, it should be connected with the life of the child, mother, family, etc. Much might be done if the periods of time used in 'time' sums (usually taught in standard III) were made more meaningful. It is easy to show what a second or a minute means; and a little discussion on how long lessons last, how long school time lasts, how long since and before holidays, would all educate children in this way. Games and sports involving 'timing' are also helpful here. Definite and direct teaching on 'dates' is required whenever they are given in the lower standards. In view, however, of the apparent lack of interest in such conventional names, it seems that the picturesque and descriptive aspects of history are those which should receive most attention. Most of the time should be given to making the past as vivid as possible in concrete detail.

Since children at this stage seem to notice chiefly what people in bygone ages had *not*, rather than what they *had*, it seems that more emphasis should be laid on *positive* detail. The method should not be entirely that of noting the contrast between past and present, but should aim at building up a vivid and real conception of the past. The contrast is obvious to the child in proportion as both past and present become vividly known in detail.

Pictures, models, charts, dramatization, handicrafts, are all a means to this end. The chief fault which the writers have noticed in this connexion is the narrowness of scope in the details of previous epochs which are taught. This fault is most noticeable in history dating from the Christian era; it is not so apparent in the more primitive history.

Little ability to conceive of continuity and development is apparent up to 11 years of age. This should be left to the higher classes; but in the opinion of the experimenters time charts could be much more extensively used than they are at present. The concrete, pictorial representation of a conventional and abstract thing is thus given to the child[1]. In no school where these tests were carried out was a time chart used.

Since in many elementary school children there was a tendency to place epochs, events, and people in the order in which they had appeared in the child's own consciousness, and not in the real chronological order, the concentric method can perhaps be called in question; especially since the primitive and early historical periods appear to be most interesting to children. The use of a time chart might counterbalance the dangers of the concentric method.

The apparent development of a keen interest in people at the age of 10–11 years is also significant. In the experience of the writer it is then that 'heroes' either of fact or fiction evoke most admiration from the child. The second stage of history teaching, then, should centre in biography, as the first in intimate concrete details of typical historical periods.

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[1] A useful example is described by H. Madely, *School as a Training for Citizenship*, p. 38.

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THE SCHOOL IN THE SERVICE OF VOCATIONAL STUDY[1].

By OTTO LIPMANN.

(*From the Institut für angewandte Psychologie, Berlin.*)

I. *The need for determining the nature and degree of endowment* (pp. 337, 338).
II. *Endowment includes capacities and interests. The 'material' and 'formal' aspects of endowment must be distinguished* (pp. 338–341).
III. *Quantitative and qualitative classifications of vocations* (pp. 341–343).
IV. *The value of such classifications for vocational guidance* (pp. 343, 344).
V. *The scope and methods of vocational diagnosis* (pp. 344–348).
VI. *The functions of the school in vocational diagnosis* (pp. 349–351).

I.

We may distinguish a quantitative and a qualitative aspect of vocational study. The one seeks to establish the *degree* of individual endowments, the other the specific *nature* of these endowments. It has been assumed that the degree of the individual's endowments should constitute the only criterion as to whether the individual should in later life be called upon to adopt a higher, middle or lower vocation, individuals being accordingly drafted to such schools (in Germany *Gymnasium*, *Realschule* and *Volksschule* respectively) as would afford an adequate preparation for the corresponding vocations: while a knowledge of the *nature* of an individual's endowments has been supposed to be of importance for the choice of vocation within the general range of occupations suitable for the particular degree of endowment that the individual may possess.

The problem, however, is not so simple. We have largely ceased to regard schools as means of preparation for different classes of occupation; the idea of 'unitary schools' (*Einheitsschule*) is to a large extent now understood not only in the sense that schools should cease to represent any particular social class and that transference from one kind of school to another should be made easier, but also as signifying that the recognised demand for differentiation should be met not by a differentiation of pupils according to schools, but by a differentiation of pupils within each single school. A separation of pupils according to their degree of endowment would have as its consequences: first, that those classes which were denuded of the more gifted scholars would be deprived of the stimulating influence of leading personalities; secondly, that those vocations which are recruited from the lower schools, especially therefore the industrial and manufacturing occupations, would have to abandon all their leading personalities to the higher professions. Thirdly, there exists also the general question whether the concept of endowment can be completely expressed in quantitative terms, or whether we should not rather from the beginning distinguish certain qualitative types of endowment, such as practical and theoretical types, linguistic and mathematical types, etc. The idea of the 'unitary school' is at present therefore often understood in the sense that the pupils should be grouped according to the degree and nature of their endowments, and furthermore that the constitution of the groups should vary according to the subject of study; that one and the same pupil, for instance, may belong to a less advanced group in the case of linguistic studies than he does in the case of mathematics.

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[1] Translated by J. C. Flügel.

II.

Alike for these schools of the future and our schools of the present day there exists the necessity of forming a judgment as to the nature and degree of a pupil's endowments. Endowment includes both ability itself and willingness to use this ability, in other words both capacities and interests (in which statement we understand by interest the active and effective interests only). In the case of interest it is even more evident than in the case of abilities that nature and degree cannot be considered separately. Of a very few persons only can we say that they display a generally strong or generally feeble interest; in the great majority of cases preference is given to *one* field of interest, or else one or more fields of interest are neglected; *i.e.* the degree of interest differs according to the nature of the field in which it is exercised.

In considering the different kinds of endowment, with regard both to ability and interest, it is possible to distinguish the 'material' and 'formal' aspects of endowment. By the material aspects we understand the direction of endowment towards those fields of knowledge or activity, which as the result of our civilisation have developed into fixed 'professions' or 'occupations'; in this sense we may distinguish abilities and interests for mathematics, languages, music, sculpture, electrical engineering, architecture, carpentry, shoemaking, etc. It can *not* be the function of the school to establish the existence of these material endowments, *i.e.* to enter on vocational study in the narrower sense of the term, and this for several reasons.

A. The school has no opportunity to observe the behaviour of pupils with regard to these material fields of endowment. This applies both to the various handicrafts and commercial occupations and also to the arts and sciences: the work in school subjects often differs, not only in fact but even in principle, from work in the corresponding branches of knowledge and science. The science of higher mathematics has but few points of contact with the elementary mathematics which is taught in school. The way in which natural science and linguistic science are treated in school gives the pupil no conception of the scientific treatment of these subjects: though it is of course true that the 'work school' (*Arbeitsschule*) may help to bridge over this gap.

B. It would in many cases be difficult if not impossible to determine unequivocally the degree of endowment for any one of these material fields, since endowment is composed of both abilities and interests, and these exhibit no necessary correspondence one to another so far as these material fields are concerned. I need only call to mind Goethe's interest in pictorial art—an interest that was quite disproportionate to his productive abilities in this field. Many similar instances could be given; as regards myself for instance, I may say that I have no ability, even of a purely receptive kind, at all corresponding to my interest in higher mathematics, while on the other hand a certain linguistic ability that I possess is unaccompanied by any corresponding interest in the acquirement of foreign languages.

C. It is only within very narrow limits possible to predict from the presence of such endowments as can be diagnosed at an early age, the preponderance of similar endowments in later life. This is true more especially of the direction of *interests*, which, as is well known, are in youth liable to frequent change as regards their material aspects and sometimes undergo complete reversal. But even the material *abilities*, *i.e.* just those factors which in the opinion of many constitute vocational aptitude, are exposed to change as a result both of development and of the actual exercise of a vocation—changes the extent of which is at present so little known that conclusions made in early youth as to supposed vocational aptitude can possess only a very limited value.

We do not wish to deny that for the successful practice of certain vocations a certain measure of such material abilities, *e.g.* visual discrimination, dexterity, attention, memory, etc., is necessary: but in addition to the already mentioned difficulties of diagnosing these abilities in youth, there has also to be taken into account, so far as the school is concerned in this diagnosis (that is in so far as we are dealing with the choice of a suitable career for a given individual, and not with the selection of suitable individuals for a given career), the further fact that the relevant psychological concepts are by no means unequivocal. The question whether there is *one* memory or whether there are different memories for different fields, each of which may undergo a different degree of development, is one that has long been under discussion in psychology; and it is at any rate questionable whether we ought not similarly to distinguish between different kinds of visual discrimination, attention, or even dexterity[1].

The position is more hopeful as regards psycho-physics, *i.e.* in matters concerned with purely sensory functions, such as visual, auditory and tactual acuity; but psychology is only partially concerned with the measurement of these functions, which—as in the case of general health, heart, lungs, etc.—fall more within the province of the doctor, that is, so far as the school is concerned, of the school doctor. The doctor may however be very considerably assisted in his work by the teacher, who may draw attention to any defects he has observed in his pupils.

For the reasons which we have already considered it is impossible, in dealing with the choice of vocation, to attach any considerable importance to the supposed interest of the young person for this or that particular vocation; since this interest is in the great majority of cases determined purely by the material aspects of the vocation, *i.e.* it is associated with the *material* or *products* connected with the vocation, such as shoes, cakes, houses, electrical apparatus, etc., and not with the actual *nature* of the work with which young people are for the most part quite unfamiliar.

We may therefore conclude that any determination of vocational ability undertaken with a view to the choice of occupation on the part of those who are still young must aim at a diagnosis of the 'formal' endowments only. This is a task which should be performed by the school, if it undertakes to give advice as regards the choice of occupation; it is a task too, which it should be possible for the school to carry through to a very considerable extent.

We must now endeavour to make clear what is to be understood by

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[1] In view of the generally assumed close relationship between the requirements of *particular* vocations and certain *definite* forms of these psychical characteristics, we may include these abilities among the *material* endowments.

'formal' endowments. I am not in a position at present to make more than a rough preliminary sketch of this field, since the investigation of endowments, so far as it is connected with the study of vocational ability, has hardly yet approached the subject of formal endowments. What I have to say on the matter is therefore far more in the nature of a programme for research than of a report of the results of research.

III.

In dealing with formal endowments, we may retain the customary division into higher, middle and lower vocations, though in doing so we must use these concepts in a definite and special sense. The height of any vocation in our scale is determined by the extent to which the worker in that vocation acts 'freely' in virtue of his intelligence or imagination, working little with fixed rules and within fixed limits.

It is clear that some vocations admit of such 'free' action to a greater extent than others—the freedom being as a rule greater in learned and artistic than in industrial occupations—and that some men are more capable and more inclined than others to work on individual lines. A man is the more adapted to a vocation which permits or demands work of this free and individual kind, the greater his intelligence and imagination (both of which are 'formal' characteristics) and the greater his interest in using the abilities for the free manipulation of given material. Men of this kind should avoid vocations in which the nature of the work is prescribed in all its details, though it is far from being necessary that they should all adopt a vocation of academic or artistic nature; since commercial and manufacturing vocations are also capable of being developed into higher professions. Such men, on the other hand, as are lacking in the characteristics we have just described should of course avoid the learned and more especially, the artistic vocations.

Alongside of this more or less quantitative classification of vocations, it is possible to make qualitative classifications, and this from various points of view. Without going into detail, a few such classifications may be enumerated here. All the categories to be mentioned refer to formal endowments, both as regards abilities and interests. Among the oldest of such classifications—one that is usually confined to the higher professions but which holds good also for many vocations of intermediate standing—is that which divides activities into those of a 'productive,' 'reproductive' and 'receptive' nature. Another classification, related to the above, regards vocational activities as 'gnostic' (aiming at knowledge), 'technical' (aiming at the achievement of results) or 'symbolic' (representing the inner life of man, artistic). We may further distinguish 'theoretical' and 'practical' vocations, according as they demand—in general or with special reference to the higher professions—more theoretical or practical intelligence[1].

This last-mentioned classification of vocational activities and the corresponding endowments constitutes in some respects a transition to the material point of view. This is true perhaps to an even greater extent of the further classification which treats occupations under different heads according as they deal with persons, things or thoughts.

The value of the classifications we have so far mentioned lies chiefly, though not exclusively, in their application to the higher professions. The following classifications are perhaps of greater importance in dealing with the occupations of intermediate rank. These vocations are classified according as they involve more manual work or more mental work; exact reproduction of given models or work of a 'freer' kind; rough or fine work; work that is executed under detailed instruction or work that is executed independently; long continuous processes or frequent short processes; frequent repetition of the same activity or frequent change of activity; concentration of attention on a narrow field or distribution of attention over many details; work in which each individual acts alone or work in which he acts in intimate cooperation with others; solitude or companionship with one or several fellow-workers; work out of doors or work indoors (room, factory, machine shop, etc.). It is difficult to bring all these points of view within a single system. An attempt at such a system is that which has been made by the *Institut d'Orientació Professional* in Barcelona. In this the vocations are classified according to the types of intelligence, temperament and character that they require. Work that is principally 'mental' in nature requires abstract intelligence, 'psycho-physical' work requires verbal intelligence, while work that is principally 'muscular' requires technical intelligence. Temperaments which easily admit of automatization are fitted for work of a *fixed* and *definite* kind, in which the various processes are constantly repeated; while temperaments of the opposite type are suited to work of a more *variable* kind. Work is further of a 'perceptual,' 'perceptuo-motor' or 'motor' character, and this last classification is made to correspond to similar types of character.

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[1] Theoretical and practical intelligence differ principally, according to our view, in the two following respects.—(i) In a task which involves theoretical intelligence, the understanding of the task itself makes demands upon intelligence, whereas practical intelligence is concerned only with the execution of the task. (ii) In tasks demanding only practical intelligence both the starting point and the goal are usually given in terms of *perception*, whereas in tasks demanding theoretical intelligence, these are mostly given in terms of *thought*.

These various formal characteristics of a profession do not of course constitute a complete 'vocational picture,' but they do include the majority of the essential points involved in the choice of vocation. Vocational study must therefore be based on the investigation of the endowments corresponding to these various vocational characteristics.

IV.

Before we enter upon the problem as to what means exist in the school for the determination of these types of endowment, there remains a question of fundamental importance to be settled. It is clear that not everyone can be easily assigned to one or other of the above-mentioned types, and this is true also of some of the young persons in need of vocational advice. What significance then have the various characteristics that we may have assigned to the various occupations, for the determination of mental traits with a view to vocational advice? In my opinion the importance of these characteristics lies principally in this; that with their help we are enabled to recognise, on the one hand those who are specially suited (or so to speak predestined) for particular occupations, on the other hand those who are quite unsuited for particular occupations. Any determination of traits based upon these characteristics is not quantitative in the sense that the presence of a characteristic in a particular degree implies a precisely corresponding degree of suitability for a profession. These characteristics permit rather only of a differentiation of individuals into those who are specially suitable, indifferent and definitely unsuitable; and it must be added that the group of indifferent individuals will be by far the largest of the three. Not all occupations demand specific qualities, and not all men possess specific endowments. Vocational Psychology must needs concentrate itself on the relatively few remaining occupations and individuals, in which specific qualities (positive or negative) are needed or are present.

Quantitative determination of vocational aptitudes is of course within the bounds of possibility, but this will be a matter of definite selection rather than of guidance and advice, and therefore falls no longer within the province of the school. Quantitative degrees of vocational aptitude, especially those within the group of indifferent individuals, would relate in the first place to certain of the above-mentioned material abilities, certain forms of visual discrimination, dexterity, power of optical combination, etc., in the second place to general intelligence, theoretical and practical. I am indeed inclined to attribute greater importance to intelligence than to the material and more elementary specific abilities, that is at least where the above-mentioned conditions with regard to formal qualities are fulfilled; since an intelligent person will often be in a position to compensate for the lack of such specific abilities as may be insufficiently developed, by his use of other abilities of a different kind. Such vicarious function however is only possible where the vocation is of such a nature as to admit of a certain degree of modification to suit individual needs and capacities, that is, is capable of being developed into something resembling the 'higher' professions. It follows from this that the degree in which the material specific abilities are present is of greater importance for vocational selection, the lower and more mechanical is the class of vocation concerned; while the degree of intelligence, on the other hand, becomes of ever greater significance with increasing 'freedom' of the occupation. From this point of view, therefore, it is possible to establish a scale of vocations, which would indicate what degree of intelligence is required or is utilisable at any particular point of the scale; such vocational scales have already been drawn up in America in connexion with the army intelligence tests.

It is the aim of vocational diagnosis to discover the nature of an individual's endowments before his professional career has actually begun. In so far as the relative endowments are of a formal nature, there are many opportunities of different kinds for their detection; but in so far as they are of a material nature, they can be recognised only with the help of a more or less exact reproduction or imitation of the relevant vocational activity—either as a whole or in its constituent parts.

V.

The methods of vocational diagnosis may be classified according to various points of view:

I. The behaviour of the subject may be:
  (1) spontaneous,
  (2) reactive; in which case the procedure may be:
    (*a*) unsystematic,
    (*b*) systematic,
      (α) individual examinations,
      (β) collective examinations.

II. The behaviour may be studied:
  (1) by introspection,
  (2) by external observation, on the part of:
    (*a*) the teacher,
    (*b*) the parents,
  (3) by quantitative measurements of ability.
III. The results of the study may be assessed:
  (1) by self-judgment,
  (2) by external judgment, on the part of:
    (*a*) the parents,
    (*b*) the teacher,
    (*c*) the vocational advisor.

We will here examine more carefully a few of these possibilities.

Great importance, especially for the diagnosis of formal endowments (abilities and interests), attaches to spontaneous behaviour. Has the young person any spontaneous interests; if so of what kind? To what kind of activities do they lead him? What are his achievements in the fields of his spontaneous interests? We have here to rely, at any rate to some extent, upon the child's own statements, especially if we extend our questions, as we usefully may, so as to discover what interests he would *like* to call into play, if he had the necessary opportunity. Apart from this last additional field of inquiry however, much useful material may be gained from observations on the part of the parents; though the *evaluation* of such material, both that which relates to the interests and (more especially) that which relates to spontaneous activities, must be left to some more objective source of judgment. Parents have often no objective standard of comparison, and they are for obvious reasons frequently inclined to overestimate both the strength of the interests and the quality of the performance. Even less reliance can be placed upon the introspections of the young people themselves. We must not ask a child what interests him, but rather what he does, or what he would like to do. It must not be forgotten, moreover, that every judgment represents the conclusion of a process of induction based upon individual observations, and that the question as to whether the available data justify the conclusions is one of great difficulty, one which must certainly not be left to the discretion of the children themselves, nor (in most cases) to that of the parents, and which can be entrusted even to the teacher only under reservation.

Of principal importance for the school itself is the study of the reactive behaviour of the pupil. Such study must rely chiefly on casual opportunities for observation in the course of school life, not only in the class room but also during free intervals, school excursions, etc. We are here concerned not so much with the interests shown in the various subjects of instruction as with the above-described formal types of endowment. So far as material endowments are concerned, new opportunities for observation should, as we have already indicated, result from such modifications in the existing methods of instruction as are contemplated in the 'work schools.' As we have also mentioned above, the material interests directed towards definite occupations are often accompanied by lack of knowledge concerning the real nature of the activities involved in these occupations. Here the school can also be of assistance by giving pupils the opportunity of acquiring this knowledge. This may be done in three ways:

A. By actual instruction; the nature of vocational activities being made a subject of school study. For the reasons already given, the description of physical products and processes is for this purpose less important than that of the actual nature of the work involved in the vocation.

B. This is the case also where the instruction is accompanied by illustrative material, such as can be provided, for example, by the cinematograph. Such 'vocational films' however must in the first place be more than mere 'propaganda films': they must on the contrary give a true picture of the difficulties of the work. In the second place they must illustrate not so much the processes performed by the *machines* as the duties of the *workmen*. The so-called vocational films which have been produced up to the present are seriously deficient in these two respects. The cinematograph could nevertheless be so employed as to be of very great assistance in this field.

C. Better than abstract teaching, better even than teaching with the help of illustration, is instruction by means of actual practice. The school could be of immense service to vocational diagnosis, if it possessed workshops of its own, in which pupils could learn from their own experience the nature of the principal industrial and manufacturing activities. The actual performances of the pupils in different fields could then be objectively determined, while at the same time—and this I consider even more important—the pupils themselves would have an opportunity of judging which occupations they found pleasant and which disagreeable.

There is no sharp division but rather a continuous transition between casual and unsystematic opportunities of observation on the one hand

and systematic experimentation on the other. Every verbal testing of a pupil in class may of course be regarded as an individual experiment *in nuce*, while all written work or home work represents a sort of mass experiment. The difference between experiments of this kind and experiments in the strict sense—psychological tests in particular—lies, first, in the relatively inexact form, the less strict conditions of testing, the less objective estimation of results that characterize the school tests; secondly, in the nature of what is tested. School tests are essentially tests of knowledge, while psychological tests aim at measuring ability. While this distinction between school tests and the tests of experimental psychology is true in fact, it does not correspond, or at most corresponds only in part, to any *necessary* difference in the circumstances of the two kinds of experiment.

We must of course deprecate any attempt to turn the school into a psychological laboratory; the school must never lose sight of its own purposes—instruction and education, and everything the school does must, directly or indirectly, have reference to these two ends. While it is at liberty to undertake the task of diagnosing the endowments of its pupils, it is only justified in doing this in so far as this task has a bearing on instruction and education. We should be carefully upon our guard lest, neglectful of this principle, the school should overstep these limits; as would be the case if it were to place itself *directly* in the service of vocational diagnosis.

Nevertheless, a great portion of such diagnostic material as the school may collect for its own purposes, can be made of use for purposes of vocational guidance; and it is to the advantage both of the school itself and of the wider interests extending beyond the limits of the school, that this diagnostic material should be obtained by the exact methods of psychological experimentation and should not be confined to the mere examination of the pupil's knowledge. Attempts in this direction have indeed already been made. Lessons in drawing can, for instance, quite legitimately be made to include exact measurements in visual accuracy, especially since such diagnostic 'experiments' can easily be made to take the form of 'exercises.' Similarly in geometry, it is possible to use problems of the kind that are known in psychology as tests of the power of visual combination. In linguistic examinations, use can be made of the definition test, the three-word test, the word-completion test, the criticism test, etc. Even certain association tests can be employed here, such as those which consist in supplying a superordinate, coordinate or subordinate concept, the finding of parts for given wholes or wholes for parts, suitable adjectives or verbs for given substantives, etc. In general it is advisable to use also for purposes of examination problems of a kind that have not yet been used in the course of the actual instruction, but which can be solved by methods analogous to those that have already been practised: in this way the examination ceases to be a mere test of knowledge and becomes a test of ability. As to fields where this is possible, I need only refer to arithmetic, where analogy problems of this kind can easily be arranged in any number, and to grammar (both of the mother tongue and of foreign tongues), where the rules of declension and conjugation can be applied to new words in the case of which there has been no previous practice.

It is perhaps unnecessary to enter into the question of the respective merits of individual and mass examination, since both kinds are always employed in schools. The mass examination in the shape of written class work has the advantage that the same questions can be given to all the pupils, so that an exact comparison of the results becomes possible. All mass experiments suffer however from the disadvantage that it is impossible to study the different methods of work adopted by the individual pupils. Such study is moreover of great importance, since two results of equal merit may have been achieved in very different ways, and since the comparison of different methods of working may lead to practical conclusions of the greatest significance. The aim of our tests does not lie merely in the obtaining of numerical results and in the indication of a pupil's merit by means of a figure. Much more important even here are the qualitative observations that may be made during the tests. It is desirable that all instructions for the carrying out of tests should contain a list of the points in the behaviour of the subject which the experimenter should specially observe while the former is engaged on the test.

Although the possibility of making such observations confers an immense advantage on the individual test, we must on the other hand not overlook the fact that in these tests it is often impossible to give the same experimental material to all the pupils who are to be tested. This drawback can and will be gradually diminished, in so far as we are able to have at our disposition a large quantity of standardized material for use in our tests. The results obtained by any individual subject will then be compared, not with those of other subjects tested on the same occasion, but with a group of results obtained at other times and other places.

VI.

All this applies not only to tests within the school, but quite generally to all examination for purposes of vocational diagnosis. School tests have the disadvantage that they are not carried out by psychologists but by teachers. Even outside the school, however, vocational tests are, unfortunately, not always conducted by psychologists; and the teacher has at any rate the advantage over the engineer that he is accustomed to young people. Such psychological training also, as is necessary for the conduct of psychological experiments, is, in virtue of his professional education, more easily acquired by the teacher than by the engineer. Further, test experiments of the kind we have been considering must be made to fit into the general plan of the school, and must be carried out so often that, in the first place, the errors of the individual measurements will cancel one another, and, in the second place, the teachers will in the course of time acquire a mastery of the *technique* of experimentation. Finally, as already said, we attribute the chief importance, not to the numerical results of the tests, but to the observations that accompany them. The evaluation of these results and observations must of course not be left to the teacher but entrusted to the expert vocational advisor.

The great advantage of school tests as compared with those performed outside the school lies in the fact that the school need not be content with a single experiment, but can repeat the tests again and again and can observe the behaviour of pupils over very long periods.

As the result of work done in the *Institut für angewandte Psychologie*, I am inclined to be very sceptical as regards the diagnostic and prognostic value of single experiments. In so far, therefore, as single experiments may be absolutely necessary for purposes of selection, we must insist that the experimental results shall be supplemented by observations made in school. The *Hamburg Psychological Laboratory* has obtained excellent results from such a combination of experimental measurements and school observations.

In order to increase the value of these latter, the observations should not be made *ad hoc* just before the examination, but wherever possible should extend over the whole school period and should be undertaken and recorded not by one teacher only but by all those who have had to deal with the pupil at his various stages of development. As shown by our earlier considerations, however, the observations need only be made with reference to such (formal) qualities as are clearly related to certain types of endowment, and therefore also to such individuals only as show themselves clearly to belong to one or other particular type of endowment. The demands made upon the teacher would not therefore seem to be excessive. It will be well to provide the teacher with a definite set of instructions as to how the observations should be carried out (several provisional sets of instructions of this kind have already been drawn up) and to make it part of his official duty to carry out and record his observations in accordance with these instructions. The extent to which the observations should relate to the pupils' spontaneous behaviour respectively may well be left to the discretion of the teacher. In every case, however, he should confine himself as far as possible to the collection of facts, leaving to the vocational advisor the task of drawing conclusions from these facts.

Under no circumstances should the school itself undertake vocational advice. Its only function in this respect is to provide vocational advisors with a part of the material upon which their advice must be based—a part only, for such facts concerning individuals as the school can furnish do not constitute the whole of the data required by the advisors, who must review these facts in the light of the knowledge of the psychological requirements of the various professions and training centres, and who must also bear in mind the economic factors concerned in the choice of a profession, the future opportunities presented by various careers, the economic circumstances of the moment, etc. It is the special professional duty of the vocational advisor to be well-informed on all these matters, and it would mean that a double duty would have to be undertaken by the teacher, were the school itself to attempt vocational advice. Such a course seems to me impossible, especially for the reason that vocational advice is necessarily concerned not only with professional activities but also with the process of training. Many of the qualities which can and should be made subjects of observation in school are of importance not only for the choice of vocation but also, and in an even higher degree, for the selection of a suitable place of training. Thus a pupil who is adapted to solitary and individual work had best be apprenticed to a private craftsman, while another who easily adapts himself to a group of fellow workers and who is generally of a sociable character will do better in a factory. Further, experience has shown that vocational advice should be accompanied by the recommendation of a suitable training centre, since the former when unaccompanied by the latter is left hanging in the air without the means of being put into immediate execution and is often totally neglected; in the absence of such recommendation, the pupil will frequently adopt any means of training that may happen to present itself, even if he is thereby adopting the most unsuitable career that could be imagined; or else he enters upon some unskilled occupation that requires no training.

Herein also, as I should like to emphasize by way of conclusion, the school may perform a very valuable service in the cause of vocational guidance, in that it may impress upon its pupils before their departure the general importance of vocational advice, may urge them not to decide upon the choice of a career without consulting an expert who will be able to give advice based upon a knowledge of the pupils' characteristics and of the actual economic situation, and may warn them not to leave this momentous decision (a decision that may involve the success or failure of their whole life) to chance circumstances, to a passing mood, to the suggestions of an unqualified advisor or to the influence of mere custom or convention.

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THE DEFINITION OF THE WORD AND THE SENTENCE[1].

By ALAN H. GARDINER.

It ought not to have been very difficult, one would have thought, to analyse the difference between a word and a sentence. Everyone, whether philologist or layman, knows that these two have, so to say, a quite distinct 'feel' about them; 'yonder house,' or simply 'house,' yields no satisfaction, while 'yonder house is Tom's' does so, and it seems reasonable to suppose that the presence or absence of satisfaction is a vital feature in the distinction which we make in using the terms 'word' and 'sentence.' Yet when such great authorities as Hermann Paul and Wilhelm Wundt are consulted, we find this point wholly ignored. Wundt defines the sentence as "the splitting up into its parts of a whole that is present in consciousness[2]," and Paul defines it, in much more cumbersome fashion, as "the linguistic expression, or the symbol, for the fact that several presentations or groups of presentations have become combined in the mind of the speaker, and the means for producing a like combination of the like presentations in the mind of the listener[3]." A simple example will, I hope, finally demonstrate the insufficiency of such pedantic definitions. If I say to a friend 'come for a walk,' it is indeed clear, with Wundt, that the notion which I have in my mind is communicated to my friend piecemeal, but that friend would be very much hurt if he thought that my sole aim in thus speaking to him were to chop up for his benefit a transient phenomenon of my consciousness. Obviously something is left out in Wundt's definition, and what is left out is precisely any explanation why a sentence, as opposed to a word or a mere combination of words, always seems in a certain measure 'satisfactory'—satisfactory, that is to say, inasmuch as it is self-sufficient and complete psychologically and socially, even when, as occurs often enough, it may also provoke resentment or dissatisfaction of one kind or another. The same objection holds good of Paul's definition: when I say 'come for a walk,' there is evidently something more intended than the mere communication from speaker to listener of a notion that has occurred to the former; somehow or other the dynamics of the situation is neglected in this definition, just as in that given by Wundt. The insufficiency of Paul's analysis is drastically revealed when such a question as 'is this Jones' house?' is compared with the statement 'this is Jones' house.' Here the same presentations or groups of presentations are combined in the speaker's mind and also conveyed to the listener, and yet the two sentences have an utterly different import. Could there be clearer evidence that the essence of the 'sentence' must be sought in some other direction? None the less, I have looked in vain for any more convincing analysis. The definitions due to English scholars are simpler, without answering objections of the type already brought; so Stout, "The sentence...is the expression of a unit of thought"; and Sweet, "The sentence is a word, or group of words, capable of expressing a complete thought or meaning[1]." But why do we go about troubling our neighbours with our units of thought? Why do we inflict our 'complete thoughts' or 'meanings' upon them? Ought we not to be content with mere thinking, instead of launching our thoughts upon the air? Again, the often expressed view that a sentence alone can have meaning or convey a thought seems to attribute to the terms 'thought' and 'meaning' too restricted and technical a signification. To me, at least, it appears that if I say 'yonder house,' pointing at the same time, I do express a meaning and, in so far, convey a thought, though that meaning or that thought certainly does not constitute a sentence.

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[1] A paper read before the Philological Society, Dec. 2, 1921.

[2] "Die Zerlegung eines im Bewusstsein vorhandenen Ganzen in seine Teile." W. Wundt, *Völkerpsychologie: Die Sprache*, 2nd ed., ii, 241.

[3] "Der Satz ist der sprachliche Ausdruck, das Symbol dafür, dass sich die Verbindung mehrerer Vorstellungen oder Vorstellungsgruppen in der Seele des Sprechenden vollzogen hat, und das Mittel dazu, die nämliche Verbindung der nämlichen Vorstellungen in der Seele des Hörenden zu erzeugen." H. Paul, *Prinzipien der Sprachgeschichte*, 4th ed., iv, 121.

In a recent volume of the *Indogermanisches Jahrbuch* (VI, 1918), the various definitions of the sentence have been collected and critically examined by a scholar whose name I do not remember. Unfortunately, I have been unable to obtain this article, so I pass at once to my own opinions, which are the result of a strenuous effort to rid the problem of all abstractions, to look at Language in its setting of real life, and to regard sentences as the social facts, familiar from daily intercourse, which they obviously are. There are a thousand sentences spoken for every one that is written, and surely the right method of approach is to investigate speech, not as we find it in books or grammars, but as perhaps the most vital of all social phenomena, as the indispensable instrument of our social existence, where we use sentences at every moment to give or to ask information, to express wishes or give orders, or in some way to exert influence upon the minds of our fellows. Most writers on Language have, of course, been more or less alive to this standpoint, but Marty alone, so far as my reading goes, is entirely impregnated with it. His statement of the purpose of Language[1] agrees closely with my own definition, which runs: *Language is the name given to any system of articulate symbols having reference to the facts of experience, whereby speakers seek to influence the minds of listeners in given directions.*

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[1] These and other definitions are collected conveniently by A. Noreen, *Vårt Språk*, V, 51–61. My friend Dr Grapow points out that Paul Kretschmer has understood the sentence in very much the same manner as myself but has omitted the essential reference to the listener in his definition; see Gercke-Norden, *Einleitung*, I, 516–7.

This definition adds at least one new dimension to Language as usually viewed, namely the relation of speaker to listener. I do not mean to say, of course, that the relation of speaker to listener (with the converse relation) has been completely neglected by philologists; it is constantly cropping up in Paul, in von der Gabelentz, in Jespersen and in many other writers. But I do maintain that it is only very inconsistently kept in view. Still more is the neglect of this relation evident in the treatment accorded to Language in other sciences. Formal Logic is, as ever, the worst offender[2]. Formal Logic lives, if I may be pardoned the expression, in a holy terror of becoming verbal; but as she pretends to abstract from words themselves, while never ceasing to employ them in framing her propositions and syllogisms, what hope is there that she should pay any attention to either speaker or listener? As regards psychology, I would not venture to dogmatize in so disrespectful a manner; but a few recent excursions into psychological literature suggest that here too there is more than an inclination to identify speech completely with thought, and so to ignore, if not the actual audible and articulate character of speech, at any rate the attitude to a listener which speech essentially implies[3].

The four elements always present in normal speech, *i.e.* in speech as used in any social *milieu*, are (i) the speaker, (ii) the listener, (iii) the things spoken of, and (iv) the actual verbal symbols or words. With these elements in mind we shall find it easy to give, not indeed quantitative, but at any rate qualitative definitions of both the word and the sentence. Language itself seems to retain a consciousness of the four distinct elements composing it, for when the verbal symbols have as their object of reference the speaker or the listener, these are not spoken of as 'they' or 'it' like any other thing or persons, but have designations of their own, namely 'I' and 'thou.' The fourth element, the verbal symbols, do not likewise claim a personal pronoun of their own, since their essential rôle is to efface themselves in favour of the meanings which they carry.

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[1] A. Marty, *Untersuchungen zur Grundlegung der allgemeinen Grammatik und Sprachphilosophie*, I, 22.

[2] See Schiller, *Formal Logic*, 1912, for a trenchant condemnation of this discipline.

[3] In the symposium entitled *Is Thinking merely the action of Language Mechanisms?* (this *Journal*, XI, 54 foll.) none of the writers lays stress on the fact that one of the stimuli practically always present when the language-habit comes into play is the proximity of a listener. Would Professor Watson's infant run all over the house crying 'box' (p. 86) unless there were either actually or potentially present some person accustomed to act upon the suggestion thus given?

The principal types of sentences are statements, affirmative and negative, questions, imperatives with other hortatory forms of speech, and exclamations. The exactness or exhaustiveness of this classification does not matter for my purpose. Clearly the difference between these various kinds of sentence resides in the difference in each case of the speaker's attitude towards the listener: at one moment he supplies the listener with information, either by stating a thing or else by contradicting an erroneous impression; at another moment the speaker seeks information or addresses a command, or else makes an ejaculatory appeal for sympathy. These are, of course, by no means all the attitudes to a listener which speech can express; but for jests, taunts, threats, sarcasms and so forth Language has created no special forms. Now, since the various species of sentence, as above enumerated, have been universally recognized, it is little short of marvellous that the character common to them all has been universally overlooked. Clearly that common character must constitute the essence of the 'sentence,' and this essence consists in the presence of some volitional attitude towards a listener on the part of the speaker. Of course a sentence always does more than convey a volitional attitude of speaker towards listener; it always contains a reference to certain things or ideas spoken about. But to contain a reference to things or ideas is the characteristic of 'words'; every sentence consists of one or more words, though not every word is a sentence. My definitions of 'word' and 'sentence' therefore run:

A Word is an articulate sound-symbol in its aspect of denoting something which is spoken about.

A Sentence is an articulate sound-symbol in its aspect of embodying some volitional attitude of the speaker towards the listener.

Let us now put these definitions to the test. I begin with the stock-example of the word 'fire.' If I utter this word quite irrelevantly, at least it has what may be termed dictionary meaning. It may, however, mean more than this; it may actually refer to a specific fire, if such is in my mind when I utter the word. But as yet the word gives no satisfaction, it is not a sentence; it states nothing, asks no question, gives no command, in fact has nothing about it which can suggest why the speaker thrusts it upon the attention of the listener. The listener, if there is one, may remark "what about fire?" This question, when analysed to the bottom, means "What do you mean *me* to understand about fire? How am *I* intended to understand the word fire?" Otherwise expressed, the listener demands some volitional attitude on the part of the speaker which will somehow involve and concern the listener himself. Now the word 'fire' may be spoken in such a tone, and in such surroundings, as leave no doubt as to the speaker's meaning. If he utters the word while rushing from a house with a scared face, it is quite obvious that he desires to communicate the fact that the house is on fire; or if he is an officer addressing the word to his men, arms having already been presented, then it is obvious that his intention is to give a command. In both cases 'fire' is at once a word and a sentence: it is a word inasmuch as it refers to this-house-on-fire or firing-of-those-guns, and it is a sentence inasmuch as a listener or several listeners are involved in the act of communication or command. Such one-word sentences are now admitted by all modern grammarians, and it is not to be supposed that any objection will be raised on this score[1].

A conceivable objection might, however, be found in the utterance of the word 'you,' which, when it occurs out of context, is certainly felt as a mere word and not as a sentence. And yet there is here, always present, an attitude of the speaker towards a listener. But my definition said 'volitional attitude' and not simply 'attitude.' *You*, *your*, *yours* and sometimes *our*, *ours* contain a reference to the listener as whole or part of what these words speak of; just for this reason they are words, but they are not sentences until they imply in addition a volitional attitude on the part of the speaker. If the question is put 'whom will Tom send?' and the answer is 'you,' then 'you' is a word by virtue of the fact that it refers to a thing spoken about, in this case the listener, but it is now also a sentence because the articulation conveys the information to the listener that it is he whom Tom will send. The same argumentation will apply to 'yours' as answer to the question 'whose pen is this?'

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[1] See P. Wegener, *Der Wortsatz* in *Indogermanische Forschungen*, 39 (1920), 1–26. In no book that I have consulted is there shown a better understanding of the true nature of Language than in the same author's *Untersuchungen über einige Grundfragen des Sprachlebens*, Halle, 1895.

Such exclamations as 'alas,' 'hurrah' or 'damn' may perhaps be quoted as objections. We feel them as self-sufficient and accordingly, it must be supposed, as sentences; sometimes, too, it will be admitted, they make a deliberate appeal to the sympathy of a listener, and so come well within my definition. For my own part, I am prepared to concede, however, that such exclamations might possibly come to the lips even in solitude. Now in such a case, I think, it is for the psychologist to decide whether there is any dual function on the part of the speaker causing him to act as listener as well as speaker. In articulate remarks addressed to oneself, such as sometimes undoubtedly occur, a speaker actually does seem to be acting a double rôle; if a man says to himself "You must do such and such a thing," this is an autosuggestion modelled on the form usually assumed by the suggestions of others; the coercive, imperative tone is borrowed from Language as a social phenomenon, and we are here dealing with a form of Language which is clearly secondary. The same kind of answer may be made if it is objected that the lyrical poet writes only for himself, though undeniably he writes in sentences. Sir Walter Raleigh in his essay on Style comments as follows: "Yet it were rash to say that the poets need no audience; the loneliest have promised themselves a tardy recognition. The poet seems to be soliloquizing because he is addressing himself, with the most entire confidence, to the small company of his friends, who may even, in unhappy seasons, prove to be the creatures of his imagination." Hardly less than poetry, many books of science and philosophy might at first sight seem to lay claim to emancipation from an audience; their sentences seem to state what is, and writer and audience are, to say the least, in eclipse. But when we look closer, the attitude towards a circle of readers is seldom far from the surface; no book is written without some desire to instruct, to convince, or to place on record for oneself or for others.

How far sentences, and not merely disjointed words, occur in silent thought is a question of deep interest, to which it is to be hoped that psychology will give us a detailed answer. So far as my own limited and unpractised introspection goes, though I am very conscious of verbal elements in ordinary thinking, yet it is only when I have some writing in view or when I wish to fix some conclusion in my memory that I am aware of framing complete unspoken sentences. If we were in the habit of thinking in finished sentences, surely the difficulty which is often found in formulating a thought would not be experienced at all. In the cases to which I have referred, it may, moreover, be contended that there is a latent audience. But—and this is the important point—even if such is not the fact (the answer rests with psychological experiment) my definition of the sentence will not be invalidated. For of course it is possible for the sentence, as for almost anything, to be transferred to a usage for which it was not originally intended and where it ceases to be what it was designed to be except in the historical, evolutionary sense. Such a transferred use of the sentence is seen in what we are wont to call subordinate sentences, which, as every grammarian knows, are merely sentence-like equivalents of words, whether nouns, adjectives or adverbs. It is established that many subordinate sentences did actually originate in real sentences, real assertions, as for example noun-clauses of the type 'I see that he is content' from 'I see that: he is content.' How little of the true nature of a sentence there is in what we call subordinate sentences is well exhibited in such a case as 'that he has gone proves that he does not care,' where if we lop away the two so-called subordinate sentences there remains for the main sentence only the word 'proves,' which in its isolation no rational person will admit to be a sentence at all. Jespersen and others have pointed out the need for a radical reform in our terminology here[1].

On my definition the vocative will have to be a word-sentence, since it both names the listener and also invites his attention. Otherwise no alteration whatsoever will have to be made, so far as I can see, in our ordinary classification.

There is, however, just one objection to which I can give no quite satisfactory answer, though I fancy it is not of much importance. Occasionally a mere word may indicate an attitude of respect or disrespect towards the listener, and if the choice of that word be intended to please or offend, it is difficult to avoid the conclusion that on my definition the word is then a sentence. Thus when a Frenchman, in addressing a close kinsman or intimate friend, suddenly replaces the pronoun *tu* with *vous*, this *vous* may often be practically equivalent to the sentence 'I am very angry with you.' Such overtones of meaning are perhaps the most powerful weapon that speech possesses. Meredith illustrates this point with consummate skill in a passage of *Rhoda*

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[1] Jespersen, *Sprogets Logik*, 38–9. So, too, Paul, *op. cit.*, 296–7. *The Report of the Joint Committee on Grammatical Terminology*, p. 14, likewise recognizes the difficulty, for it recommends that in cases where a complex sentence "contains a subject and predicate of its own it be called the *Main Clause*, but that where it does not, it be called the *Main Predicate* or the *Main Clause*, as the case may require."

Fleming[1], where the literal meaning of the spoken words sinks into insignificance beside the emotional values (added in brackets by the author) with which they are pregnant.

> "Then he perceived in dimmest fashion that possibly a chance had come to ripeness, withered, and fallen, within the last scoffing seconds of time. Enraged at his blindness, and careful, lest he had wrongly guessed, not to expose his regret (the man was a lover), he remarked, both truthfully and hypocritically, 'I've always thought you were born to be a lady.' (You had that ambition, young madam.)
>
> She answered: 'That's what I don't understand.' (Your saying it, O my friend!)
>
> 'You will soon take to your new duties.' (You have small objection to them even now.)
>
> 'Yes, or my life won't be worth much.' (Know, that you are driving me to it.)
>
> 'And I wish you happiness, Rhoda.' (You are madly imperilling the prospect thereof.)
>
> To each of them the second meaning stood shadowy behind the utterances. And further,—
>
> 'Thank you, Robert.' (I shall have to thank you for the issue.)
>
> 'Now it's time to part.' (Do you not see that there's a danger for me in remaining?)
>
> 'Good-night.' (Behold, I am submissive.)
>
> 'Good-night, Rhoda.' (You were the first to give the signal of parting.)
>
> 'Good-night.' (I am simply submissive.)
>
> 'Why not my name? Are you hurt with me?'
>
> Rhoda choked. The indirectness of speech had been a shelter to her, permitting her to hint at more than she dared clothe in words.
>
> Again the delicious rose glowed between his eyes.
>
> But he had put his hand out to her, and she had not taken it.
>
> 'What have I done to offend you? I really don't know, Rhoda.'
>
> 'Nothing.' The flower had closed."

The discussion of these obscurer purposes of Language would lead us too far afield, and my object in the present paper is not to consider any new meanings to which the terms 'word' or 'sentence' might be extended, but rather to analyse the little comprehended but universally felt senses in which they are familiar to us. Let me state my definitions once again:

A Word is an articulate sound-symbol in its aspect of denoting something which is spoken about.

A Sentence is an articulate sound-symbol in its aspect of embodying some volitional attitude of the speaker towards the listener.

It is evident from these definitions that the terms 'word' and 'sentence,' abstractly considered, are quite heterogeneous. The common notion that a word is necessarily part of a sentence, and that this is all the difference which exists between the two, breaks down in the case of one-word sentences, where the word and the sentence are concretely coextensive. Doubtless the reluctance which students formerly felt in allowing the existence of one-word sentences was due to the mistaken idea that if it be admitted that a word can be at the same time a sentence, the very useful distinction between the two disappears. Such, however, as I have tried to point out, is not the case; the terms 'word' and 'sentence' refer to totally different aspects of concrete linguistic phenomena, and surely it is permitted to call a rat a rodent from one point of view, and a nuisance from another. Still, the fact remains to be explained that in concrete reality no sentence exists which is not at the same time either a word or else composed of words. The reason is that we deliberately restrict the term 'sentence' to purely linguistic phenomena, and that we rule out from the field of linguistic phenomena everything that is not an articulate sound-symbol having reference to a thing, *i.e.* a word. Otherwise we might easily extend the meaning of the term 'sentence' to many other social gestures, to an imploring look or to a shake of the fist. If we did this, the sentence would cease to be a purely linguistic term, and its essential heterogeneity as contrasted with the word would become quite apparent.

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[1] London: Constable & Co., 1902, 379–80.

One last thought is suggested by the foregoing discussion. At what angle is the critic of Language to place himself? Is the meaning of a sentence that which is in the mind of the speaker at the moment of utterance, or that which is in the mind of the listener at the moment of audition? Neither, I think. Certainly not that which is in the mind of the listener, for he may utterly misconstrue the speaker's purpose. But also not that which is in the mind of the speaker, for he may intentionally veil in his utterance the thoughts which are in his brain, and this of course he could not do if the meaning of the utterance were precisely that which he held in his brain. Obviously we must postulate for the critic a position which ignores neither the speaker nor the listener, nor yet again the words that pass between them. I think that the following formulation will meet the case: *The meaning of any sentence is what the speaker intends to be understood from it by the listener.*

Postscript.

The article in *Indogermanisches Jahrbuch* VI (1918), 1–20, referred to above (p. 353) has now come to hand, and proves to be a well-written and thoughtful discussion, by Karl Bühler, of prevailing opinions on the sentence. The writer starts from the view that speech has three functions, namely *Kundgabe, Auslösung* and *Darstellung*, *i.e.* approximately 'self-expression,' 'demand for response' and 'description.' It is shown that none of these functions alone yields a satisfactory definition of the sentence, though this must necessarily be defined, not in reference to its mode of origination, but in reference to its purpose. Bühler therefore finds the essence of the sentence to lie in its possession of what is common to the three functions of speech, namely *Sprachzweck* ('purpose') or *Sinn* ('sense'). The former term, when analysed further, leads, as I believe, directly to my 'volitional attitude of speaker to listener,' though Bühler does not see this, failing to recognize the absolute indispensability of the listener to language-theory[1]. The use of the term *Sinn* is decidedly a step in the wrong direction, being even less clear than *Sprachzweck,* and leading to a hair-splitting distinction between *Sinn* 'sense' as the characteristic of the sentence, and *Bedeutung* 'meaning' as that of the word (p. 18). Bühler does not realize that each of his 'functions' enters into *every* genuine linguistic utterance, *i.e.* employment of speech as a social instrument. Every sentence is *Kundgabe* (roughly 'interjectional') inasmuch as it expresses what the speaker has to say, *Auslösung* (roughly 'imperative') inasmuch as it claims, at the very least, the attention of the listener, and *Darstellung* (roughly 'assertive') inasmuch as something or other is always spoken about. Each of these three functions may come into the foreground as the specific purpose of a given utterance, and from this variation arise the different types of sentence. In conclusion, I venture to recommend Bühler's paper as a stimulating and valuable contribution to the topic; I think he has failed to solve the problem, but he has seen many of the difficulties and pointed in the right direction.

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[1] On p. 13 Bühler falls into the common error of regarding the use of language in solitary meditation as a proper use of language as such. I must insist once again on my contention that, however important this use of language may be and is, nevertheless it is secondary and a by-product—as much so, as when a book is used as a paper-weight. No progress can be made with the theory of language if the latter be removed from its proper sphere, namely social intercourse, where for its successful functioning a sympathetic interaction of speaker and listener is demanded, besides the existence of a world and of a tongue common to both.

A VINDICATION OF THE RESONANCE HYPOTHESIS OF AUDITION. III[1].

By H. HARTRIDGE.

(From the Cambridge Physiological Laboratory.)

1. *The lengths, masses and tensions of the fibres of the organ of Corti* (pp. 364–367).
2. *Physical requirements of the resonance theory* (pp. 367, 368).
3. *Physical requirements if the auditory nerve obeys the all-or-none law* (pp. 368–371).
4. *The number of basilar fibres is sufficient to account for experimental results* (p. 371).
5. *The inter-connexion of fibres of basilar membrane offers no difficulty* (p. 372).
6. *Such inter-connexion must add to the efficiency of the ear* (pp. 372, 373).
7. *The perception of tones of different degrees of loudness is readily explained* (pp. 373, 374).
8. *The neurology of audition* (pp. 374, 375).
9. *Developmental evidence* (p. 375).
10. *Tones not recognised by number of nervous stimuli per second reaching the brain* (pp. 375, 376).
11. *Ewald's theory is not unique in having been tested by means of a model* (p. 376).
12. *The existence of a constant tension in the fibres of the basilar membrane is not contrary to biological laws* (pp. 376, 377).
13. *The advantages of filling the cochlea with liquid* (pp. 377–379).
14. *Pitch limits* (pp. 379, 380).
15. *Summary* (pp. 381, 382).

In this paper I propose to deal (*a*) with the essential part of the Helmholtz theory which concerns the resonators and their properties, and (*b*) with certain criticisms which have been advanced against this part of the theory. It is now universally agreed that the sound wave motion of the air is in greater part transmitted to the cochlea *viâ* the chain of ossicles in the middle ear. It is also agreed that the hair-cells in the cochlea are the sensory end organs alone concerned with the origination of auditory stimuli received by the brain. Differences of opinion still exist as to the way in which motion of the piston of the stapes is communicated to these hair-cells. However, the debatable points have been reduced considerably of recent years, and I believe that the weight of opinion may be classified as follows:

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[1] The two previous papers on audition have appeared in this *Journal*, 1921, xi, 277–288; xii, 142–146.

(1) The belief that the basilar membrane moves as a whole. Rutherford, Wrightson and Keith.

(2) The belief that the basilar membrane is set in motion selectively, according to the agreement between the period of the incoming sound waves, and the natural rate of vibration of the different basilar fibres. Helmholtz, M'Kendrick and Gray.

(3) The belief that the tectorial membrane contains the resonating structures and not the basilar membrane. Hardesty and Luciani.

(4) The belief that the basilar membrane is selectively set in motion but not by resonance. M. Meyer and Ewald.

It will be observed that opinions 1, 2 and 4 have this in common, that they hold that the basilar membrane is the structure responsible for the stimulation of the hair-cells. In my opinion the evidence for this is so weighty that any alternative hypothesis needs no further consideration, *i.e.* I rule out belief 3 above as impossible. In this connexion the reader should turn to Keith's description, pp. 176–186 of Wrightson's book[1].

In some details Helmholtz's ideas were almost certainly defective. For example, he believed that the tips of the hair-cells are thrust against the under side of the tectorial membrane, each time the basilar membrane rises; but it is much more likely that, as Keith states, the tips of the hairs are implanted into the under side of the tectorial membrane, and Keith holds that they undergo bending twice during each complete cycle.

While agreeing with Keith that the anatomical evidence is in favour of the tips of the hairs being implanted in the under side of the tectorius, yet it seems to me that there is another manner in which the stimulus to the hairs might occur. I believe with Keith that the one fulcrum of the basilar membrane is close to the external spiral groove, but it seems to me that the other fulcrum is situated not where the internal spiral ligament begins (as Keith and Wrightson suppose), nor where it is attached to the bony spiral lamina, but near some point between the two.

The tectorius will, if this is the case, not lie over one edge of the vibrating structures but over approximately their centre. The drawing by Fraser and Dickie (Fig. 23, p. 235 of Wrightson's book) shows this feature not only for fibres of medium length, but also for those at the top and the bottom of the cochlea. If then the tectorius is attached nearly at the centre of the vibrating mass, it will be nearly at that part which undergoes the greatest movement during vibration. When therefore the basilar membrane moves upwards the tectorius would be urged upwards too, but when the basilar membrane reaches the end of its motion and returns, the tectorius, possessing mass and therefore momentum, would tend to go on rising and would therefore pull on the processes of the hair-cells with the force needed to give to the tectorius that acceleration which will cause it to follow the down motion of the basilar membrane. According to this view the stimulus to the hair-cells is not by pressure (Helmholtz), nor by friction (Wrightson), nor by bending (Keith), but by traction (Hartridge). One advantage of this view is that (*a*) it explains why the stalk of the tectorius and the internal spiral groove widen as the basilar membrane widens, *i.e.* in order that the tectorius shall lie over the centre of the moving basilar membrane; (*b*) it explains why the tectorius being "inconceivably delicate and flexible" (Hardesty, Keith and Wrightson) is yet able by its mass to stimulate the hair-cells (it cannot, I think, be rigid enough to act as a bent lever, as Wrightson's and Keith's views would require); (*c*) it gives a better explanation of the mode of stimulus in the bird and alligator in which the supposed side-ways motion of the tectorius over the hair-cells is not nearly so readily visualisable as in mammals.

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[1] Wrightson, *The analytical mechanism of the internal ear*, 1918.

We have now to consider the evidence for and against the resonance of the basilar membrane.

The evidence in favour is that (*A*) the experiments described in my previous paper[1] show that there are resonators somewhere; these cannot be situated in the external or middle ear, or in the brain, and must, therefore, by exclusion be looked for in the internal ear; (*B*) inasmuch as the structure which receives motion from the piston of the stapes *viâ* the perilymph of the scala vestibuli and communicates that motion to the hair-cells at each cycle, is the basilar membrane, therefore the basilar membrane, with its arches, hair-cells, etc., must form the system of resonators, as Helmholtz concluded[2].

Against this hypothesis the following objections have been advanced.

1.

Keith objects[3] that the audible range of 40 to 40,000 vibrations per second (see section 14) could not be received by resonators varying in length according to him from ·12–·30 mm. and Roughton and Thacker[4] have objected that even if mass and tension as well as length are taken into account, as I have previously suggested[5], the wide range of tones perceived by the ear could not be accounted for, because the low-tone fibres would have to be so lightly stretched that the basilar membrane would sag, whereas on the other hand the high-tone fibres would have to be so tightly stretched that they would break.

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[1] Hartridge, this *Journal*, 1921, XII, 142–146.

[2] Helmholtz, *Sensations of Tone.* Ellis's translation, 1912, 145.

[3] *Op. cit.* 174.

[4] In a private communication to the author.

[5] Hartridge, this *Journal*, 1921, XI, 283.

Assuming that the fibres of the basilar membrane do not differ markedly from silk in their physical properties, I made measurements to see (*a*) what is the breaking strain, and (*b*) what is the least tension that would give vibrations without sag. The first was found to be 1200 grams and the second ·05 gram approximately. The least and greatest lengths for the basilar fibres as given by different observers are:

Observer	Greatest	Least	Ratio
Keith	·300	·120	2·5 to 1
Retzius	·240	·140	1·7 to 1
Henle	·410	·023	17·8 to 1
Hensen*	·370*	·030*	12·5 to 1

* Inferred.

It will be seen that there is a big divergence of opinion, which histologists should endeavour to bridge by further measurements. Taking Keith's values of ·12 and ·30 mm. respectively for the lengths of the highest and lowest resonators and 40,000 and 40 for their number of vibrations per sec., then putting X and Y for the weights in the two cases as found by experiment above, and substituting in the ordinary formula for a stretched string[1], viz.

$$R = \frac{1}{2L}\sqrt{\frac{T}{M}},$$

we find that $X = 1{\cdot}27$ grams per cm., $Y = 8{\cdot}55$ grams per cm., the ratio between the two being 1 to 6·7. Since the mass per unit length varies as the square of the linear dimensions, the latter should be in the ratio of 1 to 2·6, a figure which is well within the range of possibility. If, instead of Keith's values for the lengths of the basilar fibres, Hensen's are employed, then the conditions are much more favourable, for no change in mass per unit length whatever would be required.

A still more convincing answer to this objection is obtained, as Roughton has pointed out, when we make an approximate calculation of the actual tension per sq. cm., which the high-tone and the low-tone fibres would have respectively to possess in order to function efficiently as resonators.

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[1] Poynting and Thomson, *Sound*, 1904, 88.

The usual formula $R = \frac{1}{2L}\sqrt{\frac{T}{M}}$ can be altered to $R = \frac{1}{2L}\sqrt{\frac{T}{D \times A}}$, where D is the density, M is the mass per unit length, and A is the section area because $M = D \times A$.

Now since $D = 1$, approximately, $\frac{T}{A} = 4R^2 \times L^2$. Taking the values of R and L for the shortest resonator, viz. $R = 40{,}000$ and $L = \cdot 12$ mm. (Keith's value), we find that $\frac{T}{A} = 920{,}000$ dynes per sq. cm. But this value is only a negligibly small fraction of the stress which animal fibres have been observed to bear without breaking, as shown in the following table:

Silk[1]	...	26,000,000 grams per cm.
Hair	...	6,600,000 ,, ,,
Tendon	...	2,300,000 ,, ,,

Considering similarly the tension of the longest fibres and taking 40 as the natural period and ·3 mm. as the length, the tension per sq. cm. works out at about 6 dynes, the value being very low as would be expected. This disposes of Perrett's criticism[2] that "the tension of a string $\frac{1}{50}$ of an inch long and of microscopical thickness, having a periodic time of $\frac{1}{33}$ of a second, must, of course, by Brook Taylor's formula (1715), be something enormous!"

But a further question arises. Is such a very low value for the tension sufficient to prevent the fibre from sagging? That the answer can be 'yes' seems at first sight very doubtful, since we find, by a simple calculation from the experiments quoted above, that the minimum tension which will just prevent a horizontal silk fibre from sagging is as high as 1100 grams per sq. cm. This is about 80,000 times greater than the value we have just arrived at for the tension of the low-tone fibres. But it must be remembered that the minimum tension necessary to prevent sagging will obviously vary directly with the effective weight of the fibre. The effective weight will in its turn vary directly with the difference between the density of the fibre and the density of the medium in which it is immersed. Since the density of a basilar fibre presumably differs exceedingly little from the density of the fluid in the cochlea, the effective weight of a basilar fibre *in situ* must be enormously less than the effective weight of a silk fibre in air. Correspondingly, therefore, the minimum tension necessary to prevent sagging must also be enormously less in the former case than in the latter.

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[1] Kaye and Laby, *Physical and Chemical constants*, 28.

[2] Perrett, *Some questions of phonetic theory*, 1919, chap. v, p. 10.

The suggestion that the fibres of the basilar membrane are tightly stretched near the base of the cochlea and *vice versâ* is confirmed by Keith (*op. cit.* p. 237). He states that the lowest bridge with the shortest span is provided with massive lateral supports whereas the top-most with more than twice the span has the weakest.

The conclusion reached is, I think, that with the data at our disposal the Helmholtz hypothesis fully justifies us in thinking that the physical properties of the resonators could give approximately the range of tones met with in the human ear.

2.

Keith[1] has objected that "There are no anatomical structures that can serve as resonators in the cochlea." "The basilar fibres are not free strings, they are clothed on their lower surface by a layer of loose cells and fibres, and their upper surface is covered by a stratum of cells."

When Helmholtz advanced the view that the internal ear contained resonators, he knew that they would have to be different in their properties from the resonators handled in the physical laboratory. The resonators of the laboratory are of two types:

(*A*) Those, like tuning forks and tightly stretched strings, which have very sharp tuning, *i.e.* they do not vibrate unless the air waves setting them in motion have almost precisely the same pitch as themselves. But resonators of this type when once they are set into vibration, continue in motion for a considerable time after the stimulating sound waves have ceased, and only gradually do they die away to rest, *i.e.* their 'persistence' is very great.

(*B*) Those, like the receiver of the telephone, or the sound box of the gramophone, which are designed to respond as equally as possible to all incoming sounds whatever their pitch, and being dead beat come to rest immediately the sound waves cease.

Now the ideal requirements of the ear would be to have resonators sharply tuned like the (*A*) class, and coming to rest at once like the (*B*) class. But these two properties are physically antagonistic, no resonator can possess both properties to a high degree at once. Therefore, as Helmholtz saw, the ear resonators must be a compromise between classes (*A*) and (*B*)—a type of resonator in fact seldom seen in the physical laboratory.

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[1] Wrightson, *loc. cit.* 187.

He investigated the possible properties of various compromise resonators (*V*—*Z*) and found that they could be stated by a table[1] as follows:

TABLE I.

TYPE	TUNING	PERSISTENCE
(*A*) Stretched string	Very sharp	Very great
V	$\frac{1}{8}$ tone	38 vibrations
W	$\frac{1}{4}$,,	19 ,,
X	$\frac{1}{2}$,,	9·5 ,,
Y	1 ,,	4·75 ,,
Z	2 tones	2·37 ,,
(*B*) Telephone	Very small	Very little

As the result of experiments, performed by Helmholtz to determine which type in the above table is to be found in the human ear, he decided that type *X* was the one found for middle scale notes, inclining towards type *W* for high notes and to type *Y* for low notes. Now it is clear that resonators of this type could not consist of stretched cords alone, or they would conform too much to type *A*; similarly, they could not consist of layers of living cells alone, or they would conform too much to type *B*. In order to have the necessary physical properties they must consist both of stretched strings and of layers of cells. We see therefore one reason why it is necessary for the basilar fibres to be clothed above and below by living cells[2].

3.

Roughton has also objected that the number of resonators is insufficient particularly if the cochlea nerve obeys the 'all-or-none' law[3].

Helmholtz concluded as the result of experiment that type *X* resonator is the one found to respond to musical tones of medium pitch. He calculated[4] that if a certain musical note exactly in tune with one such resonator caused it to vibrate with an amplitude of 100, then other notes of the same loudness would cause the following amplitudes (Table II).

Suppose now that there are two resonators in the ear differing from one another by ·10 of a tone, and that a musical note just between the two be sounded, then it is clear that both resonators will respond with a 93 degree amplitude of vibration. If, now, the musical note be made sharper by ·01 of a tone, then the vibrations of the shorter resonator will increase

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[1] Cf. Helmholtz, *op. cit.* 143.

[2] Another reason is probably to nourish and protect the basilar fibres, in much the same manner as Bowman's and Descemet's elastic layers in the cornea are covered by a layer of living cells.

[3] Communicated privately to the author.

[4] Cf. Helmholtz, *op. cit.* 145.

TABLE II.

Difference in pitch between resonator and note	Amplitude of vibration of resonator
·00 of a tone	100 degrees
·01 "	99·5 "
·02 "	98·5 "
·03 "	97 "
·04 "	95·5 "
·05 "	93 "
·06 "	90·5 "
·07 "	87·5 "
·08 "	84 "
·09 "	79 "
·10 "	74 "

in amplitude from 93 to 95·5 and those of the longer decrease from 93 to 90·5. The shorter resonator will now have nearly 5 per cent. greater amplitude of swing than that of the longer, and this difference should readily affect the auditory brain centres *viâ* the cochlea nerve. Suppose now that the musical note be made sharper by one-fifth the above, *i.e.* by ·002 of a tone. Then the two resonators instead of both vibrating with an amplitude of 93 will now have amplitudes of 93·5 and 92·5, *i.e.* there will be a difference of 1 per cent. Now the sense organs of the skin and the retina respond to differences in stimulus strength of about 1 per cent. so that such a difference in the amplitudes of the ear resonators should be readily detected if the cochlea nerve does not obey the 'all-or-none' law, *i.e.* is graded in its response. Now Auerbach stated that the trained ear can perceive a difference of ·25 vibration per second over the greater part of the musical scale, *i.e.* about 2000 distinguishable pitches in the octave between tones 500 and 1000 vibrations per second, *i.e.* ·003 of a tone. Preyer gave about half that value, viz. about 1000 distinguishable pitches per octave, *i.e.* ·006 of a tone. Delezenne gave the following values, at a tone of 120 vibrations per second, a ·03 of a tone, at 500 vibrations ·005 tone, at 1000 vibrations ·0042 tone. Külpe gives at 32 vibrations ·1 tone, at 260 vibrations ·0062 tone, and at 2048 vibrations ·0012 tone. The average value found by the four observers for tones between 260 and 2048 vibrations per second is ·0042 of a tone as the least perceptible difference. Now the difference in pitch taken for purposes of comparison above, viz. ·002 of a tone, is less than half the average value found by experiment, and we have seen that resonators differing in natural frequency by ·1 of a tone should readily perceive this. But as a matter of fact, the resonators in the human ear, as will be shown later, differ by about ·01 of a tone, *i.e.* there are ten times as many resonators as I have assumed above; that is, one-tenth of the number actually present would still suffice to give the requisite acuity. The number of resonators present is therefore amply sufficient to meet the needs of the ear.

Now it has been assumed in the above reasoning that the auditory brain centre receives information not only that a certain resonator is vibrating, but also that the amplitude of that vibration has a certain value, *i.e.* the nervous impulse is graded. The possibility has to be kept in mind, however, that sensory nerves like motor nerves may obey the 'all-or-none' law. There is no evidence at the present time either for or against this view, but Troland[1] has recently considered the 'all-or-none' law from the point of view of vision, so that a similar consideration in the case of hearing may be advisable The maximum number of separate resonators in the human ear is, according to Keith, about 4000 since this is the number of complete arches of Corti (Waldeyer gives the number as 4500). Each arch has about four hair-cells (one inner and three outer) connected to it and each of these cells has its own nerve fibre. Each arch rests on approximately five basilar membrane fibres. Now the musical scale may be said to extend from 64 to 4096[2], *i.e.* over six octaves; if we assume that 600 resonators are devoted to each of these octaves, about 3600 resonators will be accounted for, leaving some 400 resonators to deal with the lower limit from 16 to 64, and the upper from 4096 to 40,000 (a more than sufficient number, for M'Kendrick says 110 would suffice). Since there are six tones in each octave, and each octave is represented by 600 resonators, the resonators must differ from one another on the average by ·01 of a tone. (This value is intended to be the average one throughout the musical scale, and is probably too good a value for the ends, but too poor a value for the middle.) If we assume that each hair-cell and its nerve fibre obeys the 'all-or-none' law, and that as the amplitude of vibration is increased, first one cell and then another begins to receive its adequate stimulus, then any one resonator can respond in five stages: (1) not vibrating; (2) one cell responding; (3) two cells; (4) three cells; (5) all cells. Suppose then that we sound a tone strictly in tune with one of the resonators, a large number of other resonators on either side will also be set into vibration but with diminished amplitude —according to the values of Table II above. If now the tone sounded be made sharper or flatter by ·0025 of a tone, between about 1 and 2 per cent. of these resonators will suffer a change, there being an increase in the number of hair-cells stimulated in the case of some and a decrease in the case of others. This change of 1 to 2 per cent. should be readily detected, and a difference of ·002 of a tone should therefore be appreciated. Now the mean value for the differential threshold for the discrimination of pitch given above was ·0042 of a tone. There are therefore ample resonators in the internal ear to satisfy experimental requirements even if the ear obeys the 'all-or-none' law. Lastly, if we assume that the four hair-cells of any one resonator operate together, and not separately as assumed above, then to perceive a total of 11,064 different pitches (which is M'Kendrick's estimate) there should be 5532 separate resonators and not 4000 as Keith found, or 4500 as found by Waldeyer. To overthrow Helmholtz's theory, however, it would be necessary to count the number of different pitches and the number of resonators in the case of one and the same man, and to demonstrate that, beyond experimental error, the former was more than twice as large as the latter.

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[1] Troland, *Journ. Amer. Opth. Soc.* 1920, 161.

[2] M'Kendrick. Schäfer, *Text-book of Physiology*, II, 1184.

4.

It may be asked[1], why,—if as Helmholtz supposed a considerable number of hair-cells are stimulated even by a single pure tone, but with different intensities (the actual in-tune vibrator stimulating its hair-cells most and others less according to their difference in tuning)—will not relatively few resonators suffice to provide the necessary acuity for difference of pitch that the ear is found to possess?

To put this criticism in a more concrete form, we have seen in the previous section that if we assume that the auditory nerves can convey a graduated impression, then the tone difference acuity of the ear could be provided by resonators differing from one another in their free periods by ·1 of a tone. Actually, however, they differ by one-tenth of this amount, *i.e.* they are ten times more numerous than they apparently need be, why is this?

The following explanation may be given. Although, by careful attention, the ear could work with fewer resonators, yet for a rapid and accurate estimate of pitch a greater number has been found necessary. If the auditory nerve obeys the 'all-or-none' law, then again rather fewer resonators would suffice, but still a greater number would be advisable for the reasons just given. If the spatial acuity of the eye was reduced to one quarter by a reduction in the number of retinal elements, we should still be able to see sufficiently well for practical purposes, but the observations would take longer, would require greater attention and would be more liable to error than they are.

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[1] The actual question was put to me by Dr H. Lamb.

5.

Wrightson[1] has stated that one of the greatest difficulties encountered in the Helmholtz theory of resonance in the fibres of the basilar membrane is that these fibres are not free but connected sideways. If the figures calculated by Helmholtz for the resonators of the cochlea in Table II be again considered, it will be observed that with a difference of ·2 of a tone between the free period of a resonator and that of the tone setting it in vibration, the amplitude will be 40 per cent. of that of the maximum possible. Now since the average difference between vibrators has the value of ·01 of a tone, it follows that 20 vibrators must lie between that with the maximum and that with 40 per cent. amplitude; that is, that between any two resonators there will be a difference of amplitude on the average of about 3 per cent. Even if all the resonators were free this value states the average difference in amplitude between the swing of two neighbouring resonators that the Helmholtz theory would require. The table shows further that the greatest difference between the amplitudes of any two neighbouring resonators is under 6 per cent. Can the different resonators be so tightly bound together that this difference in amplitude is not possible? I do not know of any evidence that such is the case. If any critic of Helmholtz should retort that there is evidence of this, I would refer him to Wrightson's book, pp. 234–8. Here Keith says: "the power of the basilar membrane to resist pressure diminishes as the membrane is followed from base to apex of the cochlea." "The bridges become more yielding, with greater amplitudes of swing as we ascend from base to apex." "From base to apex the cochlea is designed to deal with displacements which vary in amount and rapidity." Can this be the case if the whole of the basilar membrane is so firmly bound together that individual movement to the extent of 6 per cent. is impossible? In the next section an explanation will be found for this joining up of the basilar fibres to form a continuous membrane.

6.

Hensen discovered that certain tones set some hairs of the opossum shrimp into vibration, and other tones other hairs. If a system of resonators can be established so simply, an anatomist may well wonder why so complex a structure as the organ of Corti should have been elaborated for this purpose[2].

[1] Wrightson, *op. cit.* 104.

[2] Keith in Wrightson, *op. cit.* 188.

The answer to this criticism can best be met by considering the case of two tuning forks of the same pitch, one of which is driven mechanically, the other being set into vibration by resonance through the medium of the intervening air. As is well known, the amplitude of the second fork will be only a small fraction of that of the first, even if the length of time during which resonance is allowed to occur is long. But if now the prongs of the forks could be connected to pistons and these pistons be fitted one into each end of a tube containing air, then the amplitude of the driven fork would be more nearly equal to that of the driving one. If further the connecting tube were to be filled with water instead of air, then once more an increase in amplitude would be found to occur. Now, as Keith has pointed out[1], it is the purpose of the ear not only to provide man with the enjoyment of music but also to respond to the slightest disturbance, and so warn the lower animals of danger. To do this, the motions of the piston of the stapes must be conveyed to the hair-cells with the least possible loss. Obviously this could not be effected if the hairs were free as in the opossum shrimp or if the fibres of the basilar membrane were isolated one from the other, but could be effected if all the basilar fibres were joined together so that there was no leak of fluid between them. Such an arrangement does not, as I have shown in the previous section, prevent them resonating separately, but does cause them to utilise all the available energy. This, I am convinced, is the explanation of a feature of the organ of Corti which, more than any other, has strangely proved an apparent stumbling-block to the resonance theory.

7.

The following question has been asked[2], that if we assume that the auditory nerve obeys the 'all-or-none' principle, how can we explain the perception of notes of different loudness? According to the estimate given above, there are approximately 4000 complete resonators in the human cochlea. If this number is distributed over the range of audition it will be found that near the middle of the musical scale each resonator will differ from its neighbour by about ·01 of a tone. Now Helmholtz found by experiment that roughly ten vibrations must be performed at the end of a musical note before any one resonator will be reduced to one-tenth of its amplitude, and that such a resonator will vibrate with one-tenth the full amplitude when a note a semitone sharp or flat is sounded. Since each resonator differs from its neighbour by ·01 of a tone,

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[1] Keith in Wrightson, *op. cit.* 237.

[2] This question was put to the author by Mr Barcroft and Mr Roughton.

50 resonators above and 50 resonators below the in-tune resonator will be vibrating with more than 10 per cent. of the maximum amplitude; and, therefore, if we assume, as above, that a vibration of more than 10 per cent. is required for the auditory nerve to respond, then, since each resonator having four hair-cells can respond in four different intensities, such a note could be produced in approximately 400 different degrees of diminishing loudness. The suggestion that a pure tone can stimulate 100 or more resonators should not cause difficulty any more than that the head of a pin can stimulate 100 touch corpuscles.

So far as increasing loudness is concerned, the limit would seem to be reached only when neighbouring notes begin to overlap in their effects on the intervening resonators, or when dissonant resonators stimulate the auditory nerve. In this connexion the noisiness of very loud musical tones should be referred to. This limit would appear to have been reached in the case just considered, and therefore 400 different degrees of loudness is approximately the limiting number available according to theory. It would seem that this number would more than satisfy the demands of experience. If, on the other hand, the four hair-cells respond together or have a common nerve fibre the number of degrees of loudness would be reduced to 100. But this number even would seem to be sufficient to satisfy the conditions.

8.

Kolmer found that one single fibre of the auditory nerve may, by branching, supply two separate hair-cells. Luciani has stated that this directly refutes Helmholtz's theory. On the other hand, Held found that a single (nerve) fibre may be distributed along a quarter turn of the cochlea including a series of some 300–400 arch segments.

It will be observed that not only do Kolmer and Held contradict one another, but also that both, so far as the ear of man is concerned, are contradicted by the following facts: Snodgrass[1] found that in an adult with normal hearing there were approximately 14,000 fibres in the cochleal division of the auditory nerve. Keith[2] gives the number of fibres as 15,000. Each of these fibres must have had a ganglion cell, and from each cell a single process must run radially outwards towards the hair-cells. Now the hair-cells in man number 15,237 in all[3], so that practically each hair-cell has its own separate nerve, and very few (474, about 3 per cent.) would share a nerve fibre with another cell. This disposes of Held's evidence, for, according to his statement, the 3½ turns of the cochlea would be supplied by not more than 14 nerve fibres in all. The ear that he examined, if it was human, must have been highly abnormal. The possibility that the two hair-cells are fed by one and the same cochlea nerve fibre does not embarrass the resonance theory. For histological evidence shows that there are four hair-cells (one inner and three outer) in structural association with each complete arch of the organ of Corti. Since each arch forms part of the essential resonating element, all four cells belonging to an arch could, for the purposes of the resonance theory, be connected to one and the same nerve fibre. But even if it were subsequently found that in certain cases more than four cells were so connected, or cells belonging to different arches, it would only mean that that part of the ear was less efficient than it might be, in much the same way that the rods in the periphery of the retina being connected in groups with one optic nerve fibre, give a lower visual acuity than the cones of the fovea which have a nerve fibre apiece.

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[1] M'Kendrick in Schäfer's *Text-book of Physiology*, II, 1184.

[2] Keith in Wrightson's book, p. 159.

[3] M'Kendrick. Schäfer, II, 1185.

9.

Luciani[1] objects that "phylogenetically the simultaneous development of the excessively numerous specific differentiations of the fibres of the auditory nerve offers great difficulty." If we were not familiar with any other eye but that of man, should we not possibly make an objection similar to the above in the case of vision? But as we know eyes, varying in development from the simplest to the most complex, the matter causes us no difficulty. Does not the same line of reasoning apply to the ear? The development of the ear can be traced from a primitive apparatus for stabilising purposes, through the depth perceiving mechanism of fish[2], to the sound perceiving mechanism of the frog, the alligator and the bird, to reach its highest development in the bat and ourselves.

10.

Keith[3] states that even on Helmholtz's hypothesis "...the final process of analysis must be done in the brain...because the central exchange has only one key or clue to the particular station or stations from which the messages come: namely the rate at which the stimuli or messages arrive." Keith states on the previous page that "Young had postulated three different kinds of nerves for the retina; von Helmholtz went a considerable degree farther and postulated many thousands of different kinds of nerves for the ear. The most impossible aspect of von Helmholtz's theory comes to light when we carry it beyond the cochlea into the nervous system." In the case of the skin we find numerous sensory end organs with a very high degree of localisation. In the case of the eye also we find that each cone has its own local sign. Is it not possible to suppose that the same thing is true of the cochlea? That just as we learn by experience the relative positions of our fingers and of the cones of our retinas, so similarly we learn the relative positions of the different hair-cells of our cochleae? It is true that we cannot run our fingers up and down our basilar membranes as on the key-board of a piano, and so correlate touch with hearing, but neither can we with our fingers stimulate our retinas. Is it not possible that different combinations of sound waves can educate the ear in the same way that different combinations of light waves teach our eyes? I cannot believe that the central exchange of the brain uses the rate at which the stimuli arrive as the clue in the case of the ear, any more than it does in the case of the eye or the skin.

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[1] Luciani, IV, 236.

[2] Hartridge, *J. of Physiol.* 1920, LIV, 244.

[3] Wrightson, *op. cit.* 160.

11.

Luciani[1] wrote that "Ewald's theory, unlike all other theories (including that of Helmholtz), is based not on purely hypothetical considerations, but on a physical fact which can be experimentally established by a model, which adequately reproduces the fundamental conditions under which the auditory apparatus acts." M'Kendrick[2] did, in point of fact, construct a model of the cochlea on the basis of Helmholtz's theory, and observed the physical facts of sympathetic vibration with it.

12.

Meyer[3] has advanced the objection "that theories which compare the fibres of the basilar membrane to resonators introduce in opposition to the laws of biology the idea of a permanent, constant tension of the basilar membrane. Authors who expect their readers to accept it as the truth should first of all try and convince them of the possibility of living animal tissues retaining their tension for any length of time." The truth of this statement about living tissues is, I think, open to considerable doubt. The heart and skeletal muscles in man are under tension during the greater part of his life. The sphincters of the bladder and anus are firmly closed, and under tension during probably 99 per cent. of the total day. The ligamentum nuchae of the horse (and possibly also of the sheep and ox) supports the weight of the head, not only during waking, but also during sleep. The diaphragm in most animals is in tension during the whole life-time. Lastly, the outer coats of the eye have to stand the intra-ocular tension of *circa* 25 mm. of mercury during life, and do so, moreover, with practically no observable stretch. The conclusion is then, that living tissues DO retain their tension without appreciable weakening for very considerable lengths of time, and, therefore, there is no very obvious reason why the fibres of the basilar membrane should not be able to do so also.

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[1] Luciani, IV, 243.

[2] M'Kendrick in Schäfer's *Text-book of Physiology*, II, 1182.

[3] Meyer, *An introduction to the mechanics of the inner ear*, 1907, 19.

But in this connexion I would direct the reader's attention to Keith's description of the stria vascularis[1] where he asks "is the tension of the basilar membrane regulated by the stria vascularis?" And to this question he replies, "I cannot do otherwise than maintain the opinion" that it does. We know no more at present than Keith has told us of the histology of the mechanism, but it is possible that it will allow the tension to be so far reduced that the basilar fibres can obtain 'rest.' If the tensions of the fibres do in fact change from time to time then the musical tone to which any one resonator responds must also change (a fall in tension of the stria vascularis causing an apparent rise in musical pitch). It may possibly be found that this explains why good singers occasionally sing persistently sharp or flat, and why musical tones fall in pitch during yawning.

13.

"Resonance breaks down if one of the chief factors, viz. the elasticity of the liquid medium between the stapes and the auditory nerves does not at these minute pressures exist."..."It seems difficult to identify resonance which takes time to transmit its energy through air with transmission through a liquid medium which is instantaneous[2]."

Both the above criticisms are due to a misconception of the physical properties of water. In the first place a fluid possesses elasticity whether it be subjected to pressure or not. Thus air conveys sound waves because it possesses mass and elasticity, the fact that the pressures are small (when the sound is a soft one) does not affect the matter. Similarly, the second criticism may, in its turn, be criticised: that in the first place energy transmission through a liquid medium is not instantaneous. Thus sound does not travel through water instantaneously but with a speed of 1435 metres per second, that is only about 4·3 times as fast as through air, and in the second place even if the transmission were instantaneous would resonance be impossible? Surely not! It would only mean that the resonating body would be set into oscillation a little sooner. Wrightson's attempt to prove that liquid is less favourable than air therefore fails, and, as a matter of fact, under certain conditions which are satisfied by the ear, the exact opposite is the case. The physical differences between the two media which concern the problem at hand are as follows: (*a*) compressibilities: with air the volume is halved when the pressure is doubled, with water the volume remains practically unchanged (its compressibility is one twenty-thousandth part that of air; (*b*) with regard to densities: that of water is roughly 777 times that of air.

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[1] Wrightson, *op. cit.* 181.

[2] *Ibid.* 130.

If then the cochlea contained air it would be advantageous to keep the volume small, in order to reduce the compression which would follow change in pressure due to movement of stapes. It would also be advantageous to make the areas of the piston of the stapes and of the basilar membrane as large as possible, in order that a given force being distributed over as large an area as possible, the pressures developed would be kept low and therefore the compression of the air would be kept low. The cochlea would then best take the form of a shallow tube, one end being fitted with the piston of stapes, the basilar membrane stretching down the middle parallel with the piston, the lower end being fitted with the drum of the round window. If, on the other hand, the cochlea contained water, it would again be advantageous to keep the volume small, not in order to reduce compression as in the case of air, but in order to reduce the force required to set the mass of water into motion, because the density (mass per unit volume) of water is so great. The piston of the stapes and the basilar membrane should be given then the smallest dimensions compatible with histological requirements; this would reduce the weight of the parts which have to be moved, and therefore the energy losses due to that cause and will not at the same time involve difficulties due to compression because of its very low values in the case of water. This explains the small dimensions of the organ of Corti in mammals. With regard to the shape of the cochlea, physical considerations show that it confers on the basilar membrane, and therefore on audition almost complete immunity from the effects of changes of position and of both linear and angular accelerations; this immunity is contributed to by the fact that the densities of the organ of Corti and the fluid bathing it are very nearly the same.

The fluid confers two other advantages: (*a*) it makes the volume displacement of the basilar membrane equal to that of the piston of the stapes since the intervening fluid is nearly incompressible and therefore ensures that the basilar membrane shall react to weak sounds; (*b*) it increases the rapidity of damping of the basilar resonators. This latter action may be explained as follows: suppose a set of resonators having been set into oscillation by movements of the stapes piston under the stimulus of sound waves on the drum, and that these sound waves suddenly cease, then if the cochlea was filled with air these resonators could continue in vibration by causing alternate compressions and rarefactions of the air in the scalae. The oscillations would therefore only gradually die out under the natural damping of the vibrators themselves. But if now air be replaced by fluid, no such oscillations could continue, unless either the basilar membrane drives the piston of the stapes in and out, or other parts of the basilar membrane are set into vibration. The former has against it the fact that energy would have to be available for setting the masses of the chain of ossicles and drum into vibration, and the tensions of the stapedius and tensor tympani muscles and friction would have to be overcome, and is therefore unlikely, the latter has against it the fact that these other parts of the basilar membrane have natural periods very different from that of the part which is trying to keep in motion, that is that the vibrations that occurred would be forced ones, which would have a very strong tendency to die out, and would therefore tend to check strongly the 'after' motion of the vibrating resonators. It is possible therefore that the effective damping of the resonators is greater than Helmholtz supposed, owing to this fluid effect. This gives an additional factor to be taken into account when explaining the staccato ending of a musical tone[1].

14.

It will have been observed that the pitch limits assumed as a basis for calculation are those of from 40 to 40,000 vibrations per second (pp. 364, 370).

With regard to the lower limit, although values as low as eight vibrations per second have been given by authorities, it is by no means clear that the prime tone, and not one of its overtones, was stimulating the ear. Helmholtz therefore on the basis of his own experiments, in which the effect of overtones was carefully guarded against, concluded that 40 vibrations per second is a lower limit which is seldom definitely passed.

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[1] Perrett, *Some questions of phonetic theory*, 1919, ch. v, 36. Perrett, *Nature*, Nov. 7, 1918, 185. Hartridge, this *Journal*, 1921, xi, 285.

Helmholtz pointed out further that notes near this lower limit appear to be alike in pitch, and that as the pitch falls so also does their apparent intensity. He pointed out that these facts would fit in with the conclusion that the longest resonator in the cochlea is set into forced vibrations by notes with a pitch considerably lower than that of itself; this action being entirely in agreement with the probable physical properties of these resonators as explained above in section 1. It is not easy to determine the actual natural pitch of the longest resonator, but its value is probably somewhat higher even than 40 vibrations per second.

Concerning the upper audible limit which is variously given as 40,000–50,000 vibrations per second, Myers[1] has shown by experiments, in which the actual pitch was measured physically, that notes reach the audible limit for most young individuals at between 20,000 and 25,000 vibrations per second.

He showed further that the higher values found by other observers were due to reliance on the determination of the pitch of the Galton whistle by means of a formula which he found to be erroneous. He observed also that as the pitches approach the limit they decrease in intensity and appear to become identical in pitch. The shortest resonator would also seem therefore to be set into forced vibrations by notes that have a higher pitch than itself, and the perception of such notes is therefore due to this fact.

It would seem highly probable therefore that while the audible limit lies between 40 and 25,000 vibrations per second, the series of resonators in the cochlea vary by considerably less than this, say from 50 to 15,000, *i.e.* by the ratio of 1 to 300.

At present evidence is wanting as to the exact values for the pitches of the resonators themselves, but if such values as I have just given were ultimately confirmed by experiment, then some readjustment would be required in the calculations that have been given earlier in this paper. As, however, the extremes of pitch, which I have assumed, are 40 and 40,000 (*i.e.* in ratio of 1 to 1000) whereas the more probable values are 50 to 1500 (*i.e.* in the ratio of 1 to 300), the revised values would be still more greatly in favour of the resonance theory.

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[1] *J. of Physiol.* 1902, XXVIII, 417.

15.

SUMMARY.

(1) Further evidence is given in favour of the view that the probable physical properties of the resonators of the organ of Corti can account for the sensation of tones varying from the highest to the lowest.

(2) The fact that the fibres of the basilar membrane are clothed above and below by cells is in keeping with the physical requirements of the resonance hypothesis.

(3) The number of vibrating elements in the organ of Corti is more than sufficient to account for the total number of different pitches found experimentally, even if the 'all-or-none' law is assumed to hold in the case of the cochlear nerve.

(4) An explanation of the more than sufficient number of basilar resonators is given.

(5) The fact that the fibres of the basilar membrane are connected side-ways does not offer difficulty to the resonance hypothesis.

(6) On the contrary, it would greatly increase the efficiency of the resonators if the basilar fibres were connected so as to form a complete membrane.

(7) Even if we assume that the cochlear nerve obeys the 'all-or-none' law, the perception of notes of different degrees of loudness is still readily explicable.

(8) If there was definite histological evidence that two or more hair-cells are fed by one cochlear nerve fibre, it would not invalidate the resonance theory.

(9) The conception of the phylogenetic simultaneous development of the exceedingly numerous specific differentiations of the auditory nerve offers no difficulty to the resonance theory.

(10) That on the resonance hypothesis the final analysis is not done in the brain by the rate at which the stimuli arrive, any more than it is done in this way in the case of touch or vision.

(11) Ewald's theory of hearing is not unique in having been tested by means of a model. The resonance hypothesis was also tested in a similar way by M'Kendrick.

(12) The idea that the existence of a constant tension in the fibres of the basilar membrane is contrary to the laws of biology is clearly erroneous, since many structures (for example in the cornea) are under tension continuously throughout life, without showing signs of giving way.

(13) The resonance theory explains why liquid should be a more suitable medium than air for conveying the sound impulses from the piston of the stapes to the resonators.

(14) Whereas the audible limits of pitch lie between 40 and 25,000 vibrations per second, the free periods of the resonators probably vary by less than this, say from 50 to 15,000 vibrations per second, *i.e.* by the ratio of 1 to 300.

(*Manuscript received* 6 *August* 1921.)

NOTE ON A METHOD OF ESTIMATING THE TRUE STANFORD-BINET INTELLIGENCE QUOTIENTS OF ADULTS.

BY C. A. RICHARDSON.

THE following method of estimating the true Binet (Stanford Revision) Intelligence Quotients (I.Qs.) of adults, or of children approaching 16 years of age, which has occurred to the writer in the course of a general enquiry into methods of estimating the true I.Qs. of adults and adolescents, may perhaps be of interest to those engaged on Binet tests.

The difficulty which occurs in this connexion with the Binet scale lies in the fact that, owing to lack of extensiveness in the upper ranges, I.Qs. of adults, as measured by the scale, are bound to drop below their true values. The greatest possible I.Q. at which an adult can test is about 126.

The method to be described might be called the 'percentile rank (P.R.) method,' for it depends on the fact that the P.R. of a given person, in the order of intelligence, will remain nearly constant. For the method to be used, two things must be known, namely: (1) The distribution of children's Binet I.Qs.; (2) the distribution of the mental ages of adults as measured in the ordinary way by the Binet scale.

The first is fairly well known (cf. Terman, *The Measurement of Intelligence*, pp. 66 and 78), but there are not yet sufficient data to obtain the second accurately, though Terman gives a diagram based on 62 'normal' cases on p. 55 of the book referred to. The writer has, however, found the latter useful as a rough working basis. [N.B. In Terman's diagram on p. 55, 15·11 seems to be a misprint for 16·11.]

To find Binet I.Qs., proceed thus: Having obtained the mental age of any particular adult in the ordinary way, find his P.R. from the distribution table for mental ages of adults. Since, in finding the I.Q., the chronological age is taken for every adult as 16 years, the P.R. obtained will also be the given person's P.R. as regards I.Q. Then from the distribution tables for children's I.Qs., which are true I.Qs. (the distribution remaining the same for different age-groups), it is possible to find directly the approximate value of the given adult's true I.Q., his P.R. having been found as explained above.

In conclusion, two points should be noted, namely: (1) for I.Qs. below about 105, the value obtained in the ordinary way is probably very nearly the true one; and (2) Terman's distribution diagram for adults can only be used up to mental ages of about 18 years 6 months. When more extensive data are available for obtaining this distribution, the percentile rank method should give I.Q. values of considerable accuracy.

Table of Provisional Values obtained by P.R. Method.

Mental age found by usual method	True I.Q.
16 years 6 months	107
16 ,, 9 ,,	108
17 ,, 0 ,,	110
17 ,, 3 ,,	112
17 ,, 6 ,,	115
17 ,, 9 ,,	118
18 ,, 0 ,,	122
18 ,, 3 ,,	126
18 ,, 6 ,,	132

(*Manuscript received* 6 *February* 1922.)

PUBLICATIONS RECENTLY RECEIVED

An Introduction to Psychology. By SUSAN S. BRIERLEY. London: Methuen and Co. 1921. Pp. 152. 6*s*. net.

"This book," Mrs Brierley explains, "has been written to meet the first needs of non-professional students of psychology, and particularly of those who take up the subject in classes organised by the Workers' Educational Association. Its structure is the outcome of several years' discussions with such students." She divides the book into two parts, the first dealing with the scope and method of psychology, the second entering into some of its general problems, the 'wish,' the organism and its environment, instinct and intelligence, the conscious and the unconscious. A useful list of books suggested for further reading is given at the end of the book.

Mrs Brierley's attitude is, as a rule, a fairly temperate one. She is sympathetic to 'behaviourism.' She contends, for instance, that "thought always proceeds in terms either of concrete images of objects and events, of verbal images, of actual movements of the speech apparatus or of all these together. Thought *is not* either images or words, but consists in the use we make of these" (p. 107). Where thought is imageless she believes that it is still accompanied by and dependent on "movements, actual or incipient, of the speech mechanisms" (p. 106). But she insists that "any really adequate psychology must find room both for the facts of behaviour and those of consciousness" (p. 10). She is sympathetic to psychoanalysis, even to the point of accepting without modification Freud's observations on the 'reality' and 'pleasure-pain' principles. But she adopts the term 'horme' as a substitute for 'libido' (in its wider sense), and regards 'dispositions' and 'tendencies' as the "concrete forms," the "springs of action," of the horme. "The horme actually exists in these specific tendencies or 'dispositions' towards specific behaviour" (p. 47). She regards the 'wish' of the psychoanalysts as equivalent to such tendencies or dispositions (p. 52). She speaks of the nutritional horme and the sexual horme.

The special charm of the book lies not merely in its literary style, but also in the clever way in which the beginner is successively led from one to another of the main problems of psychology. There are, however, small blemishes which might well be removed when, as is certain to be the case, a new edition of the book is called for. Simple reaction times do not constitute a good fatigue test (p. 22). Because something exists, it is untrue to say that it can therefore necessarily be measured (p. 23); sensations surely exist, but *quâ* sensations, they are immeasurable. And it is surely unpardonable, in an introduction to psychology, to speak of *sensations* of pleasure (p. 63).

The Psychology of Medicine. By Dr T. W. MITCHELL. London: Methuen and Co. 1921. Pp. viii + 187. 6*s*. net.

Dr Mitchell has written this book "primarily for those readers who have had no professional training in either Medicine or Psychology, but who are anxious to keep themselves abreast of modern thought in these departments of knowledge." The title is not free from ambiguity: the contents are limited to descriptions of hypnosis and hysteria, the theories of dissociation, repression and the unconscious, psychoanalysis, dreams and the neuroses, psychotherapy and the prevention of the neuroses. The book is hence mainly occupied in a presentation of the views of Janet, Freud and Jung. This is very thoughtfully carried out, as would be expected of a writer of Dr Mitchell's experience and repute.

The Boise Survey: a concrete study of the administration of a city school system. By Professor J. B. SEARS assisted by W. M. PROCTOR and J. HAROLD WILLIAMS. World Book Co., Yonkers-on-Hudson, N.Y. 1920. Pp. viii + 290. $2.25.

Boise is a city of about 35,000 inhabitants, the capital of the state of Idaho. Professor Sears, Associate Professor of Education in Leland Stanford Junior University, and his two colleagues were asked in 1919 to survey its public school system. The report deals with the following problems: organization and administration, teaching staff, curriculum, efficiency of instruction, the health, progress and individual differences of school children, buildings and grounds, the high school, educational and vocational guidance, costs and business management. Comparisons are made between about forty other cities in the county and Boise. Tests in spelling, arithmetic and handwriting were given to a large number of children.

State Maintenance for Teachers in Training. By W. S. HERTZOG. Baltimore: Warwick and York. 1921. Pp. 144.

The author surveys the conditions justifying state aid for prospective teachers and he describes the systems which are at work in Europe, our Dominions and elsewhere. He points out that in this respect the United States ranks behind other countries.

The Value of School Supervision. By M. S. PITTMAN. Baltimore: Warwick and York. 1921. Pp. x + 129.

Two groups of schools are here compared, both situated in South Dakota, the one serving as a 'control' group, the other being an 'experimental' group to which the 'zone' plan of supervision was applied. "The special interest of the supervisor is the improvement of certain phases of the school work." The data given go to "show that supervision practically doubled the efficiency of the schools for more than half of the school subjects....School attendance was improved and the children of the advanced grades remained in school longer."

The New Psychology and the Teacher. By Dr H. CRICHTON MILLER. London: Jarrolds. N.D. Pp. 232. 6*s.* net.

The 'new psychology' is of course that of the psychoanalysts, or, as Dr Miller prefers to call them (since he clearly favours the school of Jung), analytical psychologists. There is little that is really new or inaccurate in his presentation of such teaching, but the book makes delightful reading. Its outstanding merit consists in the easy, attractive, really admirable form in which its contents are expressed.

Studies in Human Nature. By Professor J. B. BAILLIE. London: Bell and Sons. 1921. Pp. xii + 296. 15*s.* net.

Professor Baillie shows in these studies that he is dissatisfied with mere logical reasoning as a basis of general and philosophical knowledge. He discusses, mainly from a philosophical standpoint, the place of imagination in knowledge, the function of emotion in the awareness of the reality, the nature of memory knowledge, the sources and significance of laughter and tears, etc., without, however, committing himself to any definite philosophical theory.

Dual Evolution. By J. O'CALLAGHAN. London: Bell and Sons. 1921. Pp. viii + 260. 12*s.* net.

The author endeavours to reconcile idealism and realism from the standpoint of humanism; he admits both a material and a spiritual world.

Reports of the Industrial Fatigue Research Board. No. 12. *Vocational Guidance* (*A Review of the Literature*). By B. MUSCIO. 1921. Pp. 57. 1*s*. net. No. 14. *Time and Motion Study.* By E. FARMER. 1921. Pp. 63. 2*s*. net. No. 15. *Motion Study in Metal Polishing.* By E. FARMER and R. S. BROOKE. 1921. Pp. 65. 2*s*. net. No. 16. *Three Studies in Vocational Selection.* By B. MUSCIO, A. B. B. EYRE and E. FARMER. 1922. Pp. 86. 1*s*. 6*d*. net. London: H.M. Stationery Office.

These are psychologically by far the most important reports yet published by the Industrial Fatigue Research Board. The first is a valuable *catalogue raisonné* of books, etc. published on vocational 'selection' (the title seems a misnomer). Prof. Muscio gives a careful criticism of the results obtained in clerical and engineering occupations, metallurgy, music, printing, salesmanship, telegraphy, telephone exchange work, etc. Next follows Mr Farmer's report on time and motion study, divided into three parts, of which the first reviews the previous work on the subject, the second describes examples of motion study recently carried out by the author on behalf of the Board, and the third gives a bibliography of over 100 relevant books and papers. Mr Farmer rightly lays greater stress on rhythm than on speed of movement, and he objects to the setting up of a standard rate of work based on time study. He argues that time study should aim rather at initiating lines of improvement. In his report on motion study in metal polishing, where he was assisted by Mr Brooke, Mr Farmer gives striking evidence of the increased output effected by training inefficient or inexperienced workers systematically, and of the wasteful expenditure of energy which occurs towards the end of the day, with diminished output, as fatigue sets in; for the latter demonstration he ingeniously employs a recording watt-meter. The last report shows (i) that a high correlation exists between the ranking of hand compositors according to their technical efficiency and their ranking according to certain tests applied to them by Prof. Muscio, (ii) that Martin's muscular strength test which has been greatly praised in America suffers from serious defects, and that a combination of strength of grip and weight (numerous tables derived from over 2000 adolescent males are given) provides a useful indication of general strength, and (iii) that physical type —as determined mainly by finger span and by finger and hand length and breadth measurements—plays some part, though a small one, in proficiency in the work of a sweet factory.

Die Raumwahrnehmung der Blinden. Von Dr W. STEINBERG. München: Reinhardt. 1920. S. viii + 150. M. 3.25.

The work commences with a sketch of the problems raised by the psychology of the blind; this includes a historical review, an account of the chief facts involved in distinguishing two tactile stimulations from one, an analysis of the procedure in the tactile perception of form, a description of the manner in which the blind orient themselves in space, and a consideration of how the loss of vision influences the development of personality. Then follows an account of the author's own experiments with 19 blind men and women. These had to estimate the form of small wooden prisms whose section was bounded by 3–6 straight lines. In one part of the experiments, these prisms were simply grasped by one or both stationary hands; in the other part, some portion of the surface of one or of both hands was used to grope along the prisms.

As a result, the author finds that the grasping with stationary hands produces experiences which are spatial immediately, although still inadequate to the objects, inasmuch as they do not at first present all its features. The groping along the objects, on the other hand, primarily produces nothing but "movement-shapes," which are extended both in space and time. These, then, by a further operation are converted into purely spatial apprehensions of the objects. Solely in the case of the born-blind does there exist any independent tactual space. The inner portion of this consists in the body between the shoulders. The limits of the outer portion are given by arm movements; the arms explore this outer zone in a manner resembling that of searchlights. Beyond this zone, the space is only apprehended in a non-intuitive (*unanschaulich*) manner. Walking to a different place furnishes a measure of the distance, but the apprehension remains one of movement, and does not become converted into purely spatial terms.

In this work, the psychological analysis is conducted along the beautiful lines of the younger Austrian school, and it deserves most careful study by any one interested in the tactual perception of space, either from the theoretical or even from the practical standpoint.

The Glands regulating Personality. By LOUIS BERMAR. New York: Macmillan and Co. Pp. 300. 18*s.* net.

This book contains a valuable account of behaviour from the point of view of glandular changes. It deals with facts which are indispensable to the student of modern psychology. But it is spoilt by obscure ontological and teleological views which have but little bearing on the subject, and the attempt to identify the subconscious mind with the vegetative system is worthless. The main thesis, however, is of great interest, and is presented clearly and not too popularly to lack scientific importance.

Psychoanalysis and Behaviour. By ANDRÉ TRIDON. London: Kegan Paul, Trench, Trubner and Co. 1921. Pp. 354. 10*s.* 6*d.* net.

This is a simple popular account of the main doctrines of psychoanalysis, supplemented by four valuable chapters on the distinctive features in the teaching of four well-known psychoanalysts—Freud, Jung, Adler, and Kempf.

The conclusions stated in the main part of the work are, with one or two exceptions, guarded and moderate. The book is free from the wild generalisations which make so many popular works on this subject scientifically worthless, and publicly dangerous.

The title is slightly misleading; although the author has clearly been influenced by the behaviourist teaching of Kempf, it is not clear to what extent he is in accord with the more extreme views of Watson. The book is not an attempt to link Psychoanalysis on to Behaviourism as taught by the last-named writer.

The arrangement of the book is excellent, it is clearly written, and each chapter is supplied with a useful and intelligently chosen bibliography.

Die Psychische Dingwelt. Von Dr WILHELM HAAS. Bonn: Friedrich Cohen. 1921. S. viii + 216. M. 28.

The exigencies of life have forced man to recognise the objective reality of the physical world, but not of the psychical world. The psychical world, however, is just as real as the physical world, though different from it. According to Dr Haas, the reality of the psychical world is closely parallel to that of the physical. There are independent psychical things just as there are independent physical things; and the 'modes' (*Weisen*) of psychical things are as independent of our apprehension as are the qualities of physical things. There are three such 'modes,' namely, content (*Gehalt*), or that which is apprehended in acts of cognition; *Haltung* (feeling-tone), or the objective counterpart of processes of feeling and willing; and 'Character,' or the objective counterpart of processes of valuation. These three modes are always present in psychical objects, though we do not always apprehend them. The Ego is neither physical nor psychical, but is intimately related to both the physical and the psychical worlds. Some psychical things, like some physical things (our bodies, *e.g.*), are more closely related to the Ego than are others. Corresponding to the physical body there is a psychical body; but just as the Ego is related to other physical objects than its material body, so it is also related to other psychical things than those constituting its psychical body—in patriotism, in the spirit of an age, or of a movement, or of one's environment, we see such other psychical things. The problem of the nature of the Ego and its relation to the psychical and the physical, and the problem of the relation of the psychical and the physical to one another, are to be dealt with in another volume.

Vom Wesen des Bewusstseins. Von Dr LUDWIG KLAGES. Leipzig: Johann Ambrosius Barth. 1921. S. vi + 94. M. 12.

This is an expansion of a lecture on the biological bases of consciousness. The author only sketches the outlines of a comprehensive Psychology and Philosophy which he hopes to publish in the near future. Some of his ideas Dr Klages has already published in the *Deutsche Psychologie* under the title of "Geist und Seele."

An Analysis of Certain Theories of Truth. By GEORGE BOAS. (University of California Publications in Philosophy, Vol. II, No. 6, pp. 187–290.) Berkeley: University of California Press. 1921. $1.25.

The theories examined are: Subjective Theories (including (*a*) Logical Hedonism, and (*b*) Truth as the Irresistible), Relational Theories (including (*a*) the Correspondence Theory, (*b*) Truth as Formal Consistency, (*c*) Coherence in a Significant Whole) and Voluntaristic Theories of Truth.

NOTES ON RECENT PERIODICALS.

Psychologische Forschung, Band 1, Heft 1 und 2. August 1921.

Zur Psychologie des Schimpansen (Wolfgang Köhler).

A series of mainly qualitative observations on the chimpanzee made by the author at the *Anthropoidenstation* at Teneriffe. Only two illustrations of these can be here given. The chimpanzee Sultan was confined alone in a lattice-work enclosure, while the experimenter buried a pear deeply in the sand at 140 cms.' distance from the enclosure, covering it up and disturbing the sand in its neighbourhood so that the exact spot of burial was quite lost. Sultan at first showed momentary disappointment while this was done and then turned to play within the enclosure. Six minutes later the experimenter approached Sultan who grasped his hand but was repulsed. When after nine minutes the experimenter again approached, the chimpanzee repeated this behaviour with the evident object of directing the experimenter to a stick which lay to one side of the burial ground and out of reach of his enclosure. The experimenter did not satisfy him, and later (17 minutes after burial) Sultan repeated his behaviour. Half an hour after the burial, while the animal was otherwise occupied, the experimenter laid the stick where it could be grasped only with difficulty from the enclosure. At first Sultan did not perceive this change; but immediately after doing so, he seized the stick and began digging at the right spot until he had unearthed the pear. On the following day, a pear was buried 130 cms. from the lattice work, about two metres to one side of the previous burial place, while Sultan remained quiet, playing contentedly in his enclosure. After an hour, the attendant introduced a stick into the cage, through a window in the back wall. Sultan at once seized it and after an initial error of position, dug up the pear. In another series of experiments, two chimpanzees who had learnt by practice to distinguish between a large and a small rectangle by choosing the former, distinguished between the large and a still larger rectangle by generally choosing the latter; another chimpanzee who had learnt to choose the smaller of a pair of rectangles still chose the smaller (after initial hesitation and choice in the absolute sense) when the larger rectangle was replaced by one smaller than the other one.

Untersuchungen zur Lehre von der Gestalt (Max Wertheimer).

This, the first of a series of special papers on the theory of form, is of an introductory and general character. Parts are not to be considered as being compounded to make up a whole. The form of the whole is not determined merely by its parts. Parts are generally to be treated as members of the whole, the simpler being derived from the more complex, not *vice versâ*.

Tod und Leben bei den Kpelle in Liberia (Dietrich Westermann).

A chapter of Liberian ethnology, describing primitive conceptions of the mind-body relation after death.

Beiträge zur Psychologie der Gestalt (V) (P. Cermak und K. Koffka).

This is the fifth of a series of articles of which the first four have already been published by Prof. Koffka in book form after their appearance in the *Zeitschrift für Psychologie*. It deals with the visual sensations of movement in interrupted and continuous illumination, and sets out to determine the wider scope of the laws originally deduced by Korte for the relation of the temporal, spatial and intensity characters of the stimuli to apparent movements, *i.e.* to those phenomena of movement which are produced by simple successive stationary stimuli.

Über optische Inversion (Erich M. v. Hornbostel).

This deals with illusions of reversible perspective, introducing several novel instances of the phenomenon. It is curiously written in a form directed to the reader who is throughout addressed as *du*.

Eine Pseudofovea bei Hemianopikern (Wilhelm Fuchs).

This paper opens with a brief summary of the writer's previous work on the subject. It contains detailed observations on a single patient whose condition of right-sided homonymous hemianopia was due to a shrapnel wound of the skull. The writer shows how the pseudofovea or new region of distinctest vision develops within the remaining field of vision, and how its position varies with the recognised size of the object, not with the actual distance of one and the same object from the patient. The larger the size of the object, the more peripheral becomes the pseudofovea. When a letter, previously falling on the pseudofovea, is displaced so that it falls more and more outwardly on the retina, it becomes increasingly blurred and indistinct. If displaced inwardly, it may merely lose its form and meaning. This occurs not merely because its size is unfitted for that particular region of the fovea, but also because its form or meaning is unfitted. A number of separate short lines covering an appropriate extent can be recognised as a distinct and significant whole, whereas when only a single short line falls on the same retinal area it is blurred and indistinct; and when only some of a number of short lines can be apprehended as a form, only these are distinct while the remainder are blurred and of lighter colour.

The Psychological Review (Vol. XXVIII).

No. 1. *January* 1921.

The Truly Psychological Behaviourism (Mary Whiton Calkins).

Extreme behaviourism, denying or ignoring consciousness, is not psychological at all. Modified behaviourism, treating consciousness as a distinctive function of the organism, too readily falls into the extreme form. The truly psychological behaviourism is a self-psychology which emphasizes the study of the relation of self to environment.

An Attempt towards a Naturalistic Description of the Emotions (J. R. Kantor).

An elaborate paper mainly intended to show that emotions are forms of behaviour which mark the absence of any definite response system. They are chaotic responses to unusual features of the environment.

The Affective Tone of Lines (Helge Lundholm).

An experimental study. There is a feeling-tone connected with "pure lines." This is probably dependent upon the suggestion of movement. Emotions with little motor expression are suggested by lines with long and low waves; emotions with strong motor expression by lines with small waves of acute angle.

The Law of Habituation (Stephen C. Pepper).

Elements of affective sequences usually undergo two phases of development. In the first elements originally disliked are gradually rendered enjoyable; in the second "the affective sequence acquires a new movement, a pendulum swing from extreme to extreme of the sequence through a point of intense liking."

The Effect of Caffein and Acetanilid on Simple Reaction-Time (Walter Schilling).

Both retard reaction-time, caffein to a greater degree than acetanilid. They also tend to increase unsteadiness of reaction.

No. 2. *March* 1921.

Cerebral-Mental Relations (Shepherd Ivory Franz).

There is a general dependence of mental states upon the state of the brain, but no special dependence of a special mental state upon the integrity of special cerebral parts.

The Misuse of Instinct in the Social Sciences (L. L. Bernard).

Social "instincts" are aggregates of habits, and in no sense fixed and simple responses. It is not true that for social purposes instinct is dominant over habit, and both over the influence of the environment.

An Attempt towards a Naturalistic Description of the Emotions (II) (J. R. Kantor).

Emotions are of no particular biological utility, cannot readily be classified, and "are seldom if ever found in animals and young children."

On the Organisation of Intellect (Edward L. Thorndike).

An application of Spearman's 'test' of the truth of his 'General Factor' theory to data obtained by applying fifteen tests of intelligence to about 800 soldiers, and seven tests of intelligence and of 'intelligence mixed with skill' to over 900 soldiers. The enquiry suggests that the organization of intellect involves processes much more complex than those required by Spearman's theory.

No. 3. *May* 1921.

In Memory of Wilhelm Wundt (by his American students).

A series of short appreciations by various American psychologists who studied under Wundt.

The Structure of Animal Learning (J. A. Melrose).

In a very long paper it is maintained that lower animal learning involves (1) organic fixation, (2) organic spatial adaptation, (3) organic choice, and higher animal learning comprises (1) organic association, (2) organic conception, (3) organic judgment.

A poet's portrayal of Emotion (Frances Theresa Russell).

A study of certain of Browning's descriptions of emotions.

No. 4. *July* 1921.

Dynamic Psychology and the Problem of Motivation (Horace Bidwell English).

Neither hedonism nor McDougall's instinct theory is broad enough to explain human motives. We must include all relatively permanent psycho-physical dispositions among the mechanisms capable of furnishing the impetus to action.

Psychology and the Central Nervous System (Howard C. Warren).

Psychology deals with all phenomena "concerned in the interaction between environment and organism that belong to the stimulus-response type." These phenomena may be studied by self-observation, by an analysis of behaviour, and by neurological methods. No one of these methods is alone sufficient.

Apperceptive Abilities (Augusta F. Bronner).

Apperception is a distinct mental process, and there is a specialised apperceptive ability, which may be investigated by means of the pictorial completion test.

The Motivation of Radicalism (A. B. Wolfe).

Radicalism springs from a balking of interests and desires by an existing (social) state of affairs. Original desires for innovation are a spring of radicalism only when they are opposed.

The Control of Another Person by Obscure Signs (G. M. Stratton).

A very interesting study of a case of 'muscle reading' without contact, from which it appears that the subject was, without conscious intention, using visual cues of an exceedingly obscure nature, given with like absence of intention by his guides.

No. 5. *September* 1921.

Belief as a Derived Emotion (W. McDougall).

Belief is essentially the same as confidence, but whereas the latter indicates a striving for some particular end untroubled by any anticipation of failure, the whole process taking place on a practical level, in the case of belief "the striving proceeds in a similar untroubled fashion on the purely intellectual plane." Belief and doubt are related as are confidence and anxiety.

How do we acquire our Basic Reactions (J. R. Kantor).

All psychological processes involve "the two phases of stimulus and response." Tendencies, capabilities and the like, in so far as they are supposed to represent forces inherent in the organism, are mere fictions. An attempt is made to indicate the main classes of conditioning stimuli, to show how stimuli operate, and to discuss certain general types of reaction systems.

Fact and Inference in Raymond Wheeler's Doctrine of Will and Self Activity (Mary Whiton Calkins).

As a result of his experiments on the process of choosing Wheeler concluded: (1) that volition consists in kinaesthetic sensations, and (2) that volition cannot be described as self-activity. The present paper undertakes a study of the introspections of Wheeler's observers, and as a result concludes that his interpretations are unjustifiable.

The Correlation between Interests and Abilities in College Courses (Edward L. Thorndike).

The data examined seem to corroborate the doctrine of a very close relation between the order of an individual's interests and the order of his abilities.

A Device for Determining Coefficients of Partial Correlation (Clark L. Hull).

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY

GENERAL MEETINGS.

January 28, 1922.	Experiments on Memory in Mentally Defective Children. By Lucy G. Fildes.
	Psychology and Primitive Culture. By F. C. Bartlett.
March 11, 1922.	An Investigation into the Time-sense of Children. By M. Sturt and E. C. Oakden.

SECTIONAL MEETINGS.

Education Section.

December 13, 1921.	The Theories of Freud and Jung in Relation to the Problems of Education. By Constance E. Young.
January 2, 1922.	Mental Tests and Mentality. By T. H. Pear.
February 14, 1922.	Some Experiments on Form-Orientation with Especial Reference to Mirror Writing. By Lucy G. Fildes.

Industrial Section.

December 21, 1921.	Motor Capacity. By B. Muscio.

Medical Section.

December 14, 1921.	The Evolution of the War Neuroses. By Gerald Fitzgerald and James Young.
January 25, 1922.	Homosexuality and Alcoholism. By Robert M. Riggall.
February 22, 1922.	The Constituents of the Unconscious. By Leonard Williams.

JOINT MEETING.

General, Medical, and Educational Sections.

March 14, 1922.	Some Problems of Adolescence. By Ernest Jones.

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